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NOTES AND COMMENTS.

Progress.

WITH this Number, Natural Science enters on its fourth volume. Two years of experiment, and of those difficulties incidental to all new publications, are now happily ended. Without deviating from the path originally marked out for it, this venture has secured for itself a basis of which the firmness may be measured by its wide extension. For this result, far more fortunate than they anticipated, the promoters are indebted less to their own labours than to the kindly aid so constantly offered them by an ever-swelling stream of contributors, and to the warm welcome extended by an ever-widening circle of readers. To all of these, whether at home in England or far off in her scattered colonies, whether fellow-workers of our own race in America of workers joined with us in the pursuit of a common aim in all foreign lands from Germany to Japan—to one and all this Number will bear our cordial thanks for the past, and will express that gratitude which is a lively sense of favours to come.

To those of our readers and helpers who have not been with us from the beginning, and who have found difficulty in obtaining our earlier numbers, we should like to take this opportunity of restating our aims. We have been from the beginning, and we still hope to be, as our sub-title indicates, a review of Scientific Progress. Month by month we bring to the specialist a critical account of what is being done in other lines than his own; while, for the general reader, the specialist himself is constantly invited to render more accessible his stores of knowledge. As examples of such work, we may refer to the articles in this number on Cell-Division and on the Fucaceæ. We endeavour also to search out items of interest that are hidden away in unlikely volumes or in publications not easily obtained. Chiefly, in this respect, do we attempt to give the news of our own local scientific societies and of similar bodies scattered throughout the British Dominions. And here it is that we need most of that friendly

aid of which we have already spoken. It is obviously impossible for a scientific monthly to emulate the daily newspaper, to retain paid correspondents in every city of the globe, or to send a special commissioner on a roving tour. But with the goodwill of our colleagues NATURAL SCIENCE may easily be made a bond of union, and a means of communication between naturalists of our own race in all parts of the world. Not that we would in any way pass over foreign labourers in the same field; but if we can make our summary of British Scientific Progress fairly complete, then our Journal will be of all the greater use to those who have not had the rare good-fortune to be born Britons.

But it is not our only object to retail the results of investigation, and to summarise the news of the world of Natural Science. We do not merely wish to review scientific progress; but we hope to assist it. And in this hope we have polished our shield bright, and set our lance in rest. May all shams and bugbears, when they see themselves mirrored as they truly are, sink to the nothingness of which they are made; and may the more enduring obstacles to progress be, if not shivered, at least a little shocked by our onset! Of course this will gain us enemies as well as friends; but that is the more evidence of success, and, after all, we hope our enemies will not be very bitter, since we attack principles not persons, measures not men. It is in pursuit of this aim that we open our columns to those whom authority seems to treat too hardly, even if we do not always agree with them ourselves. It is not always the popular view that is the right one, and the history of Science shows too many instances of folly becoming wisdom, for even an editor to feel his judgment infallible.

F.R.S.—F.L.S.—F.G.S.—F.Z.S.

THE Royal Society is again under fire. At the Anniversary Dinner the President himself deplored internal but obvious evidences of its decay, and a correspondent of *The Times* was not slow to take up the tale. His article, which appeared on December 2, emphasises Lord Kelvin's words and adds some exceedingly outspoken criticisms. The Society is falling, he says, more and more into the hands of biologists, while workers in other branches of science no longer lay their papers before it but before those societies specially interested in their subjects. The cause of this lies, he maintains, "in the excessive influence of a small body of permanent officials, in the practical effacement of the non-official council, and in the consequent government of the Society by cliques instead of by the common-sense of the scientific community at large." As the autocrats in question he denounces the Dons of Cambridge (Trinity College) and the Professors of South Kensington. The "undue preference" shown in the disposal of the Government grant next receives his censure. Especially does he blame the Solar Physics Committee for drawing money

from this grant, despite the fact that they have since 1879 received an independent annual subsidy from the Government of no less than £500. It is, however, a little rash of the critic to wish that the distribution of this grant should be taken out of the hands of the Council; for if it were, as he suggests, divided among the various seats of learning about the country, the same difficulty of cliquism would, we fear, occur. No administrative body can wholly escape this charge.

There is another matter that might well have been touched on by *The Times* denunciator. The methods of election pursued, not only by the Royal but by many of our other learned societies, are exceedingly open to criticism. The mystic letters placed at the head of this article have too long imposed on the credulity of the public. It is too commonly supposed that they are symbols of vast erudition on the part of their happy possessors. Yet, with the exception of "F.R.S.," all these titles are obtainable by little more than simple purchase. The fellowship of the Royal Society comes, it is true, partly as the reward of original work, but mainly by the backing of influential friends. A glance at the list of that Society might possibly dispel the illusion that all its fellows were men of great learning. As for those who are really worthy of election, we do not deny that the majority of them get into the Society in course of time; but there are many others whose social position or personal peculiarities are, unjust as it may seem, allowed to tell against them. Indeed the whole system of election into the Royal Society seems to be in need of improvement. It may be legitimate, but it is hardly self-respecting for a man to suggest his own name as a candidate and then to tout round for support. So derogatory a proceeding should be impossible. Why should not the Society, without regard for personal feeling, select the twelve best men of the year, and let the election come as a surprise to the candidate? The distinction would at least be more honourable.

But it is possible that mere honour is not the only inducement to candidates for the fellowship of the Royal Society. Not this alone would cause a man to get his name put up year after year by some obliging friend, until common-sense or pique urged him to withdraw from the unsuccessful attempt. May not a deeper reason be sometimes found in the commercial value of the Fellowship, not only of the Royal but of the other Societies? We all know that F.R.S. means "Fees Raised Since," while not a few professional men could tell us, if they chose, what pecuniary profit accrued to them from the possession of any of the other magic letters. This is not just, either to the Societies or the public. Election does not and should not imply any professional ability on the part of the candidate. The tailor's cutter, when asked for a reference, replied, "Bring me some cloth and a pair of shears, and you'll want no reference." Such should be the attitude of self-respecting professional men; such is

the attitude of the single-hearted devotee of science. Fellowships may have a value in commercial circles; but no number of them can raise the reputation of a master among his colleagues, either in his own country or abroad.

FONA OR FUDDLEITE. A NEW MINERAL.

IN Mrs. French-Sheldon's entertaining volume, "Sultan to Sultan," in which she records her expedition to Kilima-njaro, there are given (p. 324) two analyses, which we have in vain endeavoured to understand. They are the work of the Royal College of Science, where they were made "through the joint courtesy of Professor Judd and Professor T. E. Thorpe, February 22, 1892." The signature of "T. E. Thorpe" is placed beneath each of them. We beg to quote the formula for the former, which is called "salt stone," as a New Year's puzzle to our readers:—

$$2\,Na_2O\,3\,W_2\,4\tfrac{1}{2}\,O.$$

The mineral, on the strength of this formula, is said to be "*simply* a hydrated sesquicarbonate of soda." Simply that! Where has the tungsten come from, and whither has the carbon gone? How does Professor Thorpe square $4\frac{1}{2}$ O with the atomic theory? And what is the meaning of Nag°, which figures in both analyses?

The second analysis is of a substance called "Tobacco stone," and this seems to be a more complex material, as it is called by Professor Thorpe "fona, mixed with the carbonate of lime and magnesia, a trace of common salt and clay." There is 29·92 per cent. of nagging soda and only 30·48 of carbon dioxide; so, apparently, this up-to-date alkali is satisfied with such a remarkably small proportion of acid that even out of this 30 per cent. it can spare some for the magnesia and lime. If this had been an ordinary compound there must have been a fine scramble for the carbon dioxide between the soda, lime, and magnesia.

Professor Thorpe very justly gives a new name to the material which has yielded these quaint analyses, for we cannot find "fona" in any mineralogical index. As the word is twice repeated, it can hardly be a misprint, though it has a certain resemblance to trona. Fuddleite would, perhaps, have been a more appropriate name. Would some chemist kindly let us know whether the analysis appears clear to him, though we hardly like to invite anyone to a task which has given us cause to doubt our own sanity? Perhaps some interpreter of dreams and expounder of oracles will tell us that in place of "Signed, T. E. Thorpe," there ought to be substituted "Signed, T. E. Thorpe, revised and emended by M. French-Sheldon."

THE VIVISECTION PROBLEM: SELF-HELP OR LAISSEZ-FAIRE?

THE Anti-vivisectionists have been bestirring themselves of late. They have had a meeting and made many speeches. They have

petitioned against the establishment of a Pasteur Institute in India. And that go-ahead Ladies' Club, or club of go-ahead ladies, *The Pioneer*, among its numerous subjects for discussion has lately included vivisection, and after an exciting debate declared it to be unjustifiable.

The justifiability of this practice depends on questions that have been questions since the beginning of civilisation, and that are likely to remain without their answer for many centuries to come. We at least do not presume to give an opinion. The above circumstances have, however, suggested one or two reflections that may be of service to both parties.

Everyone that knows anything of the progress of science, in whatever department it may be, is aware that it is effected by circuitous and often retrogressive paths. Many are the blind alleys up which we wander, many the pits into which we fall. What scientific man dares to say that he has never made a mistake? If such there be, we have small regard for him: be sure that he has never made a discovery. It is a cheap jibe to say that Science contradicts herself. That is nothing to be ashamed of. Oliver Wendell Holmes was wiser when he wrote:—

"Don't be consistent, but be simply true!"

What argument is it, then, to say that experiments on living animals have in some cases led to wrong conclusions, or that those who practised them have occasionally conducted unsuccessful operations? Prove that the whole method is unscientific, and that will be a valid argument. But who will be so rash as to maintain this, so long as in all other sciences there are not one but two factors of progress; not only observation, but also experiment?

We should hardly imagine that, at the present day, there could be found anyone who, having a knowledge of the subject, would deny that, however numerous the failures, experiments on living animals have been productive of great benefit to other animals. In both cases Man is naturally included under the term Animal. It is this benefit that is brought forward by the advocates of such experiments in excuse of any pain that may be inflicted by their performance. Benefit is, perhaps, not always derived; on the other hand, pain is by no means always inflicted. Let us assume that the exceptions balance one another. We shall then have left in one scale some amount of pain, some amount of benefit in the other. Now the benefit consists in the alleviation or the removal of pain. But it must be clearly borne in mind that, whereas the pain inflicted is on a finite number of individuals during a certain comparatively short space of time, the pain removed, on the other hand, is on an infinite number of individuals for all the unknown future. Which then is the heavier scale? Surely none will deny that the pain destroyed far outbalances the pain inflicted. Only on the assumption of an immediate millennium, can any other answer be given to the question.

The problem, though still perhaps insoluble, has become a little clearer. Pain and misery are inevitable in this world. For countless ages Nature has worked by woe. But out of anguish there rises advance, and pain is the parent of pleasure. It is thus that the mills of God are grinding, and man cannot stop their mighty wheels. But, may he hasten them? That is the question. A question which involves issues far larger than the justifiability of vivisection, issues such as the right of one individual over another, be the individuals animals or men. A question to which mere expedience has in all ages forced from the vast majority of mankind an affirmative answer. But, a question which, viewed from the peaks of philosophy or the precipices of morality, must still remain without reply.

THE NORTHERN DISTRIBUTION OF *Oxalis cernua*.

IN the recently-issued number of the *Proceedings of the Linnean Society* (Oct., 1893, p. 31), Professor G. Henslow discusses the northern distribution of *Oxalis cernua*, Thunb. This plant is a native of the Cape of Good Hope, but has become distributed, not only on the islands of the Atlantic, as the Bermudas, Canaries, and Madeira, but along both the north and south coasts of the Mediterranean. The first to allude to its occurrence in the northern hemisphere was Father Giacinto, who mentions the plant as being cultivated in the Botanic Garden at Valetta, Malta, in 1806. Professor Viviani records it for North Africa in 1824, naming it *Oxalis libyca*, while A. de Candolle refers (Fl. Calp., 1846) to its introduction into Gibraltar in 1826. About that time it probably arrived in Egypt with the mandarine orange, introduced from Malta by Youssouf Effendy; at the present time it occurs there only in the orange-gardens of Cairo and Esneh. The author suggests that the plant mentioned as cultivated in Malta in 1806—which Zerapha states that his contemporary, Dr. Giacinto, brought from the Cape, for the information of his pupils—was the source of the diffusion of the species in the Mediterranean region. The floral structure of the northern specimens affords an argument in favour of such an origin. While the species is naturally trimorphic (having long-, mid-, and short-styled forms) at the Cape, and also fruits there, it has never been known to bear fruit in the northern hemisphere, and the short-styled form is the only one described as occurring anywhere around the Mediterranean. Mr. Henslow examined numerous plants in the Maltese Islands and Egypt, and found, with but two exceptions, only the short-styled form. A single plant introduced with some palms observed at Cannes was also short-styled. The exceptions were two long-styled plants in the botanic garden of the School of Medicine at Cairo, one cultivated in a pot, and a stray one in the garden; and these, it is suggested, were a recent introduction from the Cape.

The intercourse in trade which has long existed between Malta

and the African coast, is sufficient explanation of its early introduction into the latter region.

Mr. Henslow goes on to describe the method of multiplication in the northern hemisphere. In Malta the plant is ubiquitous, carpeting the roadsides as well as all the exposed open ground around Valetta, and insinuating itself between the loose stones of the walls, appearing at the surface like a green fringe around each. It covers the tops of the walls in many places, and the lofty fortifications; it forms luxuriant borders to the fields, and also invades cultivated soil, so that, where weeds are not uprooted, a field will look as yellow as an English meadow with buttercups. It is propagated entirely by bulbs. A large plant growing among loose, stony *débris*, if dug up in January, will be found to possess a long tapering stem, throwing off thread-like lateral roots, and bearing minute leaf-scales with small white bulbils at intervals; as well as several larger ones at the crown, below the cluster of leaves. The fine thread-like rhizome extends downwards, sometimes for more than a foot, and grows from a bulb of last season. In many cases when the plant grows as above, the stem proceeds further downwards like a thread; but after a certain distance suddenly increases in diameter, forming a short rod-like structure about 1½ to 2 inches long, with a bulb at the end. This explains how it can reach great depths from which new plants arise in a subsequent season. On plants growing luxuriantly, runners above ground may be formed, which also produce bulbs at the nodes, so that a number of young plants arise at a short distance from the site of the parent.

The author remarks that the extensive vegetative reproduction of *Oxalis cernua* in the Mediterranean, like that of *Elodea Canadensis* in this country, is a strong argument against the assumption of the necessity of cross-fertilisation to ensure a vigorous offspring. Change of hemisphere has also resulted in a changed flowering period, as, in the north *Oxalis cernua* is in full blossom all the winter from November to April, while it is described as flowering at the Cape in June and July. The lengthening of the period is doubtless due, in part, to the fact that no fruit being produced, the plant goes on flowering, as it were, in hope, using up the energy which would otherwise be expended in the formation of fruit and seed.

The Structure of Diatoms.

A recent number of the *Verhandlungen des Naturhistz Med. Vereins zu Heidelberg* (vol. v., part ii.), contains an interesting paper on the structure and nuclear division of Diatoms, by Dr. Lauterborn. Readers of Natural Science (*see* Sept., 1892) will remember the author's name in connection with some researches upon the movement of these organisms which were carried on by him and Professor Bütschli. The present paper is a preliminary communication (with one plate) of a more extensive work which Dr. Lauterborn promises

to bring forward shortly. Besides many interesting details concerning the structure of the protoplasm and shell of Diatoms, and some valuable methods of staining the tissue while living, important and striking results were obtained with regard to the nuclear division, which in these forms always takes place by karyokinesis. The centrosome is always present close beside the nucleus, and visible even during life in the resting cell, but there are no striations round it until the commencement of division. At an early stage in the division there appears, between the centrosome and nucleus, a third body, which is the rudiment (Anlage) of the central spindle—though never spindle-shaped itself. It is probably derived from the centrosome, perhaps by division, but its origin has not been clearly made out. Appearing originally as a rounded body, then becoming rod-shaped, it expands and grows in a peculiar manner, which may best be illustrated by comparing its shape first to a coin, then, by increase of depth, to a piece used in the game of draughts, then to a drum, and, finally, to an elongated cylinder, which is longitudinally striated. Hence, in the rod-like stage its long axis does not correspond to the long axis of the future spindle, but to its transverse axis. When full-grown, it sinks into the nucleus, which, in the meanwhile, has passed through the stages with the dense and loose skein, and has now lost its membrane, and contains V-shaped chromosomes. At this period the centrosome, with its system of rays, becomes indistinct and disappears, and the cell protoplasm shows radiate striations round each of the poles of the central spindle. The chromosomes form an equatorial ring round the central spindle (*Monaster* stage). The ring splits into two rings which travel apart (*Dyaster* stage). The two rings become narrowed and constrict off the two ends of the central spindle, the two pieces so cut off becoming the centrosomes of the daughter cells. The cleavage of the cell passes through the middle of the central spindle, halving it accurately. The two halves of the central spindle perhaps become absorbed into the pieces which were cut off to form the daughter centrosomes. The daughter nuclei reconstitute themselves. The author concludes his paper with some interesting comparisons between this peculiar mode of formation of the nuclear spindle in Diatoms and in other cells. He considers that his observations support Bütschli's view of the homology between the centrosome and the micro-nucleus of Infusoria. It is interesting to note that all the stages of karyokinesis described —and observed chiefly in *Surirella calcarata*, Pfitzer, but confirmed in other species—were seen and drawn in the living state as well as in preparations; also that the protoplasm of these forms shows, both when living and after fixation, a distinct reticulate alveolar meshwork. The striations round the centrosome are seen to be formed by arrangement of the meshes or alveoli in rows, as formerly described by Bütschli. Dr. Lauterborn's complete work will be awaited with considerable interest.

THE principles of geological correlation to which we drew attention in our last number (vol. iii., pp. 404–7), do not seem to find favour with many prominent Fellows of the Geological Society. At the meeting on December 6, one speaker considered that "unless there was a physical change accompanying a faunal one, the latter was of secondary importance. Palæontology must be the servant and not the master of Geology." Another well-known stratigrapher "had no faith in fixing a plane of demarcation between Purbeck and Wealden Beds by means of Ostracoda." Are not these gentlemen somewhat insular in their point of view? They do not seem to distinguish between local and world geology. Lithological differences and other evidences of physical change are, of course, all-important for the local geologist and for mapping, but when it is a question of separating the Jurassic period from the Cretaceous, then some principle is demanded that shall be of rather wider application than the bursting of a river bar, or the closure of a lagoon in our own little corner of Europe. In short, the rock scale is parochial, but the time scale universal.

THE *Fortnightly Review* for December contains the second part of an article by Dr. A. R. Wallace on the "Ice Age and Its Work," dealing with the erosion of lake basins. It is well worth reading as a clear and concise statement of the arguments in favour of the power of ice to cut the basins. One of the points raised seems to be quite new. Dr. Wallace contrasts the well-known indented outlines of submerged valleys, such as now form harbours in the south of England, with the simple outlines of a lake in a glaciated region. In the latter the tributaries fall directly into the lake, instead of flowing into lateral arms formed by the partial submergence of the surrounding country, as would be the case if the main lake originated through subsidence.

WE are glad to hear from letters from Mombasa that Mr. G. F. Scott Elliott left that town for the interior early in November. He has been delayed on the coast in the organisation of his caravan, which numbers 73 porters. The abandonment of the Ruwenzori and Toru Stations will add greatly to his difficulties; but he has left fully resolved to reach his goal of Ruwenzori. We are also glad to hear that Lieutenant von Höhnel has safely arrived at Mombasa. He was seriously injured by a wounded rhinoceros some distance to the north of Kenia, and great anxiety was felt as to whether he would survive the long journey to the coast. He returned *viâ* Kibwezi, where from Dr. Charters his wounds received proper medical attention. His companion, Mr. Astor Chanler, will continue his journey to the north as soon as the men whom he sent back with von Höhnel have returned to him.

I.

Tyndall.

WITH the death of Professor Tyndall has passed away the second of the men whose names are associated as the three English men of science of the Victorian era. His claim to be included in this trio does not rest on his being the deepest thinker, the most accurate and ingenious experimentalist, or the most original investigator in the branch of science upon which he was engaged. If it did so, he might have to yield his place to another, for Lord Kelvin is probably as much his superior as a physicist, as Browning is thought by many to have surpassed Tennyson as a poet. Nevertheless, in both cases, the lesser man may be the more typical of and more influential on the age in which he lived. Though Tyndall's work must be ranked far below that of Darwin, he was far more the representative man of the two, owing to his brilliant versatility, restless energy, his combination of the culture of the literary student with the insight of the scientist and the power of the man of action, his breadth of sympathy and the apostolic zeal with which he fought for a sounder and more scientific system of education.

Professor Tyndall was born in the village of Leighlin Bridge, in Carlow, in 1820, but he came of a Gloucestershire family which is said to have been connected with William Tyndale, the famous Protestant martyr. His parents occupied but a humble position in his native village; his father had been a private in the Royal Irish Constabulary, and after his retirement from this force, he was in business as a small country shopkeeper. Tyndall, however, had the advantage of a fairly good education in one of the small schools which used to be the pride of many an Irish village; here he was well grounded in mathematics and grammar, and acquired from the one subject a habit of scientific method, and from the other an interest in literature. He has himself told us[1] how much he owes to the study of the latter at this school, though he entertained less affection for the discussions on the comparative merits of Popery and Protestantism which occupied a large proportion of his time. In spite of his father's poverty, Tyndall was kept at school till he was nineteen, but this was by no means unusual in Ireland before the days when attendance till thirteen was compulsory. On leaving school he obtained an appoint-

[1] An Address to Students: University College, 1868.

ment as one of the Civil Assistants in the Ordnance Survey, and was at first quartered at Carlow. He was, however, soon moved to Lancashire, and then continued his studies at the evening classes of the Mechanics' Institute at Preston; he at this time intended to become a civil engineer, and contemplated emigrating to America. Some experiments, however, which he saw one evening during a lecture at Preston, roused in him a keen interest in pure science apart from its practical applications.

He remained on the Ordnance Survey for five years and, as he had had experience of most branches of the work, became a well-trained surveyor. When the railway mania burst upon the country, this meant money, and Tyndall in 1844 entered the employment of a Manchester firm; for three years he was kept very hard at work on various surveys, but the excitement and strain and wild unrest of this life were too much for him. He had saved about three hundred pounds, so he threw up his appointment and in 1847 joined the staff of Queenswood College in Hampshire.

This step was probably induced not so much by the fact that the toil of the railway survey was too severe: he had no interest in the work: he was never avaricious and he had already come to feel an enthusiasm for science and had fallen under the influence of Carlyle. He had intended to devote his leisure here to research, but he felt the need of further scientific education before he could seriously commence this. In company, therefore, with his friend and colleague, Dr. Edward Frankland, he left England in 1848.

It was probably due to the teaching of Carlyle that they decided to study in Germany, and there the fame of Bunsen drew them both to Marburg. There they studied chemistry under the great master, and worked at physics under Gerling and Knoblauch. With the latter he wrote one of his earliest papers, "On the Deportment of Crystalline Bodies between the Poles of a Magnet," which was published in the *Philosophical Magazine* for 1850, and was republished in the *Annales de Chemie*, the Archives of the *Bibliothèque Universelle*, and Poggendorf's *Annalen*. At the same time he was a constant reader of Carlyle; and Tyndall has himself told us that it was thus he derived the inspiration which kept him hard at work, and made him face his early morning tub through the cold of a German winter.

From Hesse-Cassell Tyndall moved to Berlin, where he worked at diamagnetism in the laboratory of Professor Magnus, and gained the friendship of Helmholtz and others of the ablest of the younger school of German physicists. He came to London in 1850, and there made the acquaintance of Faraday. Of his first meeting with the man of whom he has often spoken as "my master" he has given us a charming account in his life of Faraday. Immediately on his return to England he received an appointment at the Royal School of Mines, where he found among his colleagues his life-long friend, Professor Huxley. In 1852 he was appointed Professor of Physics at this school, then

in Jermyn Street. The year 1853 was an important one in Tyndall's life. He was elected a Fellow of the Royal Society; and at the instance of Dr. Bence Jones he gave the first of his long series of lectures at the Royal Institution; and shortly afterwards Faraday gained for him the appointment of Professor of Natural Philosophy at the Royal Institution, with which Society he remained connected till his retirement in 1887. For the last 20 years of this period he was, in addition, Superintendent of the Laboratory there, to which office he succeeded on the death of Faraday in 1867.

During his student days in Germany Tyndall made a visit to the Alps, and the impression they made upon him was ineffaceable. He went back, in company with Professor Huxley, in 1856, in order to study some glacial problems, and from that year onwards rarely missed a visit to Switzerland. To his scientific work there reference will be made later on; but no sketch of his life would be complete which did not consider the Alpine feats of which he was so justly proud. He was one of the early group of mountain climbers, and one of the most successful of the band of Englishmen who led the attack on the more dangerous of the giants of the Alps. He must have been a man of splendid physique and sound judgment, or he must inevitably have come to grief during some of his reckless feats. No doubt most of Tyndall's early ascents and glacier expeditions had a scientific purpose; but they were carried out with reckless disregard for life and limb, and he did many things for which he would nowadays get his knuckles rapped pretty briskly in the pages of the *Alpine Journal*. Such, for example, are his ascent of the Jungfrau alone, without a greatcoat, and with only a light alpenstock, which he lost on the top. A still more daring deed was his first ascent of the west point of the Dufour-Spitz—the highest peak of Mte. Rosa—from the Riffel; in the only previous ascent of this point the party consisted of five first-class English climbers, Ulrich Lauener, and three other guides, and they went up, moreover, by the west arête. Tyndall did it absolutely alone.

His greatest Alpine achievement was the first ascent of the Weisshorn from Bies in 1861, in which he was accompanied by Wenger and the old guide who still lives at Zermatt, and has ever since been known as "Weisshorn Bennen." In his attacks on the Matterhorn he was less successful; but for long he held the highest record before the summit was actually reached. He was the third to gain the top, and the first who crossed the mountain from Breil to Zermatt; it was in a ten minutes' rest on the arête during this traverse that he thought out the exquisite little prose poem, "Musings upon the Matterhorn." Three years later he made the third modern traverse of the very difficult glacier pass known as the "Old Weisshor," by a variation on the previous routes, and crossing above the Filar glacier. The last and most difficult of his first ascents was in 1862, when he climbed from Breil with the guides, Bennen, Carrel,

and Walthers, the peak that will long keep alive the memory of his Alpine feats with the name of the "Pic Tyndall."

Most of these ascents, and his other climbing adventures, and he had not a few, were recorded in his charming work, "Hours of Exercise in the Alps" (1871).

Another athletic feat was his expedition at Niagara, when he worked, by the south side of the gorge of the Horseshoe Fall, round the promontory upon which there stood the Terrapin Tower, and under the foot of the Fall.

In addition to his Royal Institution Professorship he held numerous other appointments at different times. In 1855 he was made one of the Examiners under the Council for Military Education. In 1864 he was given the post of Scientific Adviser to the Board of Trade, an office which he resigned in 1883, because he denounced as dishonest the action of the authorities in regard to some new patents. With characteristic generosity he had offered, when he accepted the office, to serve without pay, if his salary could be devoted to the benefit of the sailors. It is needless to say that he received numerous honours from the leading scientific societies at home and abroad; he was awarded the Rumford Medal of the Royal Society in 1864, while he was a doctor of four universities. Halle made him Ph.D., Oxford D.C.L., and both Cambridge and Edinburgh L.L.D. In connection with the last it may be remembered that he received the diploma from the hands of Thomas Carlyle, who that year held the office of Rector. With Carlyle Tyndall was very intimate, and he reverenced him as a father. After Mrs. Carlyle's sudden death he carried off the broken-hearted old philosopher to the south of France, and when Froude's publication of her letters had thrown a shadow on the fame of Carlyle, no one stood up more manfully than Tyndall in the defence of his old teacher and friend. The bust of Carlyle on the Chelsea embankment was unveiled by Tyndall about this period, and the prophecy at the close of his speech was one of those that greatly help their own fulfilment. He compared Carlyle in nobility of form and ruggedness of strength to the Bel Alp that rises at the back of his châlet on the Eggishorn, and the shadow that overhung the memory of Carlyle to the bucket of water whipped up into a cloud that could temporarily obscure the mountain outline; but he predicted that the mist would soon be swept away, and the character of Carlyle stand out as clear and spotless as before.

Tyndall's connection with the British Association is mainly recollected owing to his famous Belfast address in 1874, in which he discussed the relations of contemporary scientific thought and revealed religion; the orthodox party took alarm at the views therein expressed being proclaimed so publicly before the world; and Tyndall was pelted with pamphlets, scolded in sermons, and denounced in the press. The address was unquestionably a materialistic manifesto, and it is a remarkable sign of the progress of the last twenty years, that

those who, like *The Times*, denounced it at the time, accept it to-day. Tyndall first went to the meetings of the Association at Ipswich in 1851, and for some years was a regular attendant; in 1867 he delivered the "Working Men's Lecture," taking as his subject, "Matter and Force"; he was President of the Mathematical and Physical Section in 1868, when his address dealt with "Scientific Materialism." In 1870 he was the lecturer for the year, and read his famous essay on the "Scientific use of the Imagination."

In later years Tyndall was not so prominently before the public, especially after his marriage, in 1876, to the Hon. Louisa Hamilton, the eldest daughter of the late Lord Claud Hamilton. He spent most of his time at his house at Haslemere, and went every summer to his chalet, the Villa Lusgen, in Switzerland. In 1877, however, he accepted the Presidency of the Birmingham and Midland Institute, and gave as his inaugural address a lecture on "Science and Man," by which he again roused the wrath of his theological opponents. In 1887, he resigned his position at the Royal Institution, and was entertained at a farewell dinner by what was described by *Nature* at the time as the most representative gathering of English Science that had ever been held. Since then he had written or lectured but little, though the Orange blood that was in him led him to enter into political warfare in opposition to the different Home Rule schemes. Two years ago he was taken dangerously ill, but he recovered, and he visited Switzerland this summer as was his wont. He returned in October, and, in consequence of a tragic accident, peacefully passed away on the 5th December.

It is difficult for one who is not a physicist to give any sketch of Tyndall's work in science, as his main work lay in this department of research; but this I may be allowed to attempt, as physics do not come within the domain of NATURAL SCIENCE. The first research was upon "The Phenomena of a Water Jet," which was not, however, published till 1851. The subject which he made especially his own was diamagnetism, upon which he wrote about twenty papers, commencing with a short note "On Diamagnetic and Magnecrystallic Action," published in 1850 in the second volume of the *Chemist*, and in the Reports of the British Association in the following year a more detailed memoir on the same subject, entitled "Diamagnetism, the polarity of the diamagnetic force, the magneto-optical properties of crystals, and the relations of magnetism and diamagnetism to molecular arrangement"; this gained for him admission to the Royal Society. Radiation was another question on which he did much work. He took it as the subject of his "Bakerian" and "Rede" Lectures in 1864 and 1865, when he announced his discovery of "calorescence," in which, by altering the refrangibility of the ultra-red rays of the spectrum, they can be rendered visible. This is simply the converse of Stokes's well-known experiments on "fluorescence," by which the ultra-violet rays, after analogous treatment,

can be seen. Some of his most interesting results with radiation were in connection with its meteorological influence. Thus he showed that the effects ascribed by de Saussure, Hopkins, and others to the atmosphere, really result from the aqueous vapour in it, which forms the blanket protecting the earth from excessive cold at night. His study of the atmosphere in this connection subsequently came in usefully during his elaborate experiments on fog-signals. In relation to the same subject, he was led to study the effects of the floating particles in the air, and he showed that the blue of the sky, of the Alpine lakes, and of the sea is due to the polarisation caused by these particles. He manufactured artificial sky by some new chemical reactions produced by light, in order to demonstrate the truth of these conclusions. Another outcome from his interest in this line of inquiry was his work on the putrefactive influence of the floating particles of the air. This he summarised in two lectures; one on "Fermentation, and its bearings on Surgery and Medicine," delivered before the "Glasgow Science Lectures Association" in 1876, and one on "Dust and Disease," in the course of which he described a fireman's respirator made from cotton wool and glycerine which he had invented.

More important scientifically was his valuable investigation into Dr. Bastian's claims to the discovery of the spontaneous generation of bacteria in vegetable infusions. As Tyndall has told us, as a believer in the nebular hypothesis, he is logically bound to believe that spontaneous generation did come about some time; but with true scientific candour he clearly showed that Dr. Bastian's experiments had not been performed with sufficient care, and that when full precautions are taken, the bacteria do not appear in the infusions.

His geological work is far more important than his biological. His first contribution to this subject was a lecture "On some of the Eruptive Phenomena of Iceland," delivered before the Royal Institution in 1853, in which he illustrated by experiments the causes of Geysirs. His remaining geological papers may be divided into two groups, first, those bearing on slaty cleavage; and second, on glaciers and glaciation. His main contribution to the former was published in three papers in 1856; one was "Observations on the Theory of the Origin of Slaty Cleavage of H. C. Sorby" (*Phil. Mag.*, vol. xii., pp. 161-164), the second was a lecture on "Crystalline and Slaty Cleavage," finally published in his "Fragments of Science," and the third was a "Comparative View of the Cleavage of Crystalline and Slaty Rocks" (*Proc. R. Inst.*, vol. ii., pp. 295-308). In these three papers he agreed with Sorby as to the fundamental difference between the mechanical and crystalline cleavage, and annihilated Professor Sedgwick's view that slaty cleavage is identical with crystalline, and that therefore mountains composed of slate are really often one huge crystal.

But Tyndall went further than Sorby and showed that slaty cleavage can be induced by pressure in an apparently homogeneous

body, whereas in the experiments of the latter some flaky particles such as mica were always mixed with the substance used. Later on, however, Tyndall recognised that the lateral yielding of the clay under pressure, and the consequent tangential sliding of one particle over another, contributed very largely to the result.

Tyndall's glacier work was more extensive and important. His main contribution was the discovery of regelation, by which the *method* of glacier motion is easily intelligible. He next showed, by some difficult work on the Mer de Glace in mid-winter, that the glaciers move in the winter as well as in the summer, and determined the rate. He subsequently showed how the well-known "veined structure" of glaciers is a result of the flow of the ice. His principal paper upon the subject was one contributed to the *Philosophical Transactions* in conjunction with Professor Huxley in 1857.

His studies upon this subject were collected and summarised in his books on the "Glaciers of the Alps," and in "The Forms of Water," which were published in 1860 and 1872 respectively. In addition to his work on the properties of glacier ice, he closely watched the action of the glaciers and held somewhat extreme views as to the extent of their erosive power. Thus he assigned a far larger influence to glacial erosion in the configuration of the Alps, in his lecture in 1864, than is fashionable at the present day. He joined in the controversy on the origin of the Parallel Roads of Glen Roy, and advocated and restated with great clearness Jamieson's theory of their formation by the action of a glacier dam.

It is, however, less on his work as an original discoverer than in his brilliant powers of lucid exposition that Tyndall's influence has depended. It is generally argued that lecturing was his forte, and in this he was unusually successful. Most of his text-books were based on his lectures, such as his "Lessons in Electricity," which were given as a course of lectures to children at the Royal Institution, and show his capacity for using the simplest of apparatus. His books on "Sound," "Light," and "Heat, considered as a mode of Motion" were also compiled from lectures. The two first were given in a tour in the United States in 1872, the profits of which, amounting to 13,000 dols., he presented to the Universities of Harvard, Columbia, and Pennsylvania, for the endowment of research scholarships.

The department of this work for which, perhaps, the younger generation owes Tyndall the deepest debt of gratitude, is that, in conjunction with his two friends, Herbert Spencer and Huxley, he fought the fight which gained for Natural Science a free hand in the investigation of the whole range of natural phenomena. This now sounds like a truism; but it was not when he wrote that "the impregnable position of science may be described in a few words. We claim, and we shall wrest from theology, the entire domain of cosmological theory. All schemes and systems which thus infringe upon the domain of Science, must, in so far as they do this, submit to its

control, and relinquish all thought of controlling it." If a distinguished prelate were to stand up before the British Association to-day, and say that recent theories on the sequence of Plutonic rocks did not agree with the order of the strata assigned to the Holy City in Revelations, and were therefore unorthodox, and therefore necessarily wrong, he would be laughed at. But when Bishop Wilberforce denounced Darwinism in 1857 this sort of argument was valid, and had to be replied to. The fact that the same is not the case now, is no doubt largely due to the Agnostic crusade of Professor Tyndall. It is now universally recognised that in all matters of natural phenomena and physical energy, Science is the final court of appeal, and that if religion teaches differently so much the worse for religion; the very reference to prayer as a form of physical energy, which was accepted 40 years back, now sounds irreverent, so great has been the progress.

During the course of this fight Tyndall made for himself many foes, in addition to those such as Forbes, Tait, and Ericson, whose enmity he had aroused during the course of his scientific work. Thus his series of papers on prayer and miracles, and his proposal to test the former by a series of laboratory experiments on a large scale, roused the fighting section of the Church. When Oxford gave him the degree of D.C.L., it was in face of the protest of some of the "old school." It was doubtless to the same cause that he owed the caricature of him that appeared in "The New Paul and Virginia," where as "The Professor" he appears as the vulgarest and coarsest of the creations of Mr. Mallock's usually subtle spite.

A second belief which Tyndall has largely helped to impress upon the public mind, is that the value of scientific education is not the dissemination of a mere catalogue of facts, but of more precise methods of thought. As he put it, "science should be studied not as a branch of education, but as a *means* of education;" we are therefore bound to carry the scientific method into every department of thought, and work, and life, and to carry every argument to its logical conclusion, let them clash with whatever cherished beliefs they may. When Tyndall returned to England from Germany this was not the current method of thought. Men worked at science, but feared its conclusions; it was the date when, as Tyndall has put it, there were "Tories even in science who regard Imagination as a faculty to be feared and avoided, rather than employed." There were men like Faraday who seemed to belong to a kind of "Manchester School" in science, who worked at science all the week, and preached Sandemanianism on Sundays: they divided their brains into two watertight compartments, and put their reason in one, and their religion in another, and trusted to habit, fashion, and indolence to keep the bulkhead impervious. But Tyndall had no sympathy with this school of thought: he believed that, on the other hand, "science has of late years assumed a momentous position in the world. Both in a material,

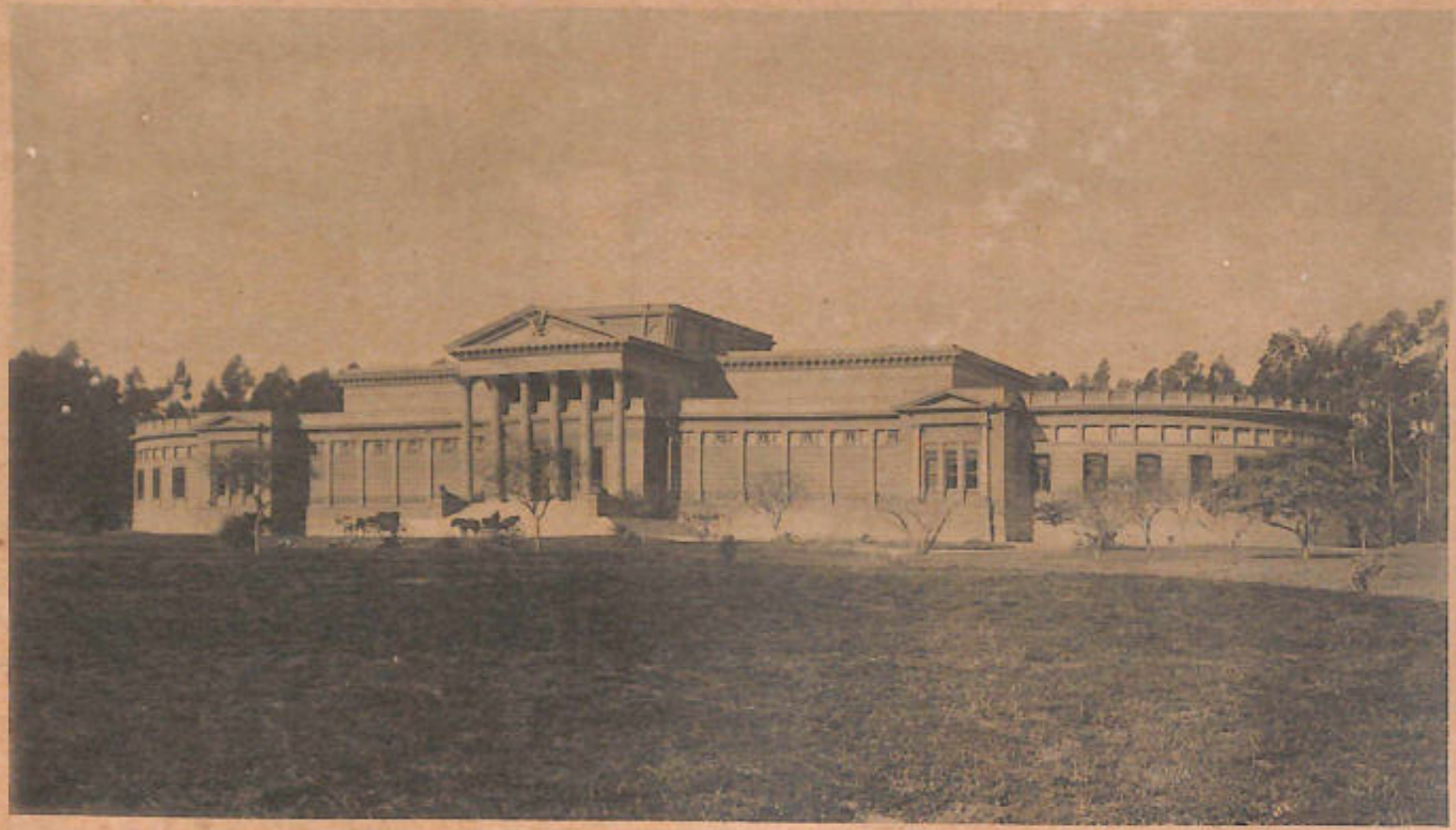
and in an intellectual point of view, it has produced, and it is destined to produce, immense changes—vast social ameliorations, and vast alterations in the popular conception of the origin, rule, and government of natural things." He therefore strove with a missionary zeal to shatter this dividing wall between science and general thought; and he did it. It was perhaps his recognition of a certain analogy between this bisected brain and the political brain of a later date, which often appears to be divided by a double bulkhead into three separate compartments for politics, morality, and reason, that led to the vigour of his anti-Gladstonian utterances. He recognised an old foe, and promptly beat his pen into a tomahawk, and dashed into the fray yelling the fiery warwhoops of his race.

There was no doctrine, he thought, savoured more of the evil one than this system of purchasing intellectual peace at the price of intellectual death; and throughout the whole of his career, in spite of all opposition and abuse, he hurled against it his most powerful ridicule and invective, and gave expression to the most passionate appeals that he could utter. The world has many such refuges, he said, and they will be used by all "to whom repose is sweeter than the truth. But I would exhort you to refuse the offered shelter, and to scorn the base repose; to accept if the choice be forced upon you, commotion before stagnation, the breezy leap of the torrent before the fœtid stillness of the swamp."

Thanks to his lucidity of exposition and the fascinating charm of his clear and nervous English, his efforts have exercised a deep influence, and before his death he had the pleasure of seeing the gospel he had preached from presidential chairs, in lectures, essays, and the Press accepted by the majority of his fellow countrymen. So much so has this been the case that even in reference to the Belfast address itself *The Times* could say on his death, "we all stand to-day where Tyndall stood 20 years ago."

It was, perhaps, an even greater satisfaction to him that he could watch the improvement in scientific education, for which he had so long contended, and the wider interest in which he did so much to foster. When we recollect how he was ever ready to leave the quiet of his study to help on the spread of scientific thought among his countrymen, one can but think that he had taken as his life motto the words of the old Israelitish leader that we may now adopt as his epitaph:—"Speak to the people, that they go forward."

J. W. GREGORY.



LA PLATA MUSEUM.

II.

Natural Science in Japan.

I.—Past.

FEW people in this world have the love of Nature so strongly inborn in them as have the Japanese. It pervades all their life. Religion, art, poetry, daily pursuits and holiday recreations: all are imbued with this nature-spirit, that seems to belong to this nation like it has to no other with which the western world has been acquainted since the days of ancient Greece.

Pardon me, severely scientific readers, while I illustrate these bold remarks more fully.

Shinto, "the way of the Gods," which, before the introduction of Buddhism from China, served the Japanese as a religion, and which the restored Mikado has of late endeavoured to re-instate in its former pre-eminence, is a religion, not so much in our modern moral acceptation of the term, as in the primitive meaning that it had for the Romans of old. A veneration of ancestors; a worship of all the powers of nature, of wind and ocean, sun and fire, food and pestilence; a worship mingled with love that has personified the rivers, the mountains, and the trees of their beautiful native land; a religion of artless forms and homely prayers, that eschews both the maxims of the "ower guid" and the sermons of the "ower lang," and that is almost pathetic in its natural simplicity: such is Shinto.

Art, not the conventions of the schools and the copies of Chinese masters, but the living naturalism started by Okyo and perfected by Hokusai—how it sports with and idealises the minutest facts of this natural world! Natural this art is in its irregularity, even in its impossibility, and above all in the life with which it is instinct. It is not the remote or the grand that inspires these artists, but the near, the quaint, and the beautiful. "The Japanese," one must admit with Basil Hall Chamberlain, "are undoubtedly Raphaels of fishes, and insects, and flowers, and bamboo-stems swaying in the breeze." Not merely the painters, but the carvers in wood and in ivory, the workers in clay and in metal, all are caught in the embrace of an ardent naturalism chastened by a pure simplicity. With what loving diligence the craftsman toils over his golden representation of a beetle, with wings, elytra, and mouth-parts all complete, while the patience of the sculptor who gave five years to making the

feathered eagle of steel, lately shown at Chicago, is hardly to be conceived by us hurried Europeans.

Poetry, with all its conventions, and they are neither few nor easy, is for the Japanese almost entirely an impressionist rendering of nature. Sometimes a mere sketch, that appeals to one solely by its beauty; sometimes a piece of such sympathy with the soul of things that its subtlety and delicacy perpetually charm while they perpetually elude us. But this is a subject on which my pen is apt to run away with me, so in this place I must content myself with translating, however imperfectly, a poem of either kind. First, then, read this description of—

AN AUTUMN EVE.

Passed the shower; but yet
On dark fir-leaf its memory lies
In pearls of glistening wet,
Wherefrom silver mists arise
Veiling in night the autumn skies.

And now hear this, remembering that it is centuries older than the song of Keats:—

MORNING-GLORIES.

Shame! that one should call
Morning-glories' dew-sprent gleam
A thing ephemeral.
Flowers, on that wise, might deem
Man an evanescent dream.

Thus we pass to the pleasures and pursuits of the people, of which this last poem inevitably reminds us. For if there is one thing characteristic of the Japanese, it is their love of flowers. When I was in Tokyo it was late summer, and early every day the florists' gardens at Irya were thronged by those who came to see the morning-glories bloom, while towards evening worshippers visited the island shrines of Benten to delight in the sacred lotos that flowered around them. Everyone has heard of the numbers that crowd that wonderful avenue at Ueno Park when its double cherry-blossoms float like pink clouds in the air, while the world itself has yielded to the chrysanthemum, the royal symbol of Japan. Nor is this merely fashion, of the same sort that takes some of us to private views and musical "at homes." No house in Japan so small but has its little landscape garden, if only in the back-yard; and no inn so humble but can find a flower to brighten the room of every guest.

The love of animals, though not perhaps quite so conspicuous, is nevertheless sufficiently obvious. Some of this may be due to the influence of the Buddhist religion with its principle of incarnation. This at least places a great check on the taking of animal life, so long, that is, as the animals are warm-blooded. This too, together with various mythological traditions, may account for the prevalent practice of keeping sacred animals attached to the various temples. These animals are generally albinos, but also include herds of deer,

tortoises, and carp. In a few cases the number and different kinds of animals give the temple-yard almost the aspect of a zoological garden, and the resemblance is by no means diminished by the light-hearted crowds that pass from cage to cage, feeding the inmates with biscuits and beans. Whatever may be the theological explanation of this, the mere fact argues an initial sympathy with the brute creation, of that peculiar but common kind that manifests affection by imprisoning its object. Primitive and child-like this spirit may be; but we know it well enough ourselves, and so we have a fellow-feeling for these bright-eyed, brightly-clad little damsels that fly their butterflies fastened to a long black hair, and these laughing troops of boys that chase the cockchafers with long bamboo poles sticky at the tip.

More need not be said. The love that these children of nature have for their mother is now abundantly exemplified. But this feeling, as my friend O. H. Latter has recently reminded us (NATURAL SCIENCE, vol. iii., p. 41), is in no way connected with the scientific spirit. The demand for natural history objects supports more than one shop in Tokyo, itinerant vendors of the same ply a thriving trade, and the travelling raree-show reaps a harvest in every town. But the objects sold and exhibited are such as please children, not men; they excite curiosity and wonder without conveying knowledge.

At last, however, the scientific spirit, with other such modern improvements as quick-firing guns and labour disputes, has invaded the land of the rising sun; and it is the object of this paper partly to trace its gradual growth, partly to show how it flourishes to-day, now that the Japanese nation have cast off the leading-strings that they borrowed for a time from the Western world.

Just as, in ancient times, Japan received from China and Corea her religion, her literature, her art, and such science as there was to receive, so at the present day she has, for good or ill, adopted almost wholesale the methods and results of our own vaunted civilisation. The modernisation or Europeanisation of Japan has been a purely defensive measure, initiated and carried on by a certain section of the Japanese themselves, and notably by their great statesmen, the Counts Ito and Inouye. But the actual working out of the scheme has of necessity been placed till recently in the hands of foreigners employed by the Japanese Government. In this development two great races, the Anglo-Saxon and the Germanic, have played prominent parts, and in the particular parts they have played the respective genii of these two races have conspicuously asserted themselves. The English-speakers have, as usual, devoted themselves to the more practical and obviously utilitarian side of life, embellished, as usual, by the attempt to impose on an alien people their own particular religious opinions. To them are due the railways and the College of Engineering, the Navy and the Mint, the Press and the Prayer Book. The Germans, on the other hand,

have exerted their influence chiefly in the direction of history, medicine, and the natural sciences. It is to the labours of such men as Engelbrecht Kaempfer, C. P. Thunberg, P. F. von Siebold, Edmund Naumann and Max Fesca that the present position of natural science in Japan is largely due. Of late, too, many Japanese students have been attracted to America rather than Europe, both by the improvement of scientific education there and by the greater nearness of that country. As a consequence of all this, English scientific men have less communication with their Japanese colleagues, and know less about their doings than it would be to their advantage to have and know. And this it was that suggested the present account.

The present state of Japanese science will be better appreciated if we compare it with what has preceded, and trace its gradual development.

It was originally through the commercial enterprise of the Dutch that scientific men were able to acquire for themselves some knowledge of this far country, and at the same time to introduce to its inhabitants some of their own more advanced methods and knowledge. The *materia medica* of snakes' skins, dragon's blood and more disgusting, if less rare, substances, and the oriental nostrums of massage, acupuncture and moxa were, if not superseded, at all events modified by the wider knowledge of drugs and the more rational acquaintance with human anatomy communicated by the physicians of the Dutch East India Company. Of these men the first of importance was Kaempfer, a Westphalian, who came to Nagasaki, then the only port of Japan with which the Dutch were permitted to trade, in 1651, being then 39 years of age. He stayed in Japan two years and two months, during which time he was, like other foreigners, practically confined to Deshima, an island district of Nagasaki, except on the two occasions when he attended the Dutch Embassy on its yearly visit of homage to the Shogun's Court at Yedo, now Tokio. In spite of the disadvantages under which he laboured, he collected a vast amount of information, subsequently given to the world in his *Amœnitatum Exoticarum Politico-physico-medicarum Fasciculus V.* (Lemgo, 1712), and in the *History of Japan*. Here we may venture to pride ourselves on the fact that at least the original publication of the latter work was due to an Englishman, Sir Hans Sloane, who purchased the manuscript, had it translated into English by Dr. J. G. Scheuchzer, and published the translation at London in 1727-8; and this English version was the basis of all subsequent foreign editions of the work.

In Kaempfer's time a knowledge of natural history seems to have been very sparingly diffused among the Japanese. Monsters, indeed, they were well acquainted with, such as the unicorn, by them called Kirin, the Hoo, which is generally translated Phœnix, and the sea-dragon or Ryo; but animals of a less exalted type appealed

to them in a very small degree. Kaempfer regrets that the shells and other marine invertebrates, such beautiful material for a zoologist, are wasted on the sea-god, whom he calls Jebus. Pearls, indeed, they prized, having gained from the Chinese a knowledge of their value; but they knew so little of their nature as to imagine that some of them could grow of themselves and breed fresh pearls. Earthquakes, frequent though they were, they still ascribed to some large whale burrowing under the islands. We cannot, however, venture to laugh at the Japanese, for the works of Gesner and Aldrovandus, published not half a century before, contained monsters quite as terrible and myths no less absurd.

Over a century elapsed before another man of any scientific importance found his way to Japan. At last, in August, 1775, C. P. Thunberg, the Swedish entomologist, came as physician to the Dutch legation, and he stayed in Nagasaki till December, 1776. He was not idle during his visit, and the results are to be found in his *Flora Japonica*, published at Leipzig in 1784, in *Icones plantarum japonicarum*, five folios published at Upsala, 1794-1805, and in two volumes issued at Paris in 1796 and entitled *Voyages de C. P. Thunberg au Japon*. The natural history portion of the last work is comprised in chapters xviii., xix., xx., and xxiii., of volume ii. Besides making researches on his own account, Thunberg instructed some Japanese in various branches of natural history, especially botany.

Again we pass over three-quarters of a century before the arrival of the greatest German of all who have visited Japan. Philipp Franz von Siebold was born at Würzburg in 1796. In 1822 he entered the service of the King of the Netherlands as physician to the East Indian army, and on his arrival at Batavia was appointed leader of a Dutch scientific mission then starting for Japan, in which position he landed at Nagasaki in August, 1823. Here, as Professor Chamberlain writes in his useful book *Things Japanese*,—"By force of character, by urbanity of manner, by skill as a physician, even by a system of bribery which fell in with the customs of the country, and which surely, under the circumstance, no sensible man of the world will condemn, he obtained an extraordinary hold over the Japanese, suspicious and intractable as they then were. Having, in 1826, accompanied the Dutch embassy to Yedo, Siebold obtained permission to remain behind—the sole European in that great Asiatic capital, then absolutely sealed against the outer world. The excuse pleaded and accepted was that he would instruct the Japanese physicians in the more recondite branches of their art. His leisure he utilised in multifarious scientific researches; and so well did he know how to ingratiate himself that some of the highest in the land willingly added to his store of knowledge. Suddenly a rumour got about that the chief Court spy had sold him a map of the country. This was treason according to the old Japanese law. The spy was ordered to commit *harakiri* [disembowelment], and Siebold

was cast into a dungeon, from which he emerged only on the 18th January, 1830, with strict orders never to return to Japan." In 1859, however, after the American, Commodore Perry, had forced open the gates of Japan, Von Siebold returned as a semi-official ambassador, and, for a time, actually entered the Japanese service as a negotiator. In these troublous years he was not so successful, either in politics or science, and in 1862 he returned to his own country, where he died four years later.

The best known of Von Siebold's works is *Nippon: Archiv zur Beschreibung von Japan und dessen Neben- und Schutzländern*, a splendidly illustrated folio, published at Leyden and Leipzig, from 1834 to 1842, with the assistance of the King of the Netherlands. In conjunction with C. J. Temminck, H. Schlegel, and W. de Haan, he published a *Fauna Japonica*, which appeared at various dates between 1834 and 1851; while, with the assistance of J. G. Zuccarini, he presented to the Royal Bavarian Academy of Science a memoir entitled *Floræ Japonicæ familiæ naturales*. It is not, however, any of these elaborate works that now demands our attention, but, rather, a rare and little-known quarto pamphlet of 16 pages, originally published in 1824 at Leyden, and fortunately, since none of the London libraries seem to possess it, afterwards reprinted in Oken's *Isis*, Bd. xx., Heft 2, columns 135-143, 1827. Its title is *De historiæ naturalis in Japonia statu, nec non de augmento emolumentisque in decursu perscrutationum expectandis dissertatio, cui accedunt spicilegia faunæ Japonicæ*, and it contains so very curious and interesting an account of the state of natural science in Japan at the time of his visit that, considering its general inaccessibility, I think I cannot do better than quote from it somewhat extensively.

In this account of Von Siebold's, among other characteristics of the Japanese, some of which have already been alluded to, we shall notice especially these three: their love of the monstrous, the keen eye united with the cunning hand, and the practical bent of their minds. We shall further notice that the position of natural science had considerably improved since the days of Kaempfer, 175 years before. In fact Kaempfer's own labours, followed by the example of Thunberg, had aroused among the learned Japanese an almost incredible ardour and curiosity in natural history.

Botany, says Von Siebold, was especially cultivated by them, partly for its service in the pharmacopeia, since, like all oriental nations, they were much given to herbs, and had at least 500 in daily medical use, partly since they depended on the vegetable kingdom for almost all the necessaries of life, in the way both of food and clothing. But not entirely to these practical ends did they study the science, since our author adds, "for their own pleasure and for the ornament of their houses they cultivate the rarer plants, which they are at pains to procure not only from distant provinces of their own land, but even from China, and especially from Corea." Their



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botanical knowledge came originally from China, and so they denoted not only these introduced plants, but also the common native ones, by Chinese characters. From Thunberg, however, they had also received the Linnean names, and remembered them with pleasure. In their rare intercourse with either botanists or their books, it was hardly to be expected that they should have made a systematic study of botany; "nevertheless," says Von Siebold, "I have often wondered at the exactness and dexterity with which they distinguish the slightly differing species of a complicated genus and define their varieties." And, again, he tells us how the Japanese paint flowers, both wild and cultivated, so accurately that one can easily distinguish genera and species. A beautiful example of a Japanese botanical work of this period is the *Honzo Zufu*, by Iwasaki Tsunemasa, Yedo, 1828, of which a copy, belonging to Mr. A. Bisset, is deposited in the library of the Botanical Department of the British Museum.

The study of Zoology had prospered less; for investigations into the qualities of animals seemed to this herbivorous nation less necessary and useful than into those of plants. "At the same time," says Von Siebold, "they have a very accurate knowledge of such animals as do happen to be of any use to them, especially of fish, crustaceans, shell-fish, and certain other molluscs. They often form collections of shells, and make various articles out of them. In all their collections they value a thing the more for its montrosity. They carefully preserve all deformities and oddities that Nature has chanced to issue from her mint, and what Nature is unable or unwilling to produce, they manufacture by art." The Mermaids and Dragons composed of parts of different animals joined by *papier maché*, such as are still to be seen in the museums attached to certain temples, are described by Von Siebold, who admits that they so rivalled nature as easily to deceive the eyes of a tyro. "They have even," he exclaims, "attempted to deceive me with a human fœtus made monstrous by their craft. Frogs and tortoises with supernumerary heads or limbs they esteem as rarities. The vogue of these artifices is shown by the fact that they are exposed for sale at exorbitant prices. The love of lucre invents fresh monsters from day to day, and is a strange stimulus to zoological labours. To this end they keep deer, bears and monkeys, also white rats, hares and rabbits, and they rear the rarer birds which they collect from remote provinces and even from China. Although the works of Linnæus may be found here and there, chiefly in the city of Yedo, and though his method of description may in some cases have been followed by the Japanese, nevertheless I have never found traces of any systematic zoology, while the pictures of many mammals, said to be native in the interior of Japan, display nothing characteristic, although at a first glance you would have called them well painted, since their colouring is true to life. These same drawings of the animal

kingdom furnish another example of the peculiar mental attitude of the Japanese, since they fashion for themselves with their brush many birds and butterflies that are certainly not native nor have ever been seen. Still I admire their finer pictures of fish and butterflies drawn according to nature."

Geology is next treated of by Von Siebold. "I observed," he writes, "that the medical men whom I met daily could well distinguish the common minerals, such as Antimony and Zinc, and define them by their Dutch, Japanese, and even Chinese names. But in private collections of minerals, I have noticed, as above, that they chiefly esteem and preserve monstrosities. Hence fossil animals, fossil plants, with other stones curiously shaped by nature or chance, form the rarities of their collections." Obviously, though the Japanese collected fossils, they were not palæontologists; they even confused fragments of corals with simple stones. That they were excellent metallurgists, however, the fame of their copper, iron, brass, silver, gold, and gold alloy, is sufficient proof. They were also not ignorant of how to treat minerals by chemical operations, and made many preparations of mercury.

In conclusion, Von Siebold emphasises the desire of the Japanese for knowledge and their aptitude for investigation. He founded a medical college (which must have been in Nagasaki before he went to Tokio) and employed his students collecting specimens. Among interpreters and other natives he found "a liking and singular ability for natural history, excited partly by scientific, partly by pecuniary reasons." "In two weeks," he says, "I taught a Japanese youth the art of drying plants and all the methods of taxidermy, so that every day I more and more admired his manner of preparing specimens." Lastly, the more learned Japanese "lose no opportunity of gaining knowledge from European books."

My readers have by this time, if my attempt has been successful, gained some idea of the ground in which, when at last the fence was broken down, the seed of Western science was to be sown. And by this time they will be getting anxious to learn how the plant has flourished and what fruit it bears at the present day. This it will be the main object of the second portion of this paper to inform them.

F. A. BATHER.

(To be continued.)

III.

The La Plata Museum.

AS many of our readers are probably aware, when the city of Buenos Aires was made the capital of the Argentine Republic in the year 1880, it was at the same time considered advisable that the province of this name should have a capital of its own. Accordingly, two years later, the city of La Plata was founded on a site some thirty-five miles lower down the river than the metropolis, and inaugurated as the provincial capital of Buenos Aires. One of the reasons for selecting a site so near to the metropolis appears to have been the necessity of having a port where vessels of the largest tonnage could come alongside the wharves; and one of the first proceedings was to construct a magnificent ship-canal from the river, terminating at a spot known as the Ensenada, within easy distance of the centre of the new city, and provided with accommodation for ocean steamers of almost any dimensions. With characteristic Transatlantic energy, the planning and building of the future capital was pushed on apace; and there soon arose on the site of what had recently been a mere *estancia*, or cattle-farm, a city of lordly palaces and stately squares, which has been not inaptly designated the "enchanted city." While the "boom" which at that time was at its zenith in Argentina lasted, La Plata bid fair to become a rival of the metropolis, and population and building vied with each other in trying to get ahead. Unfortunately, these rosy prospects were but short-lived, and at the present time the princely palaces and broad boulevards of the "enchanted city" are, except during the sittings of the Provincial Parliament, well-nigh deserted; and instead of resounding with the rattle of carriages and the tramp of thronging multitudes, the paved streets are silent, deserted, and grass-grown.

Whether this state of comparative desertion and stagnation is likely to be permanent, or whether it is but the chrysalis-stage preparatory to the advent of a period of prosperity and progress, it behoves us not to enquire in these pages. There is, however, within the limits of La Plata, a noble building which must for ever render celebrated the name of that city throughout the length and breadth of the scientific world. This building, I need hardly say, is the Museum, which owes its foundation and present prosperity solely to the indomitable energy and perseverance of its able and accomplished Director, Dr. F. P. Moreno.

Recognising the importance of a proper appreciation of science in a country so richly endowed with palæontological treasures, Dr. Moreno lost no time in impressing on the Provincial Government the necessity of providing funds for the erection of a Museum which should be worthy of the palatial surroundings of the enchanted city; and, fortunately for science, his efforts were ere long crowned with the success they so well merited. Only two years after the inauguration of La Plata as a city—namely, in 1884—Government sanction was obtained for the erection of the Museum; and in 1889 the imposing edifice represented in our first illustration was practically complete. The building, we may state, is situated on the Ensenada, or river-side of the outskirts of the city, standing in a park, amid splendid avenues and groves of tall eucalyptus and other trees, which, in the course of a few years, will form a veritable forest. Since our excellent illustration (for which, like the others in this notice, we are indebted to Dr. Moreno) gives such a good general idea of the external appearance of the Museum, it will not be necessary to say much on this point; neither shall we spend much time on a description of the interior. We may mention, however, that after passing under the well-proportioned Grecian portico, the visitor, on entering the building, finds himself in a rotunda, with a gallery and roof supported by two tiers of iron columns, and lighted above by a large skylight; its walls being decorated with frescoes representing the scenery, native life, and some of the wonderful extinct mammals of Argentina. From this rotunda, which occupies the centre of the front of the building, there diverge, on the ground floor, two galleries on opposite sides, which, after running a straight course for some distance, curve round so as to form a pair of apses at the two extremities, which are again connected by a straight gallery running parallel to the one in front, both back and front galleries being connected by cross-galleries and chambers, so that the whole edifice forms a continuous block of building. The upper floor, which does not extend over the two terminal apses, contains the apartments of the Director and the Secretariat, together with the library, the art-gallery, and some portions of the ethnological section. On the ground-floor the central chambers are, in the main, devoted to anthropology and ethnology; while the galleries on the right of the entrance contain the geological and palæontological exhibits, and those on the opposite side the animals of the present epoch. The central hall, on the further side of the building opposite the rotunda, is, however, consecrated to the mastodons and sub-fossil cetaceans.

We might, of course, elaborate to any extent the description of the Museum itself, but since this is a matter of comparatively small interest, we proceed at once to the consideration of its contents. And here we may state at the outset that the most marvellous thing connected with this wonderful institution is the circumstance that nearly the whole of its unrivalled treasures have been collected within

the last few years by the untiring energy of its Director. Probably many persons in England are under the impression that the La Plata Museum and the Buenos Aires Museum (of which the palæontological contents were so admirably described by its late Director, Dr. Hermann Burmeister) are one and the same thing. This, however, is far from being the case, the Buenos Aires Museum being the National institution, while the La Plata Museum pertains to the province only. Without entering into the question whether or no the establishment of two such institutions within forty miles of one another was strictly advisable in the interests of science, we may state that at the foundation of the La Plata Museum, the National Museum at Buenos Aires was left untouched, and the collection of the former started almost *de novo*. And we may add, in no disparagement to the elder institution—which must always claim the prestige of containing the whole of Burmeister's types—that the younger sister has shot far ahead, so far as palæontological treasures are concerned.

So far as we can gather from an account published by the Director in 1890, it would appear that the principal aim of the Museum is to illustrate the whole fauna—both recent and fossil—of the Argentine Republic. It has, however, been recognised that it would be impossible to study the unrivalled series of fossil mammals without the opportunity of comparison with the skeletons of the living members of the same class from all parts of the world; and the Director has accordingly paid special attention to the acquisition of a representative series of specimens of mammalian osteology; our second plate showing the gallery where the greater number of these skeletons are exhibited. Among those of more than ordinary interest are the fine series of skeletons of South American Cetaceans, most of which have been obtained from the estuary of the Rio de la Plata and the coast of Patagonia, where several of them have been found stranded. The series includes skeletons of *Balænoptera*, *Megaptera*, *Hyperoödon*, and *Orca*, most of which have been referred to species distinct from those of the northern hemisphere; and among these the splendid skeleton of a member of the first-named genus, seen suspended on the left side of our plate, is remarkable for its large size, the total length being upwards of 22½ metres. Of still wider interest is the skeleton of an individual of *Neobalæna marginata*—a cetacean which we believe to have been hitherto recorded only from the South Seas. With the bare mention that a large number of both native and foreign animals are represented by stuffed specimens, we must bring to a close these few remarks relating to the recent section of the Museum, and proceed to the palæontological department, which is the one from which the institution will derive a world-wide celebrity.

Before setting out on my recent visit to La Plata, I had been prepared by the glowing accounts sent to me by the Director, as well as from the published writings of other palæontologists, to find the

Museum exceedingly rich in the fossil Vertebrates of Argentina; but on my arrival the reality far exceeded my most eager expectations, and during my first walk through the seemingly endless galleries of the Museum I was absolutely lost in astonishment and admiration at the number and beauty of its palæontological treasures. We find, for instance, in one of the galleries devoted to the display of the mammalian remains from the Pampean beds and the somewhat older formations of Monte Hermoso, near Bahia Blanca, two complete skeletons of *Toxodon*, while another of *Macrauchenia* lacks only a few of the hinder trunk-vertebræ. The latter genus is also represented by three complete mounted limbs; while around the walls are ranged, in almost endless number, skulls, jaws, teeth, and limb-bones of *Toxodon*, *Typotherium*, and *Macrauchenia*, belonging to individuals of all ages and sizes. Perhaps, however, the greatest treasures of this striking gallery are the skull of that remarkable Toxodont which has been described as *Trigodon*, but should properly be known as *Toxodontotherium*, and the skull and jaws of the allied *Zotodon*; the former being from the Monte Hermoso deposits, while the latter comes from distant Catamarca. It must be added that while one of the skeletons of *Toxodon* is formed from the bones of a single individual, the second has been completed from the remains of two animals. And here I may say a word in praise of the admirable manner in which all the skeletons have been mounted and the broken specimens restored by Señor Giacomo Pozzi, the Articulator of the Museum. At the present time the whole of the invaluable series of mounted fossil skeletons are arranged along the middle of the galleries without any kind of protection from injury. Since the palæontological section of the Museum is not yet thrown open to the general public, this unprotected state of the specimens does not lead to much harm, but I may venture to express the hope that when the whole Museum is opened the Government will be fully aware of the priceless value and world-wide interest of these unique specimens, and the necessary steps taken for providing suitable cases for their protection.

Leaving the Toxodont gallery (which is seen in the background on the right side of our third illustration) we pass on to a large hall containing the remains of *Megatherium*. Here we find one entire skeleton of this giant ground-sloth, while facing it is the trunk and pelvis of a second, with the greater part of the fore-limbs attached. Several mounted specimens of limbs and other portions of the skeleton occupy the centre of this chamber; and in the wall-cases are arranged numerous detached bones and some magnificent specimens of the skull, one of the latter being noteworthy on account of its enormous size. In the next gallery we come to a magnificent series of mounted skeletons of the Mylodons and their allies, the more or less nearly complete specimens being six in number (including one of *Scelidotherium*), while there is the greater part of the trunk of a seventh. These mounted specimens range in size from the gigantic *Mylodon*

armatus, which approaches *Megatherium* in bulk, and is characterised by its enormously expanded muzzle and tusk-like anterior teeth, to the comparatively small *Scelidotherium leptocephalum*, which is not very much superior to a tapir in size. In the surrounding wall-cases are displayed an almost endless array of skulls, limb-bones, and vertebræ, many of which are associated. Although all the mounted specimens are from the Pampean formation, some of the wall-cases contain examples of the skulls and bones of *Scelidotherium* from the somewhat older deposits of Monte Hermoso, near Bahia Blanca. These remains indicate species of much smaller dimensions than those from the Pampean, and thus serve to illustrate the general decrease in the bodily size of the members of the various groups of mammals as we descend from the Pampean beds through the Monte Hermoso deposits to the Santa Cruz beds of Patagonia. This decrease is displayed not only among the Mylodonts, but likewise in the Glyptodonts, and the Macrauchenias, as well as in some other Ungulates. For instance, while the Pampean Mylodonts include species equal to the largest rhinoceros in size, the Monte Hermoso *Scelidotherium* was smaller then a tapir, while *Eucholæops* of the Santa Cruz beds was not more than a yard in length, although closely allied to *Mylodon*. Again, among the Glyptodonts, we notice that some at least of the representatives of the genera *Glyptodon* and *Dædicurus* from Monte Hermoso were considerably inferior in size to their Pampean successors, while when we reach the Santa Cruz beds we meet with mere dwarfs, as exemplified by the genus *Propalæhoplophorus*, of which there is a beautifully-preserved skeleton and carapace in the Museum.

Reverting to the Pampean Mylodonts, I may mention that although the time at my disposal did not admit of my undertaking a detailed survey of any of the Edentates, yet I have little doubt that, if this were done, the number of nominal species in this particular group might be considerably reduced. My opinion has already been expressed elsewhere as to the inadvisability of subdividing the true Mylodonts into separate genera, such as *Lestodon*, *Pseudolestodon*, and *Grypotherium*; while the proposal to split up the group of Ground-Sloths into several families, instead of including the whole in the Megatheriidæ, is not likely to commend itself to English zoologists.

Perhaps the most striking display in the whole Museum is the magnificent series of the remains of Glyptodonts, which are exhibited in one half of the gallery containing the Mylodonts. Here we see not only a fine array of specimens of the carapace and tail-shield, with or without the skull and limbs attached; but likewise a number of entire skeletons without the dermal ossifications. These specimens comprise examples of the genera *Glyptodon Panochthus*, *Dædicurus*, and the one generally denominated *Hoplophorus*; and serve to show conclusively that the original Owenian restoration of *Glyptodon* was incorrect, the terminal tube of the caudal sheath of an *Hoplophorus* having been attached to the carapace of a *Glyptodon*.

May we accordingly venture to suggest to the authorities of the Royal College of Surgeons that the specimen in their Museum thus anomalously restored might now be advantageously dismounted, since in its present state it merely tends to perpetuate an accidental error? Marvellous as are all the Glyptodonts, the most astounding monster in the whole series is undoubtedly the one denominated *Dædicurus*. The total length of this monstrous skeleton, as it is now mounted, is upwards of 11 feet 8 inches measured in a straight line, while the carapace measures 10 feet 4 inches across the highest part of the back, and the length of the massive club-like terminal tube of the caudal sheath is upwards of 3 feet 11 inches. Since, so far as I am aware, there is nothing approaching to a complete skeleton of this strange creature in any European Museum, my readers will probably pardon me if I enter into a few details of its structure. It will be observed, in the first place, that the carapace is remarkable for its curiously hump-backed contour, in which respect it differs very markedly from the regularly egg-shaped shell of *Glyptodon*; while it is further distinguished by the absence of the bold conical bosses with which the periphery of the latter is ornamented. Then, again, there is a marked difference in regard to the structure of the individual plates of which the carapace is composed; for whereas in *Glyptodon* these are polygonal, with a rosette-shaped pattern formed by the impressions of the edges of the overlying horny shields, in the present form they are oblong plates of bone, with a smooth external surface, devoid of the impressions of horny shields, but severally perforated by from one to five large circular holes, through which quill-like bristles were doubtless protruded during life. The tail was protected for the first third of its length by eleven enormous bony hoops, each formed by a single ring of plates similar to those of the carapace, but two of which not unfrequently coalesce, the circumference of these hoops rapidly decreasing from the base of the tail towards its extremity. The terminal two-thirds of the tail are formed by the well-known club-like tube so frequently exhibited in European Museums. At its flattened and expanded extremity, this tremendous club bears a number of roughened, depressed, disc-like facets of an oval contour, which during life must evidently have given support to huge horny spines, probably not unlike the horns of a rhinoceros. The whole animal must accordingly have bristled with horns and quills, looking not unlike some giant porcupine. In the somewhat smaller species from Monte Hermoso, there are more of the disc-like surfaces on the tube of the tail, which also differs from that of the Pampean species by being less expanded at the end and by the presence of a number of flat oval plates on the upper surface. The two imperfect specimens of the carapace of the species of this genus from Monte Hermoso in the collection of the Museum are remarkable for having a crater-like elevation with a central perforation immediately over the point of

attachment of the pelvis. The occurrence of this peculiarity in two examples shows that this feature cannot be an abnormality; but, unless (as Dr. Moreno thinks) it be glandular, I am quite at a loss to guess its rise or object. The skull in this genus has a straight profile from the occiput to the tip of the nasal bones, in consequence of which the oblong aperture of the nose is of great vertical height.

In marked contrast to *Dædicurus* are the plates of the carapace in the allied genus *Panochthus*, which, although oblong in form, have a peculiarly roughened and particular external surface, without perforations for bristles. Occasionally, however, specimens are found showing the impress of horny shields arranged in a rosette-like pattern somewhat after the manner obtaining in *Glyptodon*. The tail of *Panochthus* differed from that of *Dædicurus* in that the terminal tube was less flattened, and not expanded, while it was covered with granules interspersed with disk-like surfaces, which were prominent instead of depressed, and thus evidently bore a different type of spine. More striking is the remarkable difference in the form of the skull, which had a highly vaulted profile, narrow, oblique nostrils, and an enormous descending process to the zygomatic arch. One specimen of the skull has its dermal covering of bone still preserved; each plate consisting of a smooth central disc surrounded by granular bone. Of this gigantic creature, which rivalled *Dædicurus* in bulk, although with a smaller tail, the Museum possesses in addition to several unmounted specimens, one entire carapace, three complete mounted skeletons, exclusive of the carapace, but with the terminal tail-tube, another lacking the tail, as well as a portion of a fifth.

In addition to the peculiarities in the structure of the component bony plates of the carapace already referred to, *Glyptodon* differs from both the genera above-mentioned by the form of the tail-sheath, which is composed of a number of rings, gradually decreasing in diameter, and ornamented with a series of conical knobs, the extremity consisting of a short cone similarly decorated; the similarity of this tail-sheath to that of the extinct Australian tortoise *Miolania* being not a little remarkable. The skull of this genus has neither the straight profile of *Dædicurus* nor the convex one of *Panochthus*, but the frontal and parietal planes meet one another at an obtuse angle, thus causing the nasal aperture to be wider than long. One skull from the Pampean and a second from Monte Hermoso have the bony dermal shield preserved, and show that it was composed of small juxtaposed plates, which become larger and imbricating on the occiput. Of the Pampean forms, which apparently belong to two species, the mounted series in the Museum comprises two skeletons and nearly a score of more or less nearly complete carapaces. There is also a fine series of the remains of the smaller species from Monte Hermoso.

In the genus which we may provisionally allude to under the name of *Hoplophorus*, the skull, as is well shown in a beautiful

example of the entire skeleton, is very different from that of all the allied genera, its chief peculiarity being the curiously-incurved form of the nasal bones, and the consequent involution of the nostrils. The head-shield was long, curved, and smooth, with the largest plates posteriorly. Three specimens of the carapace of this genus are mounted in the gallery; one of these being associated with the head-shield and tail-sheath. The species of this genus from the Monte Hermoso beds appear to have been fully as large as their Pampean representative.

Although I have at present had no opportunity of going into the question of the number of species of Glyptodonts represented in the Pampean formation, I have not the least hesitation in saying that a large proportion of those named on the evidence of specimens of the terminal tail-tube or fragments of the carapace will be found invalid. The same is doubtless true with regard to several of the so-called genera founded on remains from the older formations of southern Argentina. Although it is not my intention to enter here into the consideration of the Glyptodonts of the Santa Cruz beds of Patagonia, I may mention that one of the greatest treasures of the Museum is the entire skeleton and greater portion of the carapace of the dwarf *Propalæhoplophorus*, to which incidental allusion has already been made. As shown both by the conformation of the skull and tail-sheath, these dwarf Glyptodonts were much more nearly allied to *Glyptodon* than to either of the other Pampean genera, all the latter being probably, therefore, more specialised types of later origin.

Although in the Pampean formation the short-snouted Glyptodonts appear to have been the dominant types of the Loricate Edentates, it must not be inferred that the long-snouted Armadillos were absent. While some of these forms found in the deposits in question were more or less closely allied to, or even identical with, their living cousins, others, like *Eutatus*, were of much larger size, and differed by having the whole carapace formed of movable bands. Still larger was a recently-discovered armadillo, for which the name *Dasypotherium* has been proposed; the skull of this giant measuring upwards of 10½ inches in length. Remains of the existing genus *Dasypus* are not uncommon in the Pampean, and the Museum possesses some beautiful examples from Monte Hermoso, one of which shows both the skull and carapace. I may mention here that a species of *Dasypus* allied to the existing *D. minimus* (which it has been recently proposed to separate generically as *Zaëdius*) occur in the Santa Cruz beds; a fact which has an important bearing on the geological age of the latter.

Before leaving the mammals of the Pampean beds, I must not omit to mention the fine series of equine remains contained in the Museum; although in regard to one or two species, the La Plata collection is inferior to that of the Buenos Aires Museum. Among the more notable specimens, I may refer to a skeleton of an *Equus*, which has been assigned by Señor Ameghino to a so-called species

which he terms *E. rectidens*, but which, like the other specimens so-named, I see no reason for separating from *E. curvidens* of Owen. Although possessing no complete skulls, like those in the Buenos Aires collection, the La Plata Museum also contains a fine series of the remains of those horses which have been generically separated under the name of *Hippidium*. Hitherto I have not considered this separation justifiable, but from the study of the actual specimens, I am now convinced that it will be convenient to regard the extreme backward elongation of the nasal slits characterising these extinct horses as a feature of generic value. And here I may mention an instance of that want of appreciation of differences due solely to individual peculiarities and variation in age which unfortunately characterises so much of the palæontological work of Ameghino. On the evidence of a single lower equine molar from the Parana, that gentleman proposed to establish a genus stated to differ from other Equidæ by the almost total absence of folds of enamel in the cheek-teeth, for which the name of *Hipphaplus* was suggested. As a matter of fact, this tooth is nothing more than an extremely worn molar of *Hippidium*, as is conclusively proved by a large series of Pampean specimens in the Museum, which exhibit a complete transition from the unused to the much-worn type. To make matters worse, when the error was pointed out by Burmeister, the founder of the so-called genus deliberately set to work to justify his own views, instead of frankly acknowledging his error. The existence of a genus closely allied to *Equus* in the Parana beds, which are set down as of Lower Oligocene age, would not, however, by any means have suited the views of the Argentine palæontologist, and hence *Hipphaplus* must be maintained at all hazards!

The third genus of South American equines is represented by a very remarkable skull recently discovered in the Pampean deposits on the coast of the province of Buenos Aires, and first described by Dr. Moreno under the name of *Onohippidium*. While agreeing with the skull of *Hippidium* in the extreme elongation of the nasal slits, this specimen is distinguished by the presence of an enormous lachrymal fossa of an oblong form and of great depth. Although it is certain this fossa must have contained a large lachrymal gland, its size is much greater than in any other mammal, either living or extinct, with which I am acquainted. The cheek-teeth were of the general type of those of *Hippidium*.

(*To be continued.*)

R. LYDEKKER.

IV.

Note on the Air-Sacs and Hollow-Bones of Birds.

MR. F. W. HEADLEY, in his interesting paper in the November number of NATURAL SCIENCE (vol. iii., pp. 346-356), calls attention to the fact that while the Gannet and the Cormorant belong to the same order, the one has large air-sacs and a remarkably pneumatic skeleton, while the other is devoid of air-sacs and has practically non-pneumatic bones. I have said practically non-pneumatic, because in *Graculus carbo* and related forms, the humerus and sternum have small pneumatic foramina, while in the extinct *Phalacrocorax perspicillatus* and other grooved-billed species these bones are quite non-pneumatic. Mr. Headley suggests that light bones would be out of place in, or unserviceable to, diving birds, and this is undeniably the case, since the problem with them is how to keep under water.

While the solid body and dense bones of the Cormorant are directly correlated with its mode of life, the air-sacs and pneumatic skeleton of the Gannet appear to be equally good adaptive characters. The Cormorant pursues its prey beneath the water, the Gannet plunges upon its victims from above, dropping upon them headlong from heights of one to two hundred feet. Now, as the cunning mechanic has devised an air-chamber to break the accidental fall of an elevator (American for lift), so nature has applied about the neck and breast of the Gannet a series of air cushions to break the shock which occurs when a bird weighing six or eight pounds strikes the water after a drop of a hundred feet or so.

In the Brown Pelican (*Pelecanus furcus*) there are many air-cells present about the breast forming a thick mass of loose areolar tissue located exactly where they would do the most good as a buffer when this big bird plumps (dashes is hardly the word to use in connection with a pelican) down upon a shoal of fish. I have never dissected any of the white pelicans, and as these birds fish in quite a different manner from their darker relatives, it would be interesting to know if there is any corresponding difference in the arrangement or number of the air-sacs.

Another point possibly served by the air-sacs, as well as by the pneumaticity of the bones, is that of equalising the pressure of the air when a bird either dives from air into water, as does the Gannet,

or rapidly descends from a great elevation in the air, as do some of the vultures. Soaring birds, such as the vultures, screamers, and some storks, certainly have very pneumatic bones; but I do not pretend that the above theory accounts for the fact, it is merely a suggestion as to one use of the character, and while the theory works very well when applied to such birds as the Gannet and Vulture, it fails in the case of the hornbills. Here, perhaps, we may fall back on the very convenient and much-abused theory of an inheritance from some active ancestor, but this again is merely a suggestion.

It is somewhat curious that of two nearly related birds one may have certain bones pneumatic, and the other non-pneumatic, a point well illustrated by the titmice. It is, too, a general rule that animals of rapid movements have light bones, and sluggish or aquatic animals non-pneumatic skeletons; and aside from the gain in size without increase of weight, which is rendered possible by a hollow bone, may there not be an actual oxygenation of the blood in the bones?

FREDERIC A. LUCAS.

V.

Cell-division.

ALTHOUGH the main general features of cell-division are familiar to most readers of NATURAL SCIENCE, yet it is probable that not a few have been unable to find time to keep pace with even a part of the ever-increasing literature on the subject. So many varied, and in some cases contradicting, statements have been made recently, as to the real nature of the processes of cell-division, that any attempt to summarise them as probable truths is necessarily accompanied with considerable difficulty, and a likelihood to prove unintelligible or misleading. Nevertheless, it is hoped in this article to give some account of the chief features of ordinary cell-division looked at in the light of the latest researches. Ever since Strasburger, Bütschli, and Fol in 1870 drew attention to the extraordinary changes which generally take place in the nucleus of a dividing cell, a large army of investigators have given their almost undivided attention to increasing our knowledge of the subject, and it is a matter of no small regret that in such a wide field of research, which affords so many chances of gaining deeper insight into some of the most striking phenomena exhibited by living matter, few, if any, English names stand pre-eminent.

A cell may divide either (*a*) mitotically, that is to say, may pass through a process of change known as mitosis or karyokinesis, whereby a very perfect division of the chromatin, *i.e.*, the deeply-staining portion, in the nucleus, is brought about, or (*b*) amitotically, where a rough-and-ready splitting of the nucleus is substituted for the more elaborate mitotic division. The former is by far the more usual of the two, and although there are undoubted cases where amitotic division is the rule, it is probable that karyokinesis is even of wider occurrence than it is thought by many to be. It is with this form of nuclear division that the present contribution exclusively deals, and it will be found convenient to study the subject from the following points of view, viz.:—

(1.) The general features of karyokinesis.

(2.) The origin, nature, and function of the component parts of the nuclear figure.

(3.) The relation of karyokinesis to fertilisation and segmentation.

(4.) Theoretical considerations.[1]

The General Features of Karyokinesis.

In Figs. 1-8 (p. 43) the nucleus of a cell is represented in the various phases through which it passes during karyokinesis. In Fig. 1 the nucleus is in the resting condition, the chromatin being in the form of a much-coiled thread. The first preliminary stage towards division is marked by the thread acquiring a sharper contour, due to the withdrawal of numerous small branches and processes. It then proceeds to break up into a number (eight in this case) of pieces, Fig. 2. Each of these splits longitudinally into two exactly similar halves, forming in all sixteen daughter chromosomes (*Chr.*), as the masses of chromatin are now called, Fig. 3. The nuclear membrane at this period becomes indistinct and gradually disappears, and a new body comes into play, viz., the centrosome. Whatever may be its origin, the centrosome, owing to the active part it takes in the next stages, must be noticed at this point, though it will be dealt with in detail later on. A centrosome is thought by many to be an integral part of, and therefore present in, every cell during the resting as well as the active state. Since, however, it is unaffected by ordinary staining reagents, its presence is often difficult to demonstrate. At the moment when the nuclear membrane begins to disappear, the centrosome becomes surrounded by a number of radiating strands of protoplasm (Fig. 4, *c.*), which form the so-called "archoplasmic sphere." The centrosome then divides into two, each half remaining attached by means of numerous threads (*C. Sp.*). These threads are, in this case at any rate, the beginning of the future nuclear spindle, although in other cases a different origin has been assigned to it, as will be seen when the spindle is dealt with by itself. The next stage is represented in Fig. 5. The centrosomes have greatly increased their distance from one another, owing to the rapid enlargement of the spindle. The chromosomes (*x.*) have grouped themselves round the spindle in the median plane, to form the so-called equatorial plate. They have, furthermore, become doubled, each pair on itself, in such a way that the angle of the bend is directed towards the spindle, while the two ends point towards the surface of the cell. The next two stages are shown in Figs. 6 and 7. The two daughter chromosomes of each original chromosome gradually recede from one another and move along the spindle, until they come to lie, one set at one pole of the spindle and the other at the other. Nevertheless, they are not as yet entirely separated from one another, but still remain connected by delicate threads (Verbindungs Fasern, Hertwig) formed from the so-called "lâme intermédiaire" of Van Beneden. These must not be confounded with the threads comprising the spindle, from which they are quite distinct.

[1] It has been found possible in this article to treat of only the first two of these headings.

The condition shown in Fig. 7 is called the dyaster. In Figs. 5–8 only half the number of original chromosomes have been drawn. The last phase (Fig. 9) is marked by the disappearance of spindle, centrosomes, archoplasmic spheres, etc., and a return to the resting condition, accompanied by division of the body of the cell. At the moment when, in the dyaster stage, the archoplasmic spheres have attained their maximum development, the division of the cell-body commences. This usually takes place by a nipping of the cell transversely to the axis of the spindle. The furrow grows deeper and deeper inwards, until it cuts the cell into two nearly equal halves. As soon as this is accomplished, the archoplasmic spheres begin to disappear, and the chromosomes rapidly pass into the resting condition, apparently by imbibition of fluid. This stage (Fig. 10) has been termed the dispirem The chromosomes eventually break up into small grains, and a vesicular nucleus is formed, with the chromatin substance distributed as a network or coiled thread. This passage of the chromosomes from an active to a passive condition has been followed out most carefully by Boveri, who has shown that they preserve their individuality throughout the whole of the resting stage. The nuclear membrane is apparently formed by a concrescence of some of the small grains of chromatin into which the chromosomes have broken down.

The above facts may be looked on as being typical of all forms of karyokinesis, as far as regards the behaviour of the chromosomes; in the cells of different animals, however, these may vary considerably in number and appearance, from a few large threads, as in the above instance, to a large number of more or less irregular granules. Furthermore, although an exact splitting of each chromosome of the nucleus of the parent cell has not been definitely observed in all cases, especially where the chromosomes are small and numerous, it may be almost certainly assumed that some sort of process of this kind invariably does take place, either during the resting stage or when the cell is undergoing karyokinetic change. Before going on, however, to more debatable ground, where not only the interpretation of the facts, but even information as to the facts themselves, varies as we study the works of one author or of another, certain abnormalities observed in karyokinesis deserve a passing mention. It has been found that cell-division can be influenced in a remarkable way by varying the external conditions under which it is placed. The brothers Hertwig have given an account of some experiments performed on the eggs of the sea-urchin, *Strongylocentrus*, of which the following are the most remarkable.

Intense cold, *e.g.*, 2° C., has been shown to destroy all parts of the nuclear figure excepting the chromosomes. Dilute quinine appears to have an extraordinary effect on the centrosome, as a result of which it divides into four or five, each surrounded by its archoplasmic sphere, and consequently the figure may have four or five polar stars

instead of two. Perhaps, however, the most curious of all these pathological cases are those of ova which have been in some way enfeebled, and so are unable to prevent the entrance of more than one spermatozoon, a phenomenon which in these forms is abnormal. Many of these spermatozoa come into contact with the egg nucleus. There results a karyokinetic figure having three, four, or many spindles and consequently double as many polar stars as there were spermatozoa which reached the egg nucleus. Furthermore, those spermatozoa which have entered the ovum but not come into contact with the egg nucleus, may also divide karyokinetically, and combine together to form figures and patterns of most varied kinds.

The Component Parts of the Nuclear Figure.

It is now necessary to consider more in detail the various elements which go to make up the nuclear figure. From the foregoing account it will be seen that the parts of a karyokinetic figure can be tabulated in the following way:—

Nuclear figure	Achromatic elements.	Centrosomes. Archoplasm. pheres. Spindle. Lâme intermédiaire.
	Chromatic ,,	Chromosomes.

Centrosomes and Archoplasmic Spheres.—Although the so-called "Polkörperchen," or "centrosome," was discovered as early as 1870, by Fol and others, to exist in the centre of the archoplasmic spheres during the division of the nucleus, yet even at the present time opinions vary considerably as to its real origin. Van Beneden, Boveri, and indeed the majority of observers, look upon the centrosome as a permanent organ of the cell-body, present during the resting as well as active stage, and entirely external to the nucleus. Their views are to some extent borne out by the fact that in several instances, *e.g.*, epithelial cells of fishes and cœlomic corpuscles of Nemertines, an undoubted centrosome has been found during the resting stage in close proximity to, but perfectly distinct from, the nucleus. On the other hand, in few, if any cases, has the centrosome been actually traced through the stages of karyokinesis into the resting condition. O. Hertwig, on the other hand, is opposed to this view, and maintains that the centrosome is in every case essentially a nuclear structure, for the following reasons: (*a*) it has not been demonstrated, save in a few exceptional cases, as existing in the body of the cell during the resting stage; (*b*) its appearance is usually simultaneous with the breaking up of the nuclear membrane; (*c*) it has been observed to appear first at the edge of the nucleus, and then gradually wander into the body of the cell.

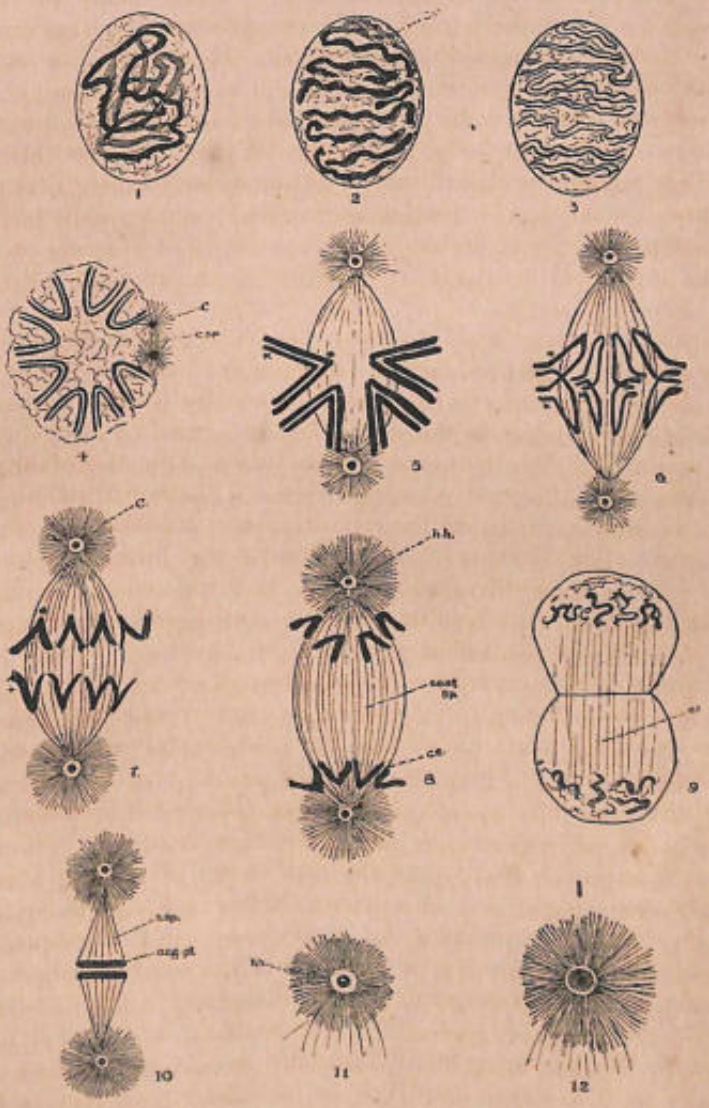
Although the opinion of Hertwig is contrary to that of most authorities, yet Brauer in Germany, and Moore in this country, have recently (September, 1893) published accounts of spermatogenesis, the former in *Ascaris megalocephala*, the latter in *Branchipus*, in

which they have apparently conclusively demonstrated the nuclear origin of the centrosome.

Mr. Moore has been able to observe the actual formation of two distinct centrosomes, by fusion of bodies he calls dictyosomes, which apparently are parts of the achromatic reticular network. If Mr. Moore's conclusions can be applied to other cases, many previous existing ideas as to the nature of the centrosome will have necessarily to undergo considerable modification. Meanwhile, his results must be looked upon as forming an isolated exception. Brauer, on the other hand, considers the centrosome to be always present in the nucleus as a permanent body, which may or may not pass through the nuclear membrane preparatory to setting up karyokinetic division. As will be seen later on, in discussing the possible phylogenetic history of the centrosome, it is probable that the views of Hertwig on the one side, and Boveri on the other, may be equally true for different cases.

Peculiar interest was aroused when it was shown that the "Mittelstück," *i.e.*, that part which separates the head and tail of the spermatozoon, was really a centrosome, taken by the spermatozoon into the egg as an organ of division, a fact supported by all the more careful researches. Furthermore, Fol has shown in the case of the egg of an echinoderm, that the sperm-centrosome (*i.e.*, the "Mittelstück") and egg-centrosome take up their position one at either end of the first segmentation nucleus in the region of the future equatorial plane. Each then divides into two, and the halves travel in opposite directions, thus resulting in four centrosomes, one lying at each corner of the somewhat square-shaped nucleus. Each half of the original sperm-centrosome fuses with half of the original egg-centrosome, thus forming two centrosomes, both of which are partly paternal and partly maternal. This done, segmentation commences in the ordinary way. Although the above process takes place in one case at any rate, it is by no means so certain that it occurs in the large majority of instances which have been studied. On the contrary, it has been found that the centrosome of the egg almost invariably degenerates and disappears before the sperm-nucleus comes into contact with that of the ovum, and consequently every centrosome in every cell of the body of the resulting organism is derived from "Mittelstück" of the spermatozoon. The view that this egg-centrosome is typically a rudimentary organ was first proposed by Boveri, and his conclusions have been verified by Vejdowsky for *Rhyncelmis*, and Fick in his admirable paper on the maturation and fertilisation of the egg of *Axolotl* (*Ann. Mikr. Anat.*, Nov., 1893).

As to the explanation of these facts, it would seem probable that the degeneration of the egg-centrosome takes place in order to prevent parthenogenetic segmentation of the ovum. This supposition may appear absurd to those who look upon the egg nucleus after the giving



FIGURES OF THE CELL-NUCLEUS DURING KARYOKINESIS.

off of the polar bodies as thereby incapable of further division without additional chromatin elements derived from the spermatozoon. Boveri, however, has shown that, in certain cases, the spermatozoon may remain in the periphery of the ovum instead of travelling towards the egg nucleus. The sperm-centrosome, however, does not likewise remain inactive, but approaches the egg nucleus and divides into two. The egg nucleus then divides karyokinetically, and with it the whole body of the ovum. The spermatozoon only comes into activity when the 4- or 8-celled stage has been reached, when it fuses with the nucleus of that blastomere in which it happens to lie. In this case it is obvious that the egg nucleus, even after giving off the polar bodies, has sufficient chromatin left to enable it to divide and reproduce the organism, but at first lacks the organ whereby that division can be brought about. Many facts of this description could be adduced to prove that the presence of a centrosome is necessary for division to take place, and since it has been shown that a spermatozoon can "fertilise" a non-nucleate fragment of ova, and develop into larvæ of exactly the same species as the animal from which the spermatozoon was taken, the following important conclusion must be drawn as applicable to most cases:—A mature ovum and a mature spermatozoon have both sufficient nuclear matter for the production of a new individual, but cannot do so independently—the ovum because it lacks a centrosome, the spermatozoon because it has not the requisite quantity of protoplasm. The object of fertilisation is therefore that each may supply the wants of the other—the fertilised ovum possessing both a centrosome and a sufficient quantity of protoplasm. The advantage of additional chromatin elements derived from the spermatozoa will be dealt with later.

As to the nature of the archoplasmic sphere little need be said. It appears to be composed of radiating strands of protoplasm, which, however, do not arise from the centrosome. They do not probably come at all into contact with it, being separated from it by the so-called "heller hof." How they are formed and what is their meaning is at present very little understood.

Turning now to a consideration of the probable phylogenetic history of the centrosome, the presence of a macro- and micro-nucleus in many Ciliata at once suggests possible homologies. It is unnecessary to go into the details of the nuclear changes which take place in conjugating Infusoria, so admirably worked out by Maupas, for it will be remembered that the micro-nucleus only is the active and important structure corresponding to the nucleus in the cells of the metazoa, the macro-nucleus merely degenerating, to be formed afresh by division of the reconstructed micro-nucleus. Can the centrosome be homologised with either of these? This, of course, is very doubtful; but if any homology is to be drawn, it would appear probable that the centrosome is more nearly related to the macro-nucleus. This may seem improbable, since it is the view of most

authorities that if any comparison can be made it is between the centrosome and the micro-nucleus. Some recent researches, however, on the ova of vertebrates and arthropods have revealed the fact that the centrosome of the ovum, although functionless, is relatively of enormous size, and remains for a considerable time, even until the embryo has reached an advanced stage in development. Later on it disappears. If the macro-nucleus of a ciliate protozoon be looked upon as a centrosome which, like the one just described, has lost its power of setting up nuclear division, and is merely a rudimentary organ, it can be homologised, or, at any rate, closely compared with the centrosome of an ordinary ovum, which, as a rule, degenerates and disappears. Hence, whether the centrosome be considered as a nuclear organ or as belonging solely to the cell-body, it matters little; for it is easy to imagine the former case to be the more primitive condition, and, from evidence gathered from different sources, we are almost compelled to allow that both conditions actually do occur. The following table may, therefore, be drawn up as representing the probable phylogenetic history of the centrosome.

(1.) A most primitive condition, where one nucleus only is present, which seems to possess not only the hereditary chromatin matter, but also power of spontaneous division, *e.g.*, *Amœba*.

(2.) A condition where the primitive nucleus becomes differentiated into two; one, the micro-nucleus, possessing the hereditary substance, the other, corresponding to the macro-nucleus, which functioned as an organ of division. Probably the two nuclei were of more equal size than are the macro- and micro-nuclei. Unrepresented as far as is yet known.

(3). The macro-nucleus has dwindled to become a centrosome. *E.g.*, Spermatozoa, tissue cells, and ova where the centrosome is practically active.

(4.) The centrosome is rudimentary and disappears, to be replaced by that brought in by the spermatozoon; thus the primitive condition of one nucleus is for a short time regained between the giving off of the polar bodies and fertilisation. This nucleus, however, has not the power of spontaneous condition. *E.g.*, inert ova.

If these are the probable stages in the phylogenetic history of the centrosome, it will be seen that the condition obtaining in such forms as *Paramœcium* is an offshoot of No. 2. The macro-nucleus, after losing its function as a centrosome, has probably increased largely in size and become a storage nucleus. The micro-nucleus has taken on double duty, so to speak, *i.e.*, it is not only the bearer of hereditary substance, but is also capable of spontaneous division.

It is by no means illegal to make this supposition of a change of function for the macro- and micro-nuclei respectively, since such a phenomenon is of constant occurrence in the animal kingdom. In the afore-mentioned cases of vertebrate and arthropod ova, a most primitive condition seems to exist. Although the macro-nucleus or

centrosome has lost its function, it retains its original size and shape, and the fact that it surrounds itself with an archoplasmic sphere, points to some of its original power being still left. Furthermore, there can be little doubt that, whether we homologise the centrosome with the micro- or macro-nucleus, it was originally derived from the single primitive nucleus. Where it appears to come out of the nucleus before, and to go back again after, karyokinetic division, it is merely a recapitulation of its past history. A point in advance has been gained by those cells where the centrosome remains permanently outside the nucleus.

No mention has been hitherto made of the remarkable observations of Herr Lauterborn on living diatoms, an account of which appears elsewhere in this number of NATURAL SCIENCE (p. 7). His results are so at variance with all previous known facts of karyokinetic division, that at present it seems safer to draw no conclusions until more confirmatory evidence has been obtained. Besides, the mechanics of the processes which he describes are completely unintelligible.

The spindle.—What has been said with regard to the nuclear or non-nuclear nature of the centrosome applies equally well to the spindle. It appears to be formed by the activity of the centrosomes. Where the latter remain permanently in the cytoplasm, the spindle would appear to be made up of strands of protoplasm resembling those which make up the archoplasmic spheres appearing between the two daughter centrosomes. Where the centrosome has an intranuclear origin, the spindle is undoubtedly formed from the achromatic portion of the nucleus.

Furthermore, two kinds of spindle must be distinguished, viz.:—(1) The so-called "central" spindle; (2) the spindle which is made up of two "half-spindles." An instance where the former obtains is that taken as a typical case of karyokinesis. The second kind of spindle is shown in Fig. 9. It will be seen that it is discontinuous in the equatorial plane, the fibres merely extending from the archoplasmic rings to the chromosomes. The function also of the two seems to be different. The two half-spindles appear to exert a pulling on the chromosomes, whereas the central spindle is probably a groundwork in which the chromosomes move by their own activity. Van Beneden derives the spindle from both the cytoplasm and the achromatic network of the nucleus.

It would, however, seem probable that, where the spindle is derived from the protoplasm of the cell (as it appears to be so in those cases where the centrosomes are permanently extra-nuclear), it takes the form of two half-spindles; but where it is formed from the achromatic network in the nucleus, it is of the "central" type. This point has not yet received sufficient attention. It may, moreover, be urged against the assumption of two different kinds of spindle, that the supposed existence of the central spindle is due to a wrong con-

ception of the "lâme intermédiaire" as being part of the spindle. On the other hand, it is possible that the two half-spindles are merely the rays of the archoplasmic spheres, and in no way to be compared to the central spindle. These points obviously still require considerably more light to be thrown upon them.

The lâme intermédiaire.—This name was given by Van Beneden to the layer of achromatic substance between the split halves of the chromosomes. It consists chiefly of delicate fibrils (Verbindungs Fasern) which lengthen out as the chromosomes increase the distance from one another. Brauer has attempted to distinguish between these fibres and those that form the central spindle, on the ground that the latter are actively contractile while the former are merely connecting threads, but whether this is so or not, there can be little doubt that these connecting threads are formed from some portion of the achromatic network of the nucleus, even in those cases where the spindle may have an extra-nuclear origin. In some cases they appear to persist longest of any of the elements of the karyokinetic figure, remaining visible even if the daughter cells have become completely cut off from one another. The chromatic elements of the nuclear figure will be dealt with in a future paper, but there are two points which should here be noticed, viz.: (*a*) The mechanics of karyokinesis. (*b*) The fate of the nucleolus.

(*a*) *Mechanics of karyokinesis.*—More, perhaps might be written on this part of the question than on any other, but it would be almost entirely of a speculative nature. Nothing is really known as to how the chromosomes move towards the poles, although it is the opinion of many that the threads of the spindle are contractile, and so pull the chromosomes apart. Exactly the contrary is the view of Strasburger for plants, and Watase for the cells of embryo Cephalopods. The former believes that the threads of the archoplasmic spheres penetrate through the porous nuclear membrane, forming two half-spindles. The movement of the chromosomes, he considers, are due partly to their own independent activity, and partly to the fact that they are pushed towards the poles by swelling up of the "lâme intermédiaire" due to the osmosis of nuclear fluid.

Watase, on the contrary, although he believes with Strasburger that there is no central spindle, but only two half-spindles, considers that the threads are stiff, like bristles, and flatten out the nucleus into an equatorial plate. By this means they gradually push the chromosomes between one another, eventually carrying one set to one pole and one to another, the membrane of the nucleus being preserved during the whole process. This view, which Watase thinks to be of general application, seems somewhat fantastic; yet, at the same time, it must be allowed that his figures strongly suggest such a process. It has recently been suggested that the centrosome is by no means a

definite structure, but is merely an expression of the peculiar substance in the archoplasm, and has attractive power on the chromosomes. Ed. van Beneden considered that the spindle threads were contractile, and fixed into the centrosome as a fulcrum. More careful examination has shown that the centrosome is invariably separated from the striated archoplasmic sphere by a light area ("heller Hof" of Boveri), in which there are no fibres or striations. It is, therefore, best to dismiss the idea of contractile threads, and to consider that the chromosomes move by virtue of their own activity along the threads of the spindle. That they do so under some sort of chemiotactic influence derived from the centrosome is also very probable, nay, almost certain. Fick has, in his paper on the fertilisation of the egg of the axolotl, shown that the spermatozoon, after entering the ovum, turns completely round, so as to bring the "Mittelstück" foremost in the direction of the centre of the ovum. The latter proceeds towards the egg nucleus, while the former follows a short distance behind by means of amœboid movements. If in this case a centrosome can apparently exercise such a strong attractive power on the sperm nucleus, which is nothing more than a mass of chromatin, why should it not have a similar influence on the chromosomes in a nuclear division?

As opposed to this view it may, however, be asked, What is the use of the spindle if the centrosome can act so powerfully on the chromosomes? In answer to this, it is possible to imagine that, owing to the necessity for perfect halving of the chromosomes, a definite path is necessary for each chromosome to move along in order to prevent confusion. Haecker believes that he has discovered the actual product which acts chemiotactically on the chromosomes. Figs. 11 and 12 show this. Fig. 11 represents a centrosome surrounded by its archoplasmic sphere at the time when the equatorial plate is just being formed. It will be noticed that the "heller Hof" (*hh.*) is large, and that the centrosome has somewhat of a vesicular appearance. Fig. 12 is drawn at the moment when the chromosomes have begun to migrate towards the poles. It will be seen that the centrosome now appears as a homogeneous granule, while between it and the "heller Hof" is a zone of a staining semi-fluid consistence. This Haecker believes to be the contents of the centrosome, which are thrown out, and exert the attracting influence on the chromosomes.

Haecker's work was chiefly carried out on Cladoceran eggs. His views must be considered as yet to be purely speculatory, especially as they do not appear able to fit in with facts described by other observers.

(*b*) *The fate of the Nucleolus.*—The word "nucleolus" has, unfortunately, to do duty for at least three distinct components of the nucleus, viz.:—(1.) The true nucleoli, which appear to be part of the achromatic network, and are unaffected by ordinary staining fluids.

It has been suggested that the centrosome is formed by a concurrence of two or more of these nucleoli. (2.) Lumps of chromatin which may be numerous, few, or even represented by a single mass. The fate of these structures is quite easy to follow out. They break down to form the chromosomes. (3). The so-called "germinal spots" which exist in the ova of many animals. They may be either masses of deeply-staining nuclear substance, with or without vacuoles; there may be one large germinal spot consisting of two distinct parts, a smaller deeply-staining moiety, and a large, more vesicular portion. The smaller of these nucleoli appear to run together to form one larger one, in which the vacuoles are rhythmically contracting. Thus, in some cases at least, the germinal spot probably acts as an excretory organ to get rid of the waste products of the actively-growing chromosomes. The vacuoles run together into one large one, which then bursts. The whole process has been watched in living eggs, and occupies from four to eight hours for each systole and diastole. What the actual fate of this kind of nucleolus is, is not yet certain, although it has been observed to maintain its individuality for some time after the dissolution of the nuclear membrane. It then suddenly disappears, and is, on this account, possibly to be looked upon as chromatin which has taken on a special function, and consequently become useless as hereditary substance. Hence its apparent dissolution when the nucleus passes into the active condition.

These, then, are the main facts of Karyokinesis. It is hoped, in a future article,[2] to discuss its important relations to those theories of Heredity which locate the hereditary tendencies in the chromosomes of the nucleus, and also to compare Mitosis with Amitosis, which latter, in all probability, was the most primitive form of cell-division.

M. D. Hill.

[2] With that article will be given references to the more important papers bearing on the whole subject.

VI.

Recent Researches on Olive-Brown Seaweeds.

ALTHOUGH the nature and functions of the contents of the conceptacles of the Fucaceæ were well-known previously, there was no definite account of the development of these bodies themselves until 1880, when the first thorough examination of the subject was made by Professor Bower (1). He gives the views of earlier writers, and tells us that Reinke looked on the bodies variously called Fasergrübchen, cryptostomata, vegetative and neutral conceptacles (simply conceptacles full of hairs but without antheridia or oogonia) as the "type of these structures" originating "by a separation of four or five neighbouring cells of the limiting tissue from one another. He compares this process with the formation of the resin-passages in the Coniferæ."

It is interesting to see that Luerssen published four years later a different view, and one nearer the truth, though he does not describe the development of the conceptacle in any detail.

Professor Bower next made an investigation of the subject, and described the whole development of the Fucaceous conceptacle in all its detail. He showed that "the division of the outer cell of the limiting tissue of the thallus by vertical walls into four, ceases in certain cases. The division by walls parallel to the surface, however, continues. A linear series of cells is thus formed which may be traced some distance into the tissues, but which is terminated by a single cell only. Later, the activity of division in the horizontal direction also ceases, and as the terminal cell of the series does not increase in size, the result is that it is surpassed by the tissues surrounding it. The terminal cell of this series we may call the 'initial' cell of the conceptacle, the cell immediately beneath it may be termed the 'basal' cell."

The development of both cryptostoma and conceptacle is shown by Professor Bower to be the same, and this fact has, of course, given rise to theories as to which was the earlier of the two in the history of the Fucaceæ. Professor Bower takes the view that the cryptostoma is an abortive conceptacle, and proposes the term "neutral" or "sterile" conceptacle. Oltmanns (2), on the other hand, takes the view that the cryptostomata are the older of the two, and have in course of time come to bear oogonia and antheridia; while I (3)

have ventured, in describing the anatomy of the genus *Turbinaria*, to take a third view, viz., that both conceptacles and cryptostomata are of equal antiquity, and that neither is an outcome of the other.

For some reason that I hardly understand, all these views, and apparently mine in particular, have supplied a source of innocent merriment to Mr. George Murray, who, both in the *Journal of Botany* and his *Phycological Memoirs*, has treated the "ancestors of the Fucaceæ" as if they came from Wardour Street. As will be seen later on, fate has been so ironical that his researches have thrown fresh light on the whole dark subject and possibly a way out.

The matter was in this stage when, as Mr. Murray has since shown, an important side-light was thrown on the subject by the investigation of the development of the conceptacles of *Splachnidium*. This strange-looking plant had been often mentioned and described, but never properly investigated, till Mr. Murray, suspecting that it might prove interesting in connection with the question of cryptostomata, handed it over to Miss Mitchell and Miss Whitting for examination. The result (4) exceeded his anticipations. The first point of interest was the strange persistent apical cell which the authors describe in the following way :—

"The apical cell in *Splachnidium rugosum* differs entirely in appearance from the surrounding meristem; it has a pear-shaped body bounded by a thick transparent mucilaginous wall, and a filiform basal process stretches towards the interior of the thallus. This cell and the very active surrounding meristem are sunk in a slight depression; the two outer layers of the meristem by radial division form the epidermal cells, which continue to divide radially until the part of the thallus in which they lie has reached maturity."

. . . . . .

"The apical cell persists throughout the life of the plant, and not only does it appear at the apex of the main branch, but cells of the same kind are found at other points where growth is taking place, *e.g.*, in the young conceptacles and at the point of formation of lateral branches. It seems as if its appearance were the herald of every local development."

Except for a slight difference in the casting of external layers of the mucilaginous cell-wall, and in the junction of the process at its base with the filaments of the thallus, the initial cell of the conceptacle was found to resemble in every particular the apical cell described above. Former investigators had supposed these conceptacles to be cryptostomata, like those of Fucaceæ, to which this alga was supposed to belong, and in their earlier stages they are exactly similar, "the tufts of hairs extending," like those of *Cystoseira*, "a long distance beyond the ostioles." Mr. Murray (6), in comparing these points in *Splachnidium* and *Fucus*, says: "In *Fucus* it will be remembered that hairs are not produced until the formation of a young conceptacle with an ostiole, while the initial cell has decayed. The

hairs in the conceptacle of *Splachnidium* are long, septate, and unbranched, increasing in length by successive divisions at the base, and giving the young conceptacle the appearance of the cryptostomata of the Fucaceæ. The sporangia are developed later. We have, in fact, in this type a sorus of sporangia and paraphyses comparable with those of *Laminaria*, rolled up and definitely limited in a conceptacular body, produced, not exactly in the same manner as the Fucaceous conceptacle, but after another manner directly comparable with it."

The occurrence of cryptostomata is by no means confined to the Fucaceæ; we find them in genera of Laminarieæ—*Alaria*, *Saccorhiza*, and *Adenocystis*. They also occur on other algæ, such as *Hydroclathrus* (Scytosiphonaceæ), described by Miss Mitchell (5), and on *Chnoospora* (Sporochnaceæ).

As regards the Laminarian cryptostomata, Mr. Murray, who has examined them himself, writes: "I have been so fortunate as to obtain good examples of their occurrence in *Saccorhiza bulbosa* and *Alaria esculenta*. *Alaria* has been frequently described as possessing cryptostomata. Greville definitely refers to them as "minute pores from which issue minute tufts of filaments"; and Professor Bower refers to them also in passing. I have not placed myself in antagonism to these authorities on this minute point, without arming myself with certainty . . . . No pits or conceptacles are formed, and in a very old *Alaria* frond which I have examined, there was at most a depression one cell deep. The *Alaria* cryptostomata, if I may call them so, are tufts of hairs of basal growth, the cell at the apex of the hair being first cut off from the epidermal layer. On Plate XVI., Fig. 7, I have illustrated the mature cryptostoma of *Saccorhiza bulbosa*. So far as I am aware, the mature stage of these bodies in *Saccorhiza* has not been figured before, and the reader who compares this figure with that of the young conceptacle of *Splachnidium*, while it yet bears hairs only, cannot fail to be struck by the resemblance in all respects except the persistent initial cell of *Splachnidium*. We have here, then, a body which has every claim to rank as a cryptostoma." Mr. Murray then alludes to Mr. Setchell's paper on *Saccorhiza dermatodea*, in which he has figured an early stage of the cryptostomata of this plant, which strongly resembles the early stage of *Alaria*. The development of the cryptostoma of *Alaria* is regarded by Mr. Murray as "arrested," and he accounts for this by the fact that the frond of *Alaria* is so thin as not to allow room for the formation of pits. He is confirmed in this view by Mr. Setchell's remark that "the cryptostomata are present on the one-layered portion of the blade (of *Saccorhiza dermatodea*) as clusters of hairs upon the flat surface."

Mr. Murray then goes on to consider *Adenocystis*, a hollow sac-like alga, in form quite unlike any of the other Laminarieæ. He says: "It bears a resemblance both to the cryptostoma of its ally *Saccorhiza* and to those of *Hydroclathrus*, illustrated by Miss Mitchell.

The reproduction of *Adenocystis* is by zoospores, borne in zoosporangia and the cryptostomata occur in the middle of sori of unilocular sporangia." The paraphyses and sporangia, which are truly Laminarian, grow on the very edge of the depression. "In the other Laminarieæ examined, the sorus occurs separately from the cryptostomata, but, in this form, in the middle of the sporangia and paraphyses, as if in a nascent effort (or a dying one, as the case may be), to form a conceptacle like that of *Splachnidium*. It will be remembered that a young conceptacle of *Splachnidium* bears hairs only at first and sporangia later on."

In *Hydroclathrus*, cryptostomata are described by Miss Mitchell as occurring among plurilocular sporangia.

Mr. Murray concludes his paper by alluding to Professor Bower's remark about the groups of hairs in the Dictyotaceæ, "which are the precursors of the reproductive cells." Professor Bower says: "How far these may be compared with the initial cell or hair of the Fucaceæ, it remains for closer observation to decide." Mr Murray attaches a prophetic value to this utterance, and notes that Falkenberg has pointed out "that in *Scytosiphon* single epidermal cells within a sorus remain sterile and grow out in the form of club-shaped unicellular paraphyses." Hairs are also found in *Asperococcus* and other allied forms, arising in similar situations, "resembling the hairs in the conceptacles and cryptostomata of the Fucaceæ, *Splachnidium*, *Saccorhiza*, and *Adenocystis*, and those in the sorus of Cutleriaceæ and Dictyotaceæ."

In conclusion, I cannot do better than quote here the summing-up of Mr. Murray's interesting paper. "A comparison of the Fucaceous conceptacle and cryptostoma, the Splachnidian conceptacle, with its persistent initial cell and the formation of its hairs yielding place to sporangia, the development of the *Adenocystis* cryptostoma in the heart of its sorus, the other Laminarian cryptostomata (*Saccorhiza* and *Alaria*) apart from the sorus, the cryptostoma of *Hydroclathrus* among its plurilocular sporangia, and finally the cases of the hairs in *Asperococcus* and the Cutleriaceæ and Dictyotaceæ—a comparison of these cases and of the evidence plainly furnished by them points very significantly to a possible origin of cryptostomata." These are striking admissions by one who has heaped contempt on "the ancestors of the Fucaceæ."

Such is the present state of our knowledge of the history and development of cryptostomata. Links are still needed to complete the chain of evidence which is to connect the hairs growing out from the surface of the Dictyotaceæ with the well-developed conceptacles of the higher forms of the Fucaceæ. But there are types still to be examined, and much interesting work to be done. The foreign forms of the Laminarieæ may, and probably will, show variations and gradations in the scale of development hitherto unsuspected, and we may look forward to most interesting results when any worker

examines and describes the sori of these forms in the light of what has been already done.

The questions to be answered, if possible, appear to me to be these :—

Are the Fucaceous conceptacles and cryptostomata the descendants of similar bodies evolved by ancestors approaching the Laminarian type, in which we see them now dying out? Is the Laminarian sorus a mere flattened-out conceptacle? or, have such shelters for spores, antheridia and oogonia, for they are merely that, been evolved independently of each other by these different types under a common necessity of protecting their spores, etc.? Do we now witness nascent or dying efforts to produce conceptacles in such forms as *Adenocystis* and *Hydroclathrus*? Are the barren cryptostomata of *Saccorhiza* prophetic or ancestral structures?

Their further examination, at all events, cannot fail to throw light where it is much needed—on the relationships of the Natural Orders of Phæophyceæ.

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E. S. BARTON.

VII.

Scientific Volapuk.

WE have more than once alluded in these pages to the misapplied ingenuity of certain scientific writers, who, being unable to use the language with which their natural heritage has supplied them, imagine that they can better convey their ideas to the world through the medium of words that are not merely new and strange, but often incorrectly constructed and far too vaguely defined (NATURAL SCIENCE, vol. ii., p. 6, and vol. iii., p. 242). Let it not for a moment be supposed that we deny the value and even the necessity of scientific terminology; on the contrary, for technical purposes, a distinct and well-defined term is, we consider, far superior to an ordinary word distorted from its ordinary meaning. At the same time, it is obviously desirable that these scientific and technical terms should be constructed according to etymological principles of a higher grade than those that govern the appellation of the latest patent medicine or the newest thing in window-blinds. And, apart from this, there are other cautions that might profitably be attended to by many of these word-mongers.

Chiefly should one hesitate before encumbering—we will not say the English language, but—the Volapuk of scientists, with any additions to its already overcrowded vocabulary. Reflect first if the idea cannot be as clearly expressed by words already in use. Brevity may brighten wit, but no less does it darken wisdom. Next consider whether the idea be actually new, or, if new, whether it be a reality. Does, for instance, Mr. Ryder or anyone else seriously suppose that there is an abstract formative force concerned in modifying the forms of bones? If not, where is the necessity for a name? As for those people who go out of their way to invent terms which neither they nor anyone else will probably ever use again in this world, what torture in the next world would be appropriate? Yet this is what Professor Hyatt does in his paper on Bioplastology, when he suddenly stops to tell us that if Hering were right in supposing heredity to be a form of memory, then *Mnemegenesis* would be a very good word for heredity. Shall we suggest that Professor Hyatt should be set first to suppose forms of punishment, and then to invent Greek names for them?

So many new terms have been proposed in the last few years, that we have determined to make some attempt to collect them year

by year in a form available for the use of the scientific public, adding definitions, too often forgotten by the original authors, and derivations when we have been able to discover them. It is not likely that our list for this year is complete. We have purposely taken no notice of terms dealing only with limited groups of animals, and we have not thought it necessary to include every possible combination of terms with prefixes and suffixes suggested by authors. Thus in his paper on Bioplastology (3), Professor Hyatt also subdivides each of the stages of Ontogeny, from Nepionic (=Brephic, Buckman and Bather) to Gerontic, into three substages, which are distinguished, on Buckman's suggestion, by the prefixes *ana-* (up towards), *meta-* (in common with), *para-* (beyond). Thus the Gerontic stage, instead of having two substages, Clinologic and Nostologic (Hyatt), or Catabatic and Hypostrophic (Buckm. and Bath.), will have three, viz., *Anagerontic*, *Metagerontic*, and *Paragerontic*. Professor Hyatt also proposes to apply Haeckel's physiological terms, or his own modifications of them, to Phylogeny, by using the prefix *phyl-*; thus, *Phylanaplasis*, *Phylometaplasis*, and *Phyloparaplasis*.

Besides these, other terms have doubtless escaped our notice, and we shall be glad of any additions. Authors would also confer a favour by sending us, in future, a note of any new terms they may propose.

We cannot conclude without protesting against a tendency, far too prevalent, of introducing these and similar terms, originally invented for the benefit of specialists, into works of a more popular description. This is not the way to popularise science. Possibly the writers think that long words give an air of learning. We believe that they tend to mystify both writer and reader, substituting sound for sense, and, too often, fancy for fact. It was to politicians, not to scientists, that words were given for the purpose of concealing thought; yet we fear that some day the latter also may find that they have floated too far into the clouds of unintelligibility, and may be brought back to earth only to repeat, in the words of the man they shouted "Hi!" at, in the *Hunting of the Snark*—

> "I said it in Hebrew—I said it in Dutch—
> I said it in German and Greek:
> But I wholly forgot (and it vexes me much)
> That English is what you speak!"

List of New Terms Proposed in the Natural Sciences during 1893.

Aborad and *aboran*, converse of "orad" and "oran." (Schulze.)

Astrokinetic (ἄστρον, a star; κινητικός, setting in motion), applied to phenomena of motion of centrosomes of cells. (Ryder.)

Astrosphere (ἄστρον, star; σφαῖρα, sphere), improved term for "attraction sphere," a phenomenon of cell-division. (Strasburger.)

Astrostatic (ἄστρον, a star; στατικός, causing to stand still), applied to phenomena of rest of centrosomes of cells. (RYDER.)

Autotrophic (αὐτός, self; τροφικός, nourishing), existing without aid of commensals: applied to lichens. (MINKS.)

Axian, axial, axiad, adjectival forms of axis, *i.e.*, the longitudinal or main axis. Compare "Centrum" and derivatives. (SCHULZE.)

Bathmology (βαθμός, a step; λόγος, science), the science of growth. A term proposed as alternative to "Auxology" (Buckman and Bather), and correlative of "Bathmism" (Cope). (HYATT.)

Bilateralien. Organic bodies with three axes of symmetry, viz., *isopol*, which equals the perlateral; *heteropol*, or dorso-ventral; and heteropol or longitudinal, the main axis. (SCHULZE.)

Bioplasmology (βίος, life; πλάσμα, substance; λόγος, science), cellular biology; correlative of "Bioplasm, etc." (Beale.) (HYATT.)

Bioplastology (βίος, life; πλαστός, moulded; λόγος, science), the study of the correlations of ontogeny and phylogeny. An alternative to "Biogeny" (Fiske) and "Biogenesis" (Haeckel non Auctt.) (HYATT).

Blastokinesis (βλαστός, germ; κίνησις, movement), migrations of embryo through and over yolk, as in the Orthopterous insect *Xiphidium ensiferum*. (WHEELER.)

Centrad or *proximad*, directed towards the Centrum. (SCHULZE.)

Central or *proximal*, near but not in the Centrum. (SCHULZE.)

Centran, within the Centrum. (SCHULZE.) In the polar axis of a Synstigma. (HYATT, 4.)

Centren, equivalent to "centran" (Schulze) (HYATT, 4.)

Centrum, the imaginary geometrical centre of a Synstigma. (SCHULZE.)

Cephalan, cephalal, cephalad, adjectival forms from κεφαλή, head. Compare "Centrum" and derivatives. (EDITOR in HYATT, 4.)

Co-type (erroneously for "syntype," *cum*, with; τύπος, type); applied in zoology to one of two or more specimens together forming the basis for the original determination of a species. (THOMAS.)

Ctetic (erroneous application of κτητικός, acquisitive), a term for "acquired characters." (HYATT.)

Ctetology (erroneously for "ctematology," κτητός acquired; λόγος science), the study of the effects of environment on organisms, and of the inheritance of those effects. (HYATT.)

Cytokinetic (κύτος, a cell; κινητικός, setting in motion), phenomena of motion within plasma or cells. (RYDER.)

Cytostatic κύτος, a cell; στατικός, causing to stand still), statical conditions within plasma or cells. (RYDER.)

Dextran, dextral, dextrad, adjectival forms from *dexter*, right; referring to right half of perlateral axis of "Bilateralia." Compare "Centrum" and derivatives. (SCHULZE.)

Dictyosome (δίκτυον, net; σῶμα, body), thread-like bodies appearing in cell-division, *e.g.*, spermatogenesis of *Branchipus*. (MOORE.)

Diplanetism (apparently an erroneous form of *diplanesis*, from δίς, twice, and πλανησις, a dispersing), double swarming of zoospores (HUMPHREY.)

Distad, directed from the Centrum. (SCHULZE.)

Distan, on the periphery of a Synstigma. (SCHULZE.)

Dorsan, on the outer surface of the dorsum. (SCHULZE.)

Ectergogenesis (ἐκτός, without; ἔργον, work; γένεσις, birth), the passive reactions of an organism to environment. (HYATT.)

Ectergogenic (erroneously for "ectergogenetic"), adjectival form of the above. (HYATT.)

Entergogenesis (ἐντός, within; ἔργον, work; γένεσις, birth), the active reactions of an organism to environment. Equals "effort" of the Neo-Lamarckians. (HYATT.)

Entergogenic (erroneously for "entergogenetic"), adjectival form of the above. (HYATT.)

Entergogenism (an impossible word), the principle of the above. (HYATT.)

Ergogeny (erroneously for "ergogenesis") (ἔργον, work; γένεσις, origin), general term for all forms of energy producing modifications of bone. (RYDER.)

Ergogenetically adverbial form of the above. (RYDER).

Genesiology (γένεσις, birth; λόγος, science), the study of the phenomena of heredity. (HYATT.) This term has already been used in another sense, viz., "the science of generation," and should therefore be inadmissible.

Genetic force, the active force of heredity. (HYATT.)

Genism (an impossible word), the principle of heredity. (HYATT.)

Hemera (ἡμέρα, day), a chronological subdivision of the geological term "age," marking the acme of development of one or more species. (BUCKMAN.)

Heteropol, see *Bilateralien*.

Heterotrophic (ἕτερος, other; τροφικός, nourishing), existing with aid of commensals; applied to lichens. (MINKS.)

Isopol, see *Bilateralien*.

Karyokinetic (κάρυον, a kernel; κινητικός, setting in motion), applied to phenomena of active stages of cell-nuclei, an old term restricted. (RYDER.)

Karyostatic (κάρυον, a kernel; στατικός, causing to stand still), applied to phenomena of resting stages of cell-nuclei. (RYDER.)

Kinetogeny (erroneously for "kinematogenesis," or "kinesigenesis," κινεῖν, to move; γένεσις, origin), energy of motion, the agency producing modifications in the form and proportions of bones. (RYDER.)

Meta-type (μετά, after; τύπος, type), applied in zoology to a specimen collected at the exact locality whence was obtained the type of the species to which it belongs, and named by the author of that species. (THOMAS.)

Mnemegenesis (μνήμη, memory; γένεσις, descent), Hering's explanation of heredity as a form of unconscious organic memory. (HYATT.) It will be observed that, in this compound, "genesis" is used in a different sense to that which it bears in the rest of Hyatt's coinage.

Monocyclon, or *Monocyclic*, is applied to a phylogeny in which only one cycle is observed. (HYATT.)

Morphic (μορφή, form), connected with structure. Instead of morphologic, when incorrectly used in this sense. (HYATT.)

Nepiastic, an emendation of the corrupt "nepionic," or "næpionic," of Hyatt; derived from νήπιος, infant, presumably by way of the rare form, νηπιάζω, I play like a child. It equals "Brephic" of Buckman and Bather. (SCHUCHERT.)

Nostic (erroneously formed from νοστός, a return home), the final stage of decline. Supplants "Nostologic" (Hyatt), and "Hypostrophic" (Buckman and Bather). (HYATT.)

Ontocycle or *Ontocyclon*, the cycle of individual development. (HYATT.)

Orad, towards the mouth opening, when mouth and anus are terminan and opposite. (SCHULZE.)

Oran, within the mouth opening, under the same conditions. (SCHULZE.)

Paraplasis, decline in Ontogeny, senescence. Supplants "Cataplasis" (Haeckel). (HYATT.)

Parameridian (a mongrel term from παρά and *meridianus*), applied to any plane parallel to the meridian or plane of the principal axis in a Syngramma. (SCHULZE.)

Paratangential (a mongrel of the same litter), within the body of a Synstigma and at right angles to its radii. (SCHULZE.)

Para-type (παρά, alongside of; τύπος, type), applied in zoology to a specimen of the original series forming the basis for the original determination of a species, but not the type, in cases where the author has himself selected a type. (THOMAS.)

Peripheran, on the periphery of a Syngramma. (HYATT, 4.)

Phylocycle or *Phylocyclon*, the cycle of development of the race. (HYATT.)

Polycyclon or *Polycyclic* is applied to a phylogeny in which many cycles are observed. Further, the number of cycles may be expressed: thus, the Arietidæ, having ten cycles, are *Decacyclic*. (HYATT.)

Pseudosomes (ψεῦδος, false; σῶμα, body), bodies corresponding with centrosomes in everything except number, which appear in spermatogenesis of *Branchipus*. (MOORE.)

Sinistran, *sinistral*, *sinistrad*, converse of *dextran*, etc. (*q.v.*). (SCHULZE.)

Statogeny (erroneously for "stasigenesis," στάσις, a standing; γένεσις, origin), antithesis of *kinetogeny*. (RYDER.)

Sympeden = Bilateralien. (SCHULZE.)

Syngramme (σύν, with; γράμμα, a drawing), organic bodies symmetrical about a main axis. This term supplants "Centraxonia" (Haeckel). (SCHULZE.)

Synstigmen (σύν, with; στίγμα, a mark), organic bodies with an imaginary geometrical centre. This term supplants "Centrosigmen" (Haeckel). (SCHULZE.)

Syntrophy (σύν, with; and τρέφω, I nourish) with erroneous variant *syntrophism*, and substantive *syntroph*. Apparently equals "commensalism" of former authors, but is specially applied to the endophytic elements of lichens. (MINKS.)

Tangential, at right angles to the radii of a Synstigma, and on the surface thereof. (SCHULZE.)

Termini, the ends of the main axis of a Syngramma, with adjectival forms *terminal*, *terminan*, *terminad* (compare "Centrum" and derivatives). (SCHULZE.)

Topo-type (τόπος, place; τύπος, type) applied in zoology to a specimen collected at the exact locality whence was obtained the type of the species to which it belongs. (THOMAS.)

Ventran, on the outer surface of the venter. (SCHULZE.)

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SOME NEW BOOKS.

ANNALS OF BRITISH GEOLOGY, 1892. A Digest of the Books and Papers published during the Year with Occasional Notes. By J. F. Blake, M.A., F.G.S. Pp. 310, 8 plates, and text illustrated. London: Dulau & Co., 1893. Price 10s.

THIS is the third volume of an extremely useful work, the previous volumes of which were noticed in NATURAL SCIENCE (vol. i., p. 69, and vol. ii., p. 234). The labour of preparing such a record is so enormous that we should under any circumstances have offered our thanks to Mr. Blake; but the present volume is so great an improvement on its predecessors that we must also tender him our congratulations. The work has passed through the fire of criticism and has emerged all the better for its ordeal. It has occasionally been thought that Mr. Blake was too unwilling to distinguish between the functions of annalist and analyst. The pungent notes that he used to interject in the middle of an abstract sometimes caused as much wrath as merriment. These are now absent, to the delight of authors if not of readers. Not but what our recorder still has his say, only his criticisms are now placed together in the form of an introductory review. This, he thinks, can scarcely be objected to if we admit the principle that "thou shalt not muzzle the ox that treadeth out the corn." To judge from this review the authors also seem to have improved, at least in Mr. Blake's opinion, and we do not think any of them are likely to reply, in the words of Agamemnon's watchman,—

"For the rest I must be silent, a great ox hath trod upon my tongue."

The abstracts now seem still more carefully done, and if they do not satisfy authors, then the authors have only themselves to blame, since they are now afforded the opportunity of revising the manuscript itself, and this is a privilege of which we are glad to see the majority have availed themselves. The illustrations are also greatly improved, partly owing to original blocks having been lent by some societies and individuals, a mode of assistance which might very well be extended. It is, however, to be hoped that the wretched appearance of fig. 2 is not due to the fact of the author having changed his printer.

Except for the addition of the interesting introductory review, which the authors mentioned will save us the labour of criticising, the arrangement of the volume follows the old lines. One or two papers, however, seem to have gone astray. The Cave Men of Mentone, for instance, were hardly Britishers, while Mr. Teall's paper in NATURAL SCIENCE on the "Sequence of Plutonic Rocks" took its examples from foreign rather than home sources. The inclusion of a section dealing with papers on foreign geology published in England is probably intended as a bait for foreign purchasers;

personally we think that in abstracting these papers, the compiler gives himself unnecessary trouble.

We have now done our duty by this book. It only remains for our readers to order copies, and so to give Mr. Blake that pecuniary support without which work of this kind cannot be carried on. If this support is not given, "the only remaining hope," says Mr. Blake, "would be that the Geological Society should undertake a record at their own expense, by the aid of paid recorders." A society so poor that it cannot subscribe £100 *per annum* for the recording of palæontological literature, is hardly likely to jump at this suggestion. On the other hand if support is forthcoming, then, says Mr. Blake, the "Annals" might very well be expanded to the "Annals of British and *Foreign* Geology."

It may be pointed out that records of geological literature are now published in most European countries and to some extent in America. Why, in the name of common sense, cannot the societies and individuals engaged in carrying on this most valuable work combine their scattered and often wasted energies, and issue a series of annual bibliographies for each country of the world? Each such bibliography might contain either the papers published *in* its own country or those *about* its own country. The former plan would be cheaper, easier, more likely to be complete, and in the end more useful. The labourers are there; they only want a leader. *Verb. sap. sat.*

F. A. BATHER.

THE STORY OF OUR PLANET. By T. G. Bonney, D.Sc., LL.D., F.R.S. Medium 8vo. Pp. xv., 592, with six plates and 170 illustrations. London: Cassell & Co., 1893. Price 31s. 6d.

THIS work forms a companion volume to Sir Robert Ball's "Story of the Sun." It is planned, as the author himself states, somewhat on the lines of Lyell's "Principles of Geology," and it seems a pity that so excellent a model is not more closely followed. Professor Bonney divides the book into five sections, entitled: "The Story: its Books and their Speech," "The Processes of Sculpture and Moulding," "Changes from Within," "The Story of Past Ages," "On some Theoretical Questions." The first part contains introductory chapters on geology and physical geography, such as can be found in any text-book; but they do not seem to be carefully welded together, or to form parts of a connected whole. The second part is better done, and the author treats in a thoroughly systematic manner of the changes that take place on the earth's surface. Whenever it speaks of mountains, or of phenomena occurring among mountains, the book becomes interesting, and the illustrations also are better selected. The parts relating to glaciers, for instance, are very good; though the author would, perhaps, have spoken less confidently as to origin of lake-basins through irregular subsidence if he had seen Mr. A. R. Wallace's paper in the last *Fortnightly Review*. Leaving out of account theoretical questions, upon which opinions may differ, we have not noticed many serious mistakes or omissions in this part of the book. Fig. 34, fossil rainprints, if it represents anything of the sort, which we doubt, must represent, not the rainprint itself, but a cast of the print seen from below.

The third part, "Changes from Within," contains accounts of earthquakes and volcanic eruptions, including several important ones that have taken place since the publication of the last edition of

Lyell's "Principles." It also gives an outline of certain recent discoveries in mountain structure, and on the origin of the crystalline schists; though this part is scarcely up-to-date.

The fourth part commences with an account of meteors and the earth's beginning, leading to chapters on historical geology. All this section seems to be compiled from various well-known text-books, and would be improved by more thorough revision. The final section, "On some Theoretical Questions," contains four disconnected chapters, which we cannot help thinking would be better placed in the sections to which they properly belong, for other equally theoretical questions have been discussed in various parts of the volume. The four chapters thus isolated refer to "The Age of the Earth," "The Permanence of Ocean Basins and Land Areas," "Climatal Change: its cause and history," and "The Distribution and the Descent of Life."

We have, perhaps, criticised this book somewhat more severely than we should otherwise have done if the author had not challenged comparison with Lyell. The mantle of Lyell has not descended to Professor Bonney; but if unscientific readers wish for a popular "Story of our Planet," we can safely recommend the present work.

Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs. Band. vi., Abtheilung iv. Aves. II. Systematischer Theil. By Hans Gadow, Ph.D., F.R.S. 8vo. Pp. 303.

Dr. Gadow, in the concluding parts of the above-mentioned work, adds the keystone to his long labours, which, it is not too much to say, will prove of incalculable benefit for all time.

Those who are familiar with the anatomical section of this book will naturally turn to the present volume with great expectations; and assuredly they will not be disappointed, for they will find that, what has ever proved a subject of extraordinary difficulty and complexity, whether regarded from the limited horizon of the Ornithologist, or the more extended range of the Biologist, has been treated with a thoroughness that will claim at once the gratitude and the admiration of all workers in this field.

By reason of the plan adopted by Dr. Gadow of presenting us with tables of comparison between the various orders and families and pithy "conclusions," it is possible to trace, step by step, the ground over which the author has passed in the construction of his present scheme—a boon which we shall not sufficiently appreciate till we recollect the painful groping in the dark, and fruitless pursuit of phantom probabilities, that must necessarily have been involved in a research of this nature.

Substantially, the present arrangement is the same as that which appeared in the *Proc. Zool. Soc.*, 1892, enlarged and improved.

Thus the Class Aves is now divided into two sub-Classes, Archæornithes and Neornithes; and three divisions, "Neornithes Ratitæ," "Neornithes Odontolcæ," and "Neornithes Carinatæ." The third division is divided into two brigades of two "legions" each, the Colymbomorphæ and Pelargomorphæ comprising the first, and the Alectoromorphæ and Coraciomorphæ the second brigade. To the Colymbomorphæ belong the Ichthyornithes, Colymbiformes, Sphenisciformes, and Procellariiformes: while the Pelargomorphæ embrace the Ciconiiformes, Anseriformes, and Falconiformes. The Alectoromorphæ contain the Tinamiformes, Galliformes, Gruiformes, and

Charadiiformes; and the Coraciomorphæ, the Cuculiformes, Coraciiformes, and Passeriformes.

Such are the main divisions of Dr. Gadow's scheme. To pursue it further, would be neither permissible nor desirable here; but we think, in justice to our task, we should indicate some of the principal features of the remainder of the book; which, as we have previously hinted, is simply teeming with information, not of the "handy reference-book" order, but facts, skilfully marshalled.

Dr. Gadow commences with a short definition of the Class Aves, in which he admits the derivation of birds from reptiles; though from which sub-class cannot of course be proved. We are reminded that the distinction between birds and "not-birds" is unmistakable; yet it is only a question of time for some fortunate palæontologist to bridge over the gap, since gap there must be, for such remarkable structures as feathers, and the modification of the fore-limb into the characteristic Avian wing, cannot have come suddenly into existence. *Archæopteryx*, the most archaic bird we know, has attained a high degree of development; so that there must have existed a long chain of unknown forms connecting it with the reptiles. For such intermediate forms the term "Herpetornithes" is proposed.

The Neornithes are probably the direct descendants of the Archæornithes; but whether *Archæopteryx* was a direct ancestor, or only a small side branch, is a matter for speculation. The Neornithes seem to have become well developed by the end of the Jurassic period; in Cretaceous times they still retained teeth, but had even then developed into flying forms with a pygostyle, and flightless forms without either a carina sterni or pygostyle.

After discussing the characters, whether primitive or pseudo-primitive, which distinguish the Ratitæ from the remaining birds, Dr. Gadow proceeds at some length to dilate upon the relation of the Ratitæ to the Carinatæ. He opens his argument by very properly disposing of the mythical relationship, hitherto considered by some to exist between the Ratitæ and the Dinosaurs on the one hand, and the Carinatæ and Pterosaurs—through *Archæopteryx*—on the other, as "profitless dreaming"; pointing out that, if the similarity of the hind-limb existing between certain of these reptiles (the Dinosaurs) and the Ratitæ be held to be a proof of relationship, then all birds must be derived from the same stock, since in all birds the component elements of the tarsus are the same. Proceeding, the author next discusses the mono- or polyphyletic derivation of the Ratitæ, and this section will require very careful reading. Dr. Gadow's conclusions have led him to regard the Ratitæ as monophyletic. He proposes to retain the "Ratitæ" as a separate group, "upon practical taxonomic grounds."

The division "Neornithes Odontolcæ" appears to have been made with some diffidence to contain the Hesperornithes and Enaliornithes; but, it is urged, the arrangement must remain until Jurassic or Cretaceous birds are discovered, which possess a carina sterni and teeth standing in a groove.

The third and last division—"Neornithes Carinatæ"—contains all the remaining birds. Of brigade 1 it is noteworthy that the Colymbomorphæ (= legion 1) include the Ichthyornithes—and, we presume, but for special reasons, the Hesperornithes would have been included also. Passing on to the Coraciomorphæ, for space will not permit us to slowly meander as we should wish through the intermediate groups, we notice that the author confirms his earlier paper

both in placing the Hornbills as a sub-family of the Hoopoes (Upupidæ), and retaining the Striges in close proximity to the Caprimulgi, instead of reverting to their time-honoured position, with the Accipitres.

Both cases will, we anticipate, create some discussion. In the case of the Striges, we fail to see how anyone, after he has carefully and impartially gone through the mass of facts which have been collected, as if for the very purpose of converting sceptics (pp. 235-241), can conscientiously stand by the old arrangement.

"The division of the Oscines into families is as yet very unsatisfactory," and we are referred for the present to the British Museum catalogues. The Australian region is to be regarded as the birthplace of the Oscines, from whence they have overspread the whole habitable earth.

Towards the end of the book some fifteen pages have been devoted to the Geographical Distribution of Birds, and extremely interesting reading it proves. The systems of Sclater, Wallace, Huxley, Newton, Sharpe, and others, are briefly reviewed, and then follows a short "Examination of birds as to their utility for dividing the earth into ornithological regions," which is fitly terminated by a contribution from Dr. Gadow himself, in which he recognises two main regions, A—Notogæa, and B—Arctogæa; the former embracing an Australian and Neotropical region, the latter a Palæotropical and Periarctic region.

Now that Dr. Gadow has said his last word, the question for the Ornithologist in the future will be not how much *is*, but how much is *not* known on any particular question upon which he may require information. The author is to be congratulated on the way in which he has fulfilled his most difficult and laborious task. W. P. P.

HORNS AND HOOFS; or, Chapters on Hoofed Animals. By R. Lydekker. 8vo. Pp. 411, with 82 woodcuts. London: Horace Cox, 1893. Price 15s.

THIS is a popular work, comprising a series of articles originally contributed to the *Field* and *Land and Water*, and now reissued in collective form, with some revision. The volume is beautifully printed and well illustrated, and will prove of interest not only to the sportsman, for whom it is primarily intended, but also to the naturalist who happens to be unfamiliar with the latest discoveries and most advanced views in reference to "Big Game" of all kinds except elephants and Carnivora. The first chapter deals with the Wild Oxen, which are all included by Mr. Lydekker in the genus *Bos*; and the various groups are described under the headings (1) aurochs and zebus, or true oxen; (2) gaur; (3) yak; (4) bison; and (5) buffalo. The musk-ox (*Ovibos*) is placed separately at the end. The second chapter treats of the Wild Sheep, which are essentially mountain animals, and are especially numerous in the highlands of Central Asia. They are subdivided into (1) the bighorns of North America and Kamschatka; (2) the argalis of Central Asia; (3) the urials of Asia, with the moufflons of Asia Minor and Europe; and (4) the bharals, as represented by the true bharal of Little Thibet and the Barbary sheep of North Africa. The Goats are next discussed and arrayed in five groups, as follows:—(1) The ture of the Caucasus, with the so-called Spanish Ibex, the sheep-like goats; (2) the pasang, or true goats; (3) ibex; (4) markhor; and (5) the tahr, or *Hemitragus*, of India. The Antelopes and Deer are

treated at great length, incorporating in a general way the latest results of systematic zoologists; and the volume concludes with two chapters on Pigs and Rhinoceroses. Unlike most authors who furnish Zoology for the general reader, Mr. Lydekker makes numerous allusions to the extinct forerunners of the "Big Game" he describes; and there is a pleasant tinge of philosophy in "Horns and Hoofs" which one rarely meets with in works of its class.

CONCHYLIOLOGIE FRANCAISE. LES COQUILLES DES EAUX DOUCES ET SAUMATRES DE FRANCE. By A. Locard. 8vo. Pp. 327, with figures in text. Paris: J. B. Ballière & Son, 1893. Price 18 frs.

THE present volume is one of a series by the same author on the shells (not the mollusca, be it noted) of France. Its scope is sufficiently indicated if "recent and quaternary" be added to its title. The work is an admirable example of the modern French school at its very worst, and is eminently calculated to dishearten any student who should attempt to use it, and cause him to abandon all pursuit of the subject in disgust and despair. The figures are, compared with the number of "species" described, few and poor; the descriptions are inadequate and confusing; while the whole work is superficial to a degree. Every slight variation in the form of the shell is held sufficient ground for making a distinct "species," and as such most synonyms and bad varieties are consequently ranked. The rows and rows of these under *Lymnæa*, *Planorbis*, *Pisidium*, and *Unio* are, for instance, bad enough, but the author fairly "beats the record" under *Anodon*. Where Isaac Lea, who was no mean hand at species-making, could only see one, Locard makes two genera with 279 species! Further comment is needless.

BRITISH EAST AFRICA OR IBEA: A History of the Formation and Work of the Imperial British East Africa Company. By P. L. McDermott. Pp. xvi., 382, with map. London: Chapman & Hall, 1893. Price 6s.

AT a time when the system of the extension of the Empire by means of Chartered Companies is on its trial, this history of one which has had a rather unfortunate career is very opportune. The book is almost entirely political, and is an *ex parte* statement of the case for the Company against its foes, both official and unofficial. It gives but little information about the country; what little there is has been written in such an optimistic spirit, and the statements are so vague and general, that one may be excused for distrusting it. One could have wished that some of the lengthy quotations from leaders in *The Times* had been replaced by extracts from the unpublished reports of the various experts, geological, agricultural, and mining, who have been employed by the Company. As the future of the district must largely depend on its economic resources, the public would be in a better position for judgment had such materials been placed before it instead of the story of the Company's past discussions with the Foreign Office. As the work has been written by a paid official of the Company at the instance of its chairman, we have no right to expect impartiality and the insertion of matter that is not to the credit of the Company; and we have not got it. For example, we are told about the station at Dagoreti as if it were still flourishing and in existence; as a matter of fact, it was abandoned by the Company's officers and immediately burnt by the Wakikuyu some years ago, and

a new one has since been built at Kabeti to replace it. Mr. McDermott has certainly done his work well, and given us an interesting history and ingenious defence of the Company; but we fear he will only strengthen the general feeling that the Company's record shows that a mixture of philanthropy and business seldom pays. Had the Company stuck to one or the other, and especially if it had confined its attention to administration pure and simple, and the utilisation of the available resources of the district, this book would probably never have been written, for an apology might then have been unnecessary.

REPORT OF THE SECRETARY OF AGRICULTURE, 1892. 8vo. Pp. 656, with 71 plates. Washington: Government Printing Office, 1893.

ON the back of the title-page of this somewhat bulky volume there is printed a copy of a public resolution:—"Resolved by the Senate and House of Representatives of the United States of America in Congress assembled,—That there be printed 500,000 copies of the Annual Report of the Secretary of Agriculture for the year 1892"; and also "That the sum of 300,000 dollars, or so much thereof as may be necessary, is hereby appropriated . . . to defray the cost of printing said report."

Three hundred thousand dollars is, roughly, £60,000. Our Transatlantic cousins have a vastly higher opinion of agriculture as a national concern than we ourselves if they are willing to spend £60,000 in taking the first step to make public the results of one year's work. But then, as Mr. Rusk, the Secretary, observes, "The great blessing which this country enjoys from the fact that it is far less than some other countries the home of large landed proprietors," presents certain difficulties which it is the province of the Department to remove. That is to say, the absence of large landowners, commanding extensive capital, throws the burden of experiment and investigation of agricultural problems on the shoulders of the Agricultural Department. This, of course, explains the bulky volume, the perusal of which we would recommend to large landowners in general, as an incentive to the publication of those results of experiment and investigation which we have a right to expect from the possessors of extensive capital.

The general report of the Secretary occupies sixty pages, and gives an idea of the scope of the Department and the possibilities of future development. Unfortunately, we find it hampered by "the inadequate compensation which it is authorised to offer to the men of talent, scientific education, and experience which it needs to carry on its most responsible duties," in which its "facilities will be found to compare very unfavourably with those of the other Departments of the Government." We know of parallel cases. It does seem somewhat incongruous that, while 300,000 dollars are voted for printing reports, the herbarium, including the largest collection of American grasses in existence, and containing type specimens of nearly all the American species described during the last fifteen years, should be housed in a non-fireproof building. Meanwhile "Congress saw fit to extend an appropriation, covering the present fiscal year," to provide for experiments on the production of rainfall by explosives, but Mr. Rusk does not think that his facts relating to this subject justify the anticipations formed by the believers in this method of artificial rain-making.

After a short report by the Assistant Secretary on the divisions directly under his charge, follows a series of detailed reports by the specialists in the various divisions. These are the Chief of the Bureau of Animal Industry, the Chemist, the Entomologist, the Ornithologist and Mammalogist, the Botanist, the Chief of the Division of Vegetable Pathology, the Pomologist, the Microscopist, the Chief of the Division of Forestry, the Special Agent in charge of Fibre Investigations, the Superintendent of Gardens and Grounds, the Statistician, the Chief of the Seed Division, the chiefs of various publication and financial divisions, the Director of the Office of Experiment Stations and the Chief of the Weather Bureau. Finally, there is a special report on tea-raising in South Carolina.

We cannot discuss these reports individually. They certainly contain a great deal of interesting and valuable matter. Thus, that of the Chemist begins with a warning against black pepsin, a preparation of pepsin and sugar, designed to increase the yield of butter, but which merely gives a compound which is not the genuine article, but an incorporation of butter with about an equal weight of other substances. An extensive examination has been made of canned and preserved foods, and it is pointed out that in some cases the tin employed to coat the cans contained over 10 per cent. of lead, which, in contact with the natural acids of fruits and vegetables, might lead to the formation of lead salts highly injurious to health. Particular attention was paid to the examination of green-coloured peas, beans, and other vegetables. It is a common custom to use a copper salt, presumably the sulphate—in canning such materials, to preserve or intensify their green colour, and the consumer may be certain, when eating very green canned peas, beans, etc., that he is consuming large quantities of copper, which, as the Chemist observes, must be condemned on hygienic grounds. This report also gives information on the detection of adulteration in honey, and describes experiments with sugar beets and sorghum. The Entomologist discusses various insect pests, such as the potato-tuber moth, the ox warble, the rose saw flies, and others, and gives an account, illustrated by three plates, of the Insectary.

The report of the Pomologist contains some brightly-coloured plates, depicting varieties of different fruits suitable for cultivation. The Eldorado blackberry looks very luscious. The Microscopist figures some edible mushrooms and also gives methods for their culinary preparation and simple means for detecting poisonous species. And so on; but we have said enough to give an idea of the contents of the report and for further details must refer the inquirer to the original.

ILLUSTRATED GUIDE TO BRITISH MOSSES; with Keys to the Genera and Species. By H. G. Jameson, M.A. Published by the Author, 6 College Road, Eastbourne. 8vo. Pp. 80, with 59 plates. Price 7s. 6d.

WE are always glad to welcome a good book dealing with any phase of British botany. Questions of general morphology, anatomy, physiology, and the like occupy so large a proportion of the energies of the modern botanist that the flora of his own country is neglected or left to workers whose want of opportunity, training, or ability often forms a serious hindrance to success. Happily, the present is not a case in point. It is evident from his preface and the style of the whole work

that Mr. Jameson has himself felt a want, and then done his best to supply it for the benefit of others. The scope of the book is well expressed in the title. It does not profess to take the place of such works as Hobkirk's synopsis, but to meet the difficulty of the beginner, who is often at a loss to know whereabouts in these books he is to look for the description of his unknown and unnamed specimen. By the aid of carefully elaborated keys he arrives first at the genus and then at the species, and the specimens once identified, can be studied in detail in the larger works. The keys hitherto published have been founded almost entirely on fruit characters, which, as the author observes, leave the student quite at a loss with regard to some of the commonest and most easily distinguished mosses, which will probably be among the first gathered, but are not likely to be in fruit. By the present arrangement, however, most of the Pleurocarpous mosses and many of the Acrocarpous may be readily determined even in the barren state. These keys were originally published in the *Journal of Botany* during the year 1891, but have been thoroughly revised and in great part re-written for the present work.

The first 20 pages of the guide are devoted to a short but useful introduction. This, with the help of the illustrations, comprised chiefly in Plates I.–VII., to the figures in which ample reference is made, forms an excellent glossary, besides giving a good general idea of the external structure of a moss. In section VII., on the practical examination of specimens, the beginner will find all necessary instructions. Under each genus in the keys to the species, the author gives some introductory remarks which, while not professing to be a generic description, call attention to points characteristic of the British species, and sometimes include information on synonymy or recent additions to the British Moss Flora.

The great features of the book are the 59 plates, which comprise, in fact, two-thirds of the whole. Mr. Jameson is his own artist and lithographer, and we heartily congratulate him on the result. Drawings of the entire plant are rare, but those characters are given which are especially useful in distinguishing the species, namely, the form of the leaf, leaf-apex, leaf-cell, and often also of the fruit.

We hope the Guide will meet with the appreciation it deserves, and add stimulus to the study of a somewhat neglected but highly-interesting division of our Flora.

THE JURASSIC ROCKS OF BRITAIN. Vol. iii. The Lias of England and Wales (Yorkshire excepted). By H. B. Woodward, F.G.S. 8vo. Pp. xii., 299, with map and woodcuts. Memoirs of the Geological Survey of the United Kingdom. London: Printed for Her Majesty's Stationery Office. 1893. Price 7s. 6d.

IT is with irate feelings that we peruse this book; not, we hasten to add, on account of anything with which the author has connection. Our anger is roused by the "get up" of the volume. Badly printed with inferior type often set awry, on decidedly third-rate paper—material so thin that the printing of one side of the page seriously interferes with the other—the work is neither a credit to its printers nor to the British Government. Placed on our desk by the side of Government publications of other countries, it does much to lower our national pride, and it sets us wondering why our Government should take such pains to show how badly they can publish a work. The book is embellished with woodcuts of sections, which the

printing has made none too clear, and with figures of fossils which, from the same cause, can hardly be identified, notably on p. 44.

In striking contrast to the above are the contents of the volume, which are, for the most part, worthy of the painstaking ability of the author. The chapters on economic geology are undoubtedly the best part of the work; they contain much information valuable to others besides geologists. The stratigraphical portion of the volume displays the writer's general knowledge of the Liassic deposits to the best advantage; and it gives a remarkable amount of information for which every geologist will be thankful. Had this information not been marred by an unfortunate abundance of obvious palæontological inaccuracies, we should have had nothing but praise for this work. Unhappily, a considerable laxity in the use of specific names, and a too great reliance on out-of-date records, has, we regret to say, rendered the palæontological information to a large extent untrustworthy. We need not warn the field-geologist; a very little investigation will tell him that as regards the zonal records, both in the body of the work and in the appendix, it will be safer "to divide by four and disbelieve the quotient." In such case, however, the descriptive portion of the work becomes hardly more than a guide-book to the quarries and to the beds that they contain; but a volume on which so much labour has been bestowed is surely worthy of something higher than this.

It would have improved the work immensely, in our opinion, if the lists of species had been compared with those of good continental authorities, and if records on which doubt was thus cast had been expunged. A healthy scepticism as regards the insular idea that this country can show associations of species which the rest of Europe cannot produce should certainly be encouraged. We may pass the inevitable mistake of calling *A. falcifer* by the name of a generically different fossil, *A. serpentinus;* but such records as *A. semicostatus* and *A. raricostatus* together "above their appropriate zones" (p. 66), *A. subplanicosta* and *planicosta* in the same bed (p. 67), *A. semicostatus* and *subplanicosta* together (p. 165), *A. birchi* and *A. oxynotus* in conjunction (p. 148), and the table at the end of the volume having *Amm. planicosta*, *raricostatus*, and *semicostatus* ranging from the *Bucklandi*- to the *Jamesoni*-zone, *A. striatulus* from the *Annulatus* to the *Jurensis*-zone, and *Rhynchonella calcicosta* from the *Planorbis* to the *Spinatus*, may well excite suspicion as to the trustworthiness of the palæontology. The specialist, from his knowledge of the labels which *Ammonites* often bear, will no doubt be able to translate some of the above, as, for instance, "*A. planicosta*" with *A. sirius* (Reynès), and "*A. raricostatus*" with the young of *A. sauzeanus* (d'Orbigny); but the general geologist will, no doubt, be fogged.

The wisdom of the division between Lower and Middle Lias adopted in this work, contrary to general palæontological opinion, and contrary to what obtains in the companion Survey volume on Yorkshire, may well be questioned. In order to make the disproportion between Lower and Middle Lias less conspicuous, the author has had recourse to a "lumping" of the zones between those of *Angulatus* and *Capricornus*; while, on the other hand, a more than usually full recognition is accorded to the zones above the latter. As a consequence of the lumping, the zonalist will find in the appendix-tables a curious admixture of zonal species besides those noted above. Perhaps, however, what will most surprise him in this connection is that six species of Ammonites are said to pass from the Lower to the

Middle Lias; while no less than seventeen species are stated to go from the Upper Lias into the Inferior Oolite. This peculiar result is, of course, due to the author's method of treatment of the Upper Lias in Dorset and Gloucestershire; and it will be interesting to note in the Oolitic volume whither the author will be led by this pursuit of lithological uniformity in defiance of palæontological evidence.

From mistakes other than those of identification, and from misprints, the work seems to be creditably free. *Dactyloceras* is given throughout the Appendix instead of *Dactylioceras*; and a species called *Ophioderma milleri* on p. 192 is alluded to as *O. tenuibrachiata* on p. 198; while "mutations of sub-genera" (p. 328) certainly puzzles us.

A certain conservatism has prevented any attempt to classify *Ammonites* according to genera; but it has not been extended to Gasteropods, for new genera like *Pseudomelania* and *Nortonia* are admitted. There is the same conservatism in the matter of Ammonite-species. We are told that "many of the species are so split up that the multitude of names is simply bewildering, and they become of little or no service to the stratigraphical geologist" (p. 328). Now, we were under the impression that from the days of Oppel onwards the best stratigraphical work done on the Continent and in America had been accomplished by those who were strict in specific identification. But here, again, this conservatism is only applied to *Ammonites*: in Brachiopods there are catalogued under *Spiriferina* all those species which Davidson was so doubtful about admitting. This is surely inconsistent.

In conclusion, we must express our sincere regret that a volume which contains an immense amount of valuable work should have been disfigured by palæontological inaccuracies arising from a laxity in the use of specific names. We regret it for the author's sake; because it too often happens that inaccuracies in the part about which he can form an opinion induce the reader to condemn the rest, of which he knows naught. Though excusable, this is often unjust.

S. S. B.

POEMS. By Arthur Christopher Benson. 8vo. Pp. xvi. and 192. London: Matthews & Lane, 1893. Price 5s. nett.

SCIENTIFIC people are often said to have no care for literature. This is surely a libel; at any rate lovers of nature will do well speedily to make the acquaintance of this little volume. In days when so many write for the sake of writing, and to be read by other writers, it is a pure pleasure to chance on a poet who takes us back again to the simple, strong heart of the world without intervention of Grub Street fripperies. Other poets have incidentally cast flashes of light on our ordinary surroundings, making for us beauty where none was seen before; but Mr. Benson takes the common objects of the field as the very text of his songs. Berries of Yew, Old Nests, The Mole, The Dandelion, Knapweed: such are some of the subjects on which he writes with the insight born of love. Not often does he deal with the deeper questions of natural science, but the following lines from the very beautiful poem addressed to A Child touch on the mysteries of inheritance:—

Old signs are written in thy tender face,
Desires, regrets that thou hast never known.
Thou art the heir of thy aspiring race,
Heir of a troubled throne,

Of hope, that hardly dost portend the morn,
And sadness, that hast scarcely guessed at pain;
God takes the characters of fate outworn,
And writes them fair again.

In his minute observations of nature, Mr. Benson reminds one of Gilbert White, and we venture to quote the following sonnet on the naturalist as of interest to our readers, and as giving some idea of our author's poetic method :—

Thou wast a poet, though thou knew'st it not
Then, on a merry morning, when the thrush
Fluted and fluted briskly in the bush,
And blackbirds whisked along thy garden plot,
Didst watch an hour beside thy hanger's foot
The quivering kestrel hung aloft the skies
To mark aught stirring, or with pensive eyes
In cherry-orchards didst forecast the fruit.
And shall I deem it idle thus to scan
The myriad life, and reverently wait,
A patient learner, auguring, behind
The restless hand, the unhesitating mind?
This was thy daily task, to learn that man
Is small, and not forget that man is great.

Space does not permit us to quote more; but we cannot conclude without offering our thanks to Mr. Benson for the pleasure he has given us. F. A. B.

INDEX TO THE PERIODICAL LITERATURE OF THE WORLD (COVERING THE YEAR 1892). By Miss E. Hetherington. 4to. London: *Review of Reviews* Office, 1893. Price 5s.

THIS valuable reference book has reached its third year and, as Mr. Stead truly says, we owe much to the industry and enthusiasm of Miss Hetherington and her assistants. We do not propose to criticise so useful a book; and, limiting ourselves to our own department, we offer a few suggestions for the next year's issue. In the list of "English and American Magazines and Reviews," pp. 8–20, we fail to find either the *Annals and Magazine of Natural History*, the *Geological Magazine*, or the *Zoologist*, while in the "Classified Tables" we only find two of them. In the "Index to the Periodicals," pp. 72–180, also, these three periodicals are conspicuous by their absence. As they contain some of the best work in the natural history sciences, and are, moreover, among the recognised channels for British workers, this seems somewhat strange. They should appear in all the lists, and should decidedly be indexed.

In future editions the journal *Yn Lioar Manninagh* should appear under L, not Y.

"MINERVA, Jahrbuch der Gelehrten Welt," edited by Dr. R. Kukula and K. Trübner (Trübner, Strasburg, price 7 marks), has made its appearance for 1893-4. It is a most valuable book of reference for those who wish to know the names and addresses of their fellow-workers, although it does not profess to include those unconnected with some public institution. The present volume is as neat in its get-up as its predecessors, and is embellished with an admirable engraved portrait of Louis Pasteur.

"An Astronomical Glossary or Dictionary of Terms used in Astronomy," by J. E. Gore, has been issued by Messrs. Crosby Lockwood & Sons. Students of Natural Science, especially those interested in Cosmogony, and in theories of climatic change, may be glad of a handy book of Astronomical terms. The definitions are for the most part brief. In the case of meteoric stones—"stones which occasionally fall from the sky"—a little more might have been added. Moreover, we may add the three kinds, Siderites, Siderolites, and Aerolites, were named by Maskelyne, not Denning.

The new edition of the Tunicata in Bronn's "Klassen und Ordnungen des Thier-Reichs" is to form the supplement to bd. iii. (Mollusca), and is edited by Dr. O. Seeliger, of Berlin. Lieferung 1 (48 pp.) lies before us, and is devoted to the first four, and part of the fifth, sections of the historical introduction, which is treated in the same thorough manner that characterises the main section of the volume.

M. Paul Pelseneer's new work, entitled "Introduction à l'Etudes des Mollusques," will, we understand, shortly appear. It is to form a single 8vo volume with 146 figures in the text, and will be published by M. Lamertin, of Brussels, at a price (6 fr.) which should put it within reach of all students of Malacology.

The Royal Swedish Academy of Science has just issued part i. of a detailed monograph of the Silurian Crinoidea of Gotland. This work, on which Mr. F. A. Bather has been engaged for the past four years, should prove of service to the numerous collectors of our own Wenlock fossils.

We have just received a number of publications from the United States Department of Agriculture. That of the most recent interest is volume ii. of illustrations of North American Grasses, dealing with the grasses of the Pacific slope, that is, of California, Oregon, Washington, and the north-western coast, including Alaska. It was prepared by the late Dr. George Vasey, and was, probably, the last official work on which he was engaged, as his letter of transmittal bears a date but little prior to that of his death. It contains plates with descriptions of one hundred of the more important species. The former are among the nicest specimens of grass illustrations that we have seen.

We are glad to learn that Dr. William Fraser Hume's pamphlet on the Cretaceous sediments, which was reviewed on p. 228 of our last volume, has now been published, and can be purchased from F. H. Butler, 158 Brompton Road, London, S.W.

OBITUARY.

PAUL FISCHER.

BORN JULY 7, 1835. DIED NOVEMBER 29, 1893.

BY the death of Dr. Paul Fischer, malacologists have suffered a heavy loss, and one that will not be easily replaced. He was for many years assistant naturalist to the chair of palæontology in the Jardin des Plantes, Paris, also a prominent member of the Commission of Submarine Dredging, and a member of several scientific societies. He does not, however, appear to have been a very ambitious man; and was quite unknown to many of his correspondents except through his writings.

As an author he was most prolific, and although he occasionally wrote on some of the lower forms of vertebrates, the mollusca claimed practically all his attention. And he was not a mere describer of shells; it is quite clear that whenever the animal of an interesting genus fell into his hands, a contemporary journal invariably contained something original from his pen concerning its anatomy. His *Observations anatomiques sur les mollusques peu connus*, a series of articles written in the *Journal de Conchyliologie* in 1856-57; *Ueber die Respiration bei den mit Lungen versehenen Land-Gasteropoden; Anatomie du genre Septifer; Anatomie de deux Mollusques pulmonés terrestres appartenant aux genres Xanthonyx et Hyalimax; Note sur l'anatomie des Cyrènes Américaines*, and many other memoirs of a like nature all testify to his thorough acquaintance with the soft parts of his favourite group. Occasionally we find him writing about bryozoa, hydrozoa, sponges, brachiopods, etc., but his interest in them seems to have been merely a passing one, kindled, perhaps, by the contents of his dredge. Sometimes geology claimed his attention, though this was generally due to some collateral influence. Thus he gives us a *Note sur la géologie du Sud de Madagascar*, in 1868; *Sur l'existence du terrain tertiaire inférieur à Madagascar*, etc. Palæontology does not seem to have had much attraction for him in his minor writings, but his knowledge of the fossil mollusca was comprehensive enough, as we shall presently see.

Altogether he has written more than 300 separate memoirs and papers, and, in addition, 100 memoirs in conjunction with MM. H. Crosse (his co-editor), Delesse, Tournouër, Bernardi, and others, published for the most part in the *Journal de Conchyliologie*.

Among his more important works we may mention the descrip-

tion, in conjunction with M. Crosse, of the mollusca collected by the Scientific Mission to Mexico and Central America; with M. Tournouër, the invertebrate portion of the fossils of Mont Léberon, in Vaucluse; and accounts of the mollusca collected during the voyages of "Le Travailleur" and the "Talisman."

Dr. Paul Fischer, in spite of the numerous writings above-mentioned, is best known as the author of the celebrated *Manuel de Conchyliologie*, published in Paris (1880–87). The appearance of this work at once stamped the author as one of the most eminent malacologists the world has ever seen. It is based on the well-known manual of S. P. Woodward, and in fact the plates illustrating it are identical with those in the English work referred to; but although the plan is the same, it is naturally a much larger work, and must have entailed years of labour. It is here that we first become acquainted with the fact that Dr. Fischer knew the fossil mollusca quite as well as the living forms, and seeing how little he had previously written on the matter, this must have been a surprise to all but his most intimate friends. To say that the work is perfect is to ignore the insuperable difficulties of the subject; but it is now, and will for some years remain, the principal text-book on recent and fossil mollusca.

Dr. Fischer was a man of somewhat reserved habits, but was always willing to render assistance to those who consulted him, and they were not few. He had been ailing for a long time, and passed to his rest on the 29th November last.

G. F. H.

GEORGE GORDON.

BORN JULY 23, 1801. DIED DECEMBER 12, 1893.

WE regret to record the death of this veteran geologist, though of course, at his advanced age, his life could not long have been spared. He was son of the Rev. William Gordon, and was born in the old manse of Urquhart, near Elgin. Entering Marischal College, Aberdeen, in 1815, he graduated M.A. in 1819. After studying Divinity both at Aberdeen and at Edinburgh, he was licensed by the Presbytery of Elgin in 1825, and was presented to the parish of Birnie, by Francis, Earl of Moray, in 1832. For 57 years he was minister of Birnie, retiring in 1889 to Brae Birnie, a house in Elgin. He was made an LL.D. of Marischal College in 1859.

He had early developed a taste for Natural History and Antiquities, and eventually became one of the most enthusiastic of Scottish geologists. In 1839 he published "Collectanea for a Flora of Moray," and later on contributed to the *Zoologist* a series of papers on the Fauna of Moray.

In the great question of the age of the Elgin Sandstones, Dr. Gordon took the deepest interest, and to the last he maintained that

they all belonged to the Old Red group, and that the Reptiles, generally considered to be Triassic, had appeared in that earlier period. To his energy we owe the grand collection of remains of these Reptiles, some of which were described by the Right Hon. T. H. Huxley, and others, even more remarkable in character, quite recently by Mr. E. T. Newton. One of the new genera has been named *Gordonia*. Dr. Gordon was mainly instrumental, some sixty years ago, in founding the Elgin Museum and Institution.

Until within a fortnight of his death, Dr. Gordon was in vigorous health for a man of his years. He was over six feet in height, and consequently a man of commanding presence; while his enthusiasm in Science was maintained to the last, and those who had the privilege of accompanying him to the Elgin quarries, when he was over ninety years of age, will never forget his wonderful energy, his geniality, and intellectual vivacity.

THE deaths are also announced of L. CHABRY, the well known investigator of experimental teratology; of HENRY RINK, author of geographical and ethnological works on Greenland; of D. SCOTT MONCRIEFF, an American ethnologist from Harvard University, who was accidentally drowned while travelling in Siberia; and of JUAN VILANOVA Y PIERA, the well-known geologist of Madrid.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. A. Baldacci, of Bologna, has returned from a botanical tour in Crete.

Dr. A. Terracciano has resigned his post as Conservator at the Royal Botanic Institute at Rome; Dr. O. Kruen has been appointed to fill the vacancy.

The prize of 1,000 fl. instituted in honour of Dr. T. Mayer has been awarded by the Academy of Sciences in Cracow to Dr. M. Raciborski for his work on the fossil flora of Polland.

The following awards have been made among French botanists: M. Gaston Bonnier to be "Chevalier de la Legion d'honneur," M. Leclere Savlon to be "Officier de l'Instruction publique," and M. L. Planchon to be "Officier d'Academie."

Professor E. D. Cope has been appointed to the Professorship of Comparative Anatomy and Zoology in the University of Pennsylvania, and Dr. A. P. Brown succeeds him in that of Geology and Mineralogy.

Mr. E. A. Minchin, B.A., Radcliffe Fellow of the University of Oxford, has been elected to a Fellowship in Biology at Merton College. Mr. Minchin's original work has hitherto lain chiefly among sponges and echinoderms; he has been engaged of late on an English translation of Professor O. Bütschli's monograph on the foam-structure of protoplasm, which has just been issued by the Clarendon Press. We understand that Mr. Minchin will reside in Oxford, continuing his researches, and giving lectures.

Mr. Arthur Willey, B.Sc., now lecturing in Columbia College, New York, has been elected by the special board for biology and geology of Cambridge University to the Balfour Studentship. The first award of the Walsingham medal, founded by the Lord High Steward for the encouragement of biological research, has been made to Mr. E. W. MacBride, who is well known by his work on Echinoderm embryology. Mr. Willey is being sent to New Ireland to investigate the early development of *Nautilus pompilius*. Could not Mr. MacBride be directed to start for Jamaica or Japan to study the embryonic and larval stages of *Pentacrinus*?

We have received No. 2 of the *Kansas University Quarterly*, which contains two entomological and two palæontological papers, and an account of some experiments on the delicacy of the sense of taste among Indians. The most important of these contributions are Dr. Kellogg's description of the head of a butterfly (*Danais archippus*), and Professor Williston's brief illustrated notes on the skeleton of Pterodactyls and a Mosasaur from the Chalk of Kansas.

THE Duke of Westminster has subscribed the sum of £1,000 to the extension fund of the Grosvenor Museum, Chester. The new portion of the building is already well advanced.

THE Manchester Museum has issued a second edition of its illustrated General Guide. Among the most recent acquisitions is a collection of the Coal-measure fossils from Lancashire, presented by Mr. Robert Cairns. A similar collection was presented some months ago by Sir U. Kay Shuttleworth.

THE Société Industrielle de Mulhouse has just issued a guide to the Mulhouse Museum. Among many other important collections therein contained, we may note the geological specimens of Kœchlin-Schlumberger; the plants and marine fossils of the Culm of Thann, collected by Albert Scheurer; the Debay collection of fossil plants from the Senonian of Aix-la-Chapelle; the quaternary mammals from the Mulhouse district; and the Mühlenbach, Rabenhorst, Schlumberger, and Han herbaria.

WE regret to learn from the recently-issued Annual Report of the Brighton Museum, of the retirement of Mr. Edward Crane from the chairmanship of the Committee. Mr. Crane has served on the Committee for twenty-one years, and was elected chairman eight years ago on the death of Dr. Thomas Davidson. During this long period he has not only rendered inestimable service to the Museum, but has also been one of the foremost promoters of the interests of Science in the town; and his resignation is only due to the feebleness caused by advancing years. There is appended to the Report a scheme for the continuation of the Booth Collection of British Birds. 220 species are already represented, and a special effort is to be made to gradually complete the series.

THE address of the President, Mr. J. F. Whiteaves, to the Royal Society of Canada, section iv., has just been issued separately. It is an account of our present knowledge of the Cretaceous Rocks of Canada.

THE Palæontographical Society announce the preparation of a monograph on the "Anthracosias and Anthracomyas of the Coal-measures," by Dr. Wheelton Hind. As Dr. Hind has already dealt with many of these forms in vol. xlix. of the *Quarterly Journal of the Geological Society*, we have good hopes that this monograph may actually, as the Council promise, "appear at an early date."

THE North Staffordshire Naturalists' Field Club has issued a concise and valuable report on the progress of geological and palæontological research in the district, with a bibliography from 1679 to the present day. It has been prepared by Mr. John Ward, of Longton.

THE Liverpool Geological Association have issued no. 5 of vol. xiii. of their *Journal*. It contains a paper by Mr. J. Herbert Jones on the influence of local geology on the commercial importance of Liverpool, and a short discussion of the metamorphic origin of granite by Mr. R. W. Boothman Roberts.

THE formation of Field Naturalists' Clubs in France seems to be on the increase. We learn from *La Feuille des Jeunes Naturalistes* (December 1, 1893) that a new Natural History Society has recently been founded at Mâcon, while similar societies are in process of formation at Bourg and in Haute-Saône. There is also

an Entomological Association at Armentières, and an Union des Naturalistes de la ...e, which latter has its headquarters at Clichy, and for its principal object the ...y of entomology.

The Edinburgh Geological Society have just completed the sixth volume of their *Transactions*. The fifth and concluding part contains several papers of interest and importance, notably an anniversary address by Mr. H. M. Cadell, dealing with the geological changes wrought by man within the Forth basin, in which changes Mr. Cadell and his forbears seem to have played a prominent part. Mr. Cadell also publishes a map of the site of Edinburgh in ancient times, showing no less than seven lakes. This paper, as well as some Notes on a Shell Mound at Tongue Ferry, Sutherland, by Messrs. Peach and Horne, will interest archæologists as well as geologists. The latter authors also draw attention to some Shelly Boulder Clay in North Ronaldshay, Orkney, which bears on the development of the Scandinavian and Scottish ice-sheets during the climax of glacial cold. The part concludes with the first half of an important paper by Dr. H. J. J. Lavis on the Ejected Blocks of Monte Somma, which deals chiefly with those consisting of more or less altered stratified limestone.

The annual Soirée of the Croydon Microscopical Club was held on the 22nd November, at the Public Hall, Croydon. There was a good attendance, and the exhibits were up to the average of former years. The most noteworthy thing for criticism, however, is the lamentable falling-off in the exhibition of microscopes. Formerly the Croydon soirée was one of the best of the season, and not only did the microscopes fill all the tables in the large hall, but the exhibition of objects was first-rate. For the last few years the microscopes have been dwindling in number, and the exhibition has become poorer and poorer, until we now see one complete table in the hall filled with butterflies, and, with but few exceptions, as poor an exhibition of microscopy as can be imagined. The objects are inferior, the manipulation of light is bad, and all the old prestige of the Croydon as a microscopical club seems tottering to its fall. Cannot a few energetic members pull it together once again, give a few lessons in illumination and general technique to the members, and restore the microscopical and the more scientific element?

CORRESPONDENCE.

Emendations of Nomenclature.

With all due deference to the Editor, I must confess to agreeing with "Macaulay's Schoolboy," that in certain cases the emendation of generic and specific names is not only pardonable but imperative. Such small alterations as the addition of an aspirate or the change of a letter can scarcely affect the intelligibility of a name. Take, for instance, *Oxynoticeras* which should be *Oxynotoceras*,—*serrodens, serridens*,—*heterogenum, hetevogenes*. These alterations are desirable, though I own that, copying others, I have allowed the incorrect forms to pass. Again, what warrant is there for the declension of *nanus* and *capricornus* as adjectives, giving them the endings *a, um* when they are substantives? Is not *Straparollus* derived from a man's name, and, therefore, to be written *Straparollia*, so as to avoid sending anyone on a wild-goose chase through Latin and Greek dictionaries for its derivation? As to more serious alterations, the proposed changing of barbarous compounds of a man's name with *ichthys* or *ceras* into mere *ia*, I personally agree with, but I consider, if they are to be carried out, that there should be a statute of limitations in those cases—say 20 years; and that older names should be exempt. Although scientific nomenclature is for scientific men, it is certainly desirable that they should do their best to conform to classical usage.

S. S. Buckman.

Elementary Zoology.

Can you or any of your readers recommend me a cheap but correct book dealing with the general anatomy of common marine animals, such as are commonly preserved in the fossil state? I would specially instance the mussel, cockle, oyster, whelk, cuttle-fish, sea-urchin, sea-anemone (as an example of an Actinozoan), crab, lobster, some brachiopod and *Flustra*, or any common Bryozoan. One is constantly being asked about some such book, but the few that I know are either too expensive, too old-fashioned, or too technical for the beginner.

207 Harrow Road, W. F. A. Bather.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan & Co., *29 Bedford Street, Strand, London, W.C.*

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

Education by Experiment.

THE young human animal is born into this world with a great curiosity and a great desire for knowledge. It longs to touch, to taste, to try in every way the properties of things around it. On its lips is from the first the catch-word of our race, the everlasting "Why?" What do we do with this inquisitive creature? We box it up in a room and set it to learn the A B C; we cramp its flexible hands with a pen; we dull its brain with verses and the multiplication table, both learned by rote; we tell it not to be tiresome and sternly repress all child-like efforts to see the wheels go round. Thus the great work of education progresses: mountains and rivers are learned in lists, sums are done by rule, geometry is repeated as an effort of memory. Then the boy, and sometimes the girl, must learn physical science. Soon he will know the laws of heat, the composition of the spectrum, the names and atomic weights of the elements, the succession of geological formations, and the classification of the animal and vegetable kingdoms; but he cannot get a fire to light, is ignorant that a tadpole turns into a frog, and gives himself a stomach-ache with a poisonous fungus. As for poor science, it is called "Stinks," and the teachers of it are naturally despised. The youth is turned out an ignorant prig or a learned trifler, the country goes to the dogs, and even the toys of the next generation are "made in Germany."

Thus we write to-day; but three centuries ago Montaigne said the same thing: "Seeing how we are taught, what wonder is it that neither pupils nor masters become more able, although they make themselves more learned. In truth, the care and expense of our fathers serve but to furnish our heads with knowledge; of judgment and virtue small news is there. . . . The question should be, whose learning is the best, not whose is the greatest." And yet it moves. Our children, if not ourselves, have already the benefit of a far more

rational system of education; one that does really lead out and train up the natural faculties of the race and of the individual, instead of coldly crushing them.

Much, however, remains to be done—in the teaching of Natural Science, quite as much as in other subjects. We must not make a bogey of Nature, but a friend, one with whom our children can converse, whom they can question for themselves, and whose tales they may delight to hear. Of course, the old way was easier—for the teacher. The new way takes time, and its results will at first be hard to estimate in either marks or money. At the outset teachers must be taught and examiners instructed; we all want a guide.

With the hour has come the man. Our guide waits for us in the person of Mr. Hugh Gordon, the science demonstrator to the London School Board; while as a guide-book we have his "Elementary Course of Practical Science," of which part i. has just been issued by Messrs. Macmillan at the price of one shilling. This is a book to excite enthusiasm in the heart of every teacher worthy of his post. Slight though it be, it is a step, and a long step too, in the right direction. Mr. Gordon's method is the method of Nature; he would educate the child as Nature has educated the race. Take nothing for granted! Ask "Why?" Try for yourself! Prove each step! Be clean, careful, accurate! Check your results! Such are the precepts that Mr. Gordon shows us most admirably how to put into practice. Exactly how he does this we do not intend to divulge. The book, though it would take many weeks to work through, can be read in less than an hour; and no hour could be better spent by all who, whether as parents or teachers, have the inestimable privilege of bringing young minds into touch with this wide and wonderful world.

Mathematical Biology.

A distinction is often drawn between the exact sciences and the natural history sciences, on the ground that only the former are capable of mathematical expression and demonstration. Attempts have of course been made at different times to subject the growth or the form of animals and plants to mathematical analysis; but, as a rule, the amount of variation that is so characteristic of living beings has baffled the enquirer. Now, however, observers are attacking the elusive quantity itself, and endeavouring to define it in terms of mathematical precision. The last number of the *Proceedings of the Royal Society* contains two remarkable papers bearing on this subject. The one is "On certain Correlated Variations in *Carcinus mænas*" by Professor W. F. R. Weldon; the other, "Contributions to the Mathematical Theory of Evolution" by Professor Karl Pearson.

It is recognised that no two animals are exactly the same, and that their organs vary irregularly. Professor Weldon has, however,

shown "(1) that the observed deviations from the average size of every organ measured are grouped symmetrically about the average, and occur with a frequency corresponding closely to that indicated by the probability integral; and (2) that the 'degree of correlation' between a given pair of organs is approximately the same in each of five local races of the species." (*Proc. Roy. Soc.*, vol. xlvii., p. 445; and vol. li., p. 2). He has now measured specimens of the shore crab in two lots, one from Naples and the other from Plymouth, and after an elaborate comparison of the correlated variations between different organs, he shows that, though variation may differ, yet the amount of *correlation* is the same for both groups.

The method followed by Professor Weldon in determining the degree of correlation between two organs was that proposed by Mr. Francis Galton in his paper on Family Likeness in Stature (*Proc. Roy. Soc.*, vol. xl., pp. 42–63), and it appears that the results obtained by that ingenious anthropometer are fully confirmed by the study of so remote a group as the Arthropoda. The suggestion has recently been made that "the method of composite portraiture, as developed by Mr. Francis Galton and others, might be employed with advantage to discover a type or standard from which degrees of variation within the limits of a species might be measured in positive or negative terms" (Bather, "Crinoidea of Gotland," p. 5). This, perhaps, is often impracticable, but the construction of a diagram from the average of numerous measurements would serve a similar purpose. Such a diagram of the carapace of the crabs in question is actually given by Professor Weldon. The diagram is drawn to scale, the right half representing a perfectly average Plymouth crab and the left an average crab from Naples.

Professor Pearson's contribution to the subject is a mathematical analysis of certain curves that may be constructed from a series of measurements of a number of objects of the same type. Such curves, which he terms *frequency curves*, are often abnormal, especially in biological measurements, such as those of Professor Weldon, or the similar ones of Thompson for prawns, and Bateson for earwigs. Although abnormal, however, they are subject to certain laws, and from a consideration of these Professor Pearson believes it possible to detect whether the individuals measured represent a mixture of two heterogeneous groups, or whether they represent one homogeneous species gradually being evolved into two differentiated species. Thus he finds that the Naples crabs are all of one species, but that they are breaking up into two varieties owing to evolution in their foreheads. Mr. Thompson's prawns, on the other hand, either have a small percentage of individuals deformed in the carapace, or else have a small but unstable giant population mixed with the normal population.

These modern mathematical investigations differ widely from the fine-drawn and metaphysical "pro-morphology," as Geddes calls it, of the old German nature-philosophers. As to their value, hear Pro-

fessor Weldon:—"It cannot be too strongly urged that the problem of animal evolution is essentially a statistical problem: that before we can properly estimate the changes at present going on in a race or species we must know accurately (*a*) the percentage of animals which exhibit a given amount of abnormality with regard to a particular character; (*b*) the degree of abnormality of other organs which accompanies a given abnormality of one; (*c*) the difference between the death rate per cent. in animals of different degrees of abnormality with respect to any organ; (*d*) the abnormality of offspring in terms of the abnormality of parents, and *vice versâ*. These are all questions of arithmetic; and when we know the numerical answers to these questions for a number of species we shall know the direction and the rate of change in these species at the present day—a knowledge which is the only legitimate basis for speculations as to their past history and future fate."

JAMAICA AND SCIENTIFIC FEDERATION.

THE Institute of Jamaica has just issued its Annual Report for 1892–3. Under the late curatorship of Mr. T. D. A. Cockerell, the Museum seems to have advanced both in scientific importance and popular favour, although economic entomology has taken up much time that might otherwise have been devoted to collecting. The list of 32 specialists in various parts of the world to whom specimens were sent for identification occasions the following excellent remarks: "It is by means of this correspondence, and through publications, that the Institute becomes of international importance; and it is submitted that no museum can be efficient as a local exponent of scientific principles unless it is in this sense international. For this reason, although the Museum is supported by the people of Jamaica, it is felt that a naturalist in New Zealand or Japan is as much entitled to information or specimens (provided they are for the purpose of serious study) as a resident of this island. This principle is so well recognised among naturalists that assistance is asked of any specialist, with the full confidence that, if possible, it will be granted. To cite an example. The Institute has been continually indebted to the United States Department of Agriculture for all sorts of help, which has been given as freely as if Jamaica were United States territory."

How strong a contrast are these words to those which, on another page of the report, the Board feel compelled to use concerning the attitude of the people of Jamaica towards an institution that should be far more intimately connected with them! "The interest taken in the Imperial Institute in America is," they say, "of a very discouraging character." This is to be regretted, but also to be excused. What does a Jamaican arriving in London first learn of this

magnificent building and its extensive organisation? He sees on a newspaper placard :—

IMPERIAL INSTITUTE.
SMOKING CONCERTS.

COUNSEL'S OPINION.

HORACE WOOLLASTON MONCKTON, F.L.S., F.G.S., of the Inner Temple, Barrister-at-Law, has kindly sent us a reprint of some "Short Papers," samples apparently drawn at hazard from the numerous compartments of a many-sided mind. Among articles so diversely interesting to the man of science as "Fish Culture at Howietown," "Legitimation," "Trial by Wager of Battle," and—but no, we dare not add "The Bagshot Beds of the London Basin"—there is one on a subject we have occasionally dealt with, "Women as Fellows of Scientific Societies." As a presentment of the question in its legal aspect, this paper may save many of our learned paupers the lawyer's fee that they could so ill spare. Mr. Monckton's opinion is that, assuming women were not mentioned in the original charter, and had not for some 60 years been admitted to a given Society, then, if it were wished to admit women, a new charter would be needed to make their election binding in law. The author's words are Delphic in obscurity and Scottish in caution:—"There is a new charter necessary—some may be of opinion that is—but this seems going rather far—suppose every fellow of the society wished to alter the usage, and there is nothing in the charter against the proposed alteration—a renewal of the charter seems somewhat superfluous. It has, we believe, been suggested by high authority, that a meeting of the fellows specially called for the purpose, may decide the question by a majority of those present, and perhaps that is a fairly practical view of the question. In any case we may perhaps say with safety that if a majority of a fairly representative meeting, after full notice, decided to alter the usage and admit women as fellows, the courts would be slow to interfere with such a decision."

THE GROWTH OF A MADREPORE CORAL.

MR. G. C. BOURNE has returned to his early love, and in the *Scientific Transactions of the Royal Dublin Society* (vol. v., pp. 205–238, pls. xxii.–xxv.) gives an exceedingly interesting account of those young stages in the development of the Madrepore coral *Fungia* that succeed the embryo-stage. The extreme difficulty of investigating organisms in which the soft body-tissues are so closely inter-penetrated by hard skeletal tissues, or stereom, is paralleled by the difficulty of describing the results in brief yet intelligible language, and the difficulty is not lessened by the use of the new terms that Mr. Bourne has found it advisable to invent. To only one point can we now direct attention. The large, flat coral, which has been called *Fungia*

from its likeness to the inverted head of a common mushroom, resembles, in its earliest fixed stage, the young of the ordinary cup coral, *Caryophyllia*. This cup-like form may remain solitary and gradually develop into the adult individual, or it may produce buds from its wall, which also develop into individuals and thus combine with the original individual to form a colony. In this way there is often produced a kind of stalk which carries many developing individuals. But after a time these latter become separated from the original stalk and set up for themselves, just as though Brussels sprouts were to fall off from the stalk on which they grew and to finish life as independent cabbages. Here is a difficulty: the various cups are joined to the stalk not only by soft living tissue but by solid, calcareous tissue or stereom; how, then, does this stereom become divided? Mr. Bourne finds that the coral is attacked, chiefly in this particular region, by various parasites, notably one of vegetable nature. The borings made by these and the limy matter deposited by them in place of the original stereom, render the junction opportunely brittle. It is, however, hardly likely that parasites should conduct their attacks solely for the convenience of their host, without some predisposing cause. Such a cause Mr. Bourne finds in the degeneration of the soft tissues in this region, and he infers that this degeneration of the soft tissues has some weakening effect on the stereom, whereby it is rendered more liable to the attacks of parasites. Similar changes are seen in shells removed from the influence of living tissue, but it is hard to learn the exact nature of the effect that the proximity of soft parts has on stereom. Mr. Bourne suggests "that the shell or coral is in some way affected by its contact with an osmotic surface, and that after separation a molecular change takes place, involving a re-arrangement of the crystalline structure." In the case of shells, however, it should be remembered that the calcite is deposited in organic membrane, which, it may be assumed, is more readily affected in this manner than would be the case if it were homogeneous as coral stereom is thought to be. It is possible that some such organic substratum may yet be discovered in the coral stereom, and this would render Mr. Bourne's explanation still more credible.

The vegetable parasite that proves so useful to *Fungia* is called by Mr. Bourne, *Achlya penetrans*. *Achlya*, however, is a genus of the Saprolegnaceæ, which are not known to live in the sea, and are in fact always destroyed by salt water. On the other hand, it has been shown that certain green algæ infest corals and marine shells in this manner, and we would suggest that the form described by Mr. Bourne is probably allied to the genus *Gomontia* of Bornet. It seems as though the botanists had kept their researches in this direction a little too much to themselves, and we hope to induce some authority on the subject to retail his knowledge in a future number of NATURAL SCIENCE.

Geology of New Zealand.

The Minister of Mines for New Zealand has recently issued his statement for 1892–3 (Wellington: 1893). The only portion of any scientific interest is an account by Mr. Alexander McKay of "Geological Explorations of the Northern part of Westland" (pp. 132–186). The country of Westland is on the west coast of the province of Canterbury, and includes the western slopes of the Southern Alps, and the lower grounds between the mountains and the sea from the Grey River to the northern boundary of Otago. The rocks exposed in the district, as shown on the geological map that accompanies the paper, are of very varied ages and character; they include Quaternary and Tertiary gravels, clays and sands, a whole series of Cretaceo-Tertiary limestones, sandstones, and marls, some diabasic ash-beds apparently of Triassic age, Carboniferous slates, slightly metamorphosed Devonian rocks, and other metamorphic and igneous rocks of which the age is uncertain.

While considering the disposition and origin of the auriferous gravels for the practical purpose of gold mining, Mr. McKay finds it necessary to investigate the former distribution of land in this portion of the South Island, and he concludes that all its great physical features are of Pliocene or post-Pliocene origin. He believes, indeed, "that a mighty mountain-range rose from the sea, possibly towering to heights far above the limits of eternal snow, since the Miocene period, and that its central and western parts were subsequently depressed, till the ocean removed or now rolls over its highest peaks." And he supposes that these changes took place as follows:—"During Tertiary times, up to the close of the marine deposits of the Miocene period, New Zealand was greatly depressed, and by far the greater part of its present area was below the level of the sea. At the period of greatest depression, Von Haast sketches us the South Island as consisting of a line of rock-islets formed by the outstanding higher peaks of the main range of mountains; but it is not clear that even these stood above the water-level. And if it be contended that a consideration of the fauna and flora of these islands at the present day, in comparison with what characterised former geological periods within the same area, necessitates the uninterrupted existence of land, it does not follow that this land must have been the crest of the Southern Alps. Such land may have lain to the westward of the present coast-line, but more probably it lay to the eastward in the line of the older axis of New Zealand, trending in a N.W. and S.E. direction. The N.E. and S.W. line of elevation is modern compared with this other, and probably was but feebly marked prior to the elevation at the close of the Miocene period."

In a previous report (1890–91, p. 1), Mr. McKay has dealt with the mode of appearance of the main chain and other mountain ranges of the northern and central parts of the South Island. "The manner in

which the Younger Miocene and Cretaceo-Tertiary strata are tilted and faulted so as to form nearly vertical strata in contact with the crystalline and older sedimentary rocks between Kanieri Lake and Hokitika River, and the manner in which the same, and even younger beds, are involved along the western base of Mt. Greenland, and at the same time appear on the top of the mountain and far below sea-level, within a horizontal distance of some four to five miles, shows conclusively that very great displacements have taken place." Further, he claims that the various lines of dislocation prove the subsidence of vast masses of land, which formerly extended westward of Hokitika, and constituted the western wing of the great anticlime, if not the central knot of the whole mountain system. And it was this now vanished land that provided the materials of the older auriferous gravels.

Would that theory and practice might always walk so amicably hand in hand! But not every mining geologist is a McKay.

"A NATURALIST'S NOTES OFF MULL."

IN *Good Words* for December last, there is an article with the above title by one who gives the name of "Nether Lochaber." The writer remarks that "As Saint Patrick is fabled to have banished all noxious reptiles from Ireland, so, according to Adamnan, did Saint Columna banish all noxious reptiles from Iona, and so, according to a very old tradition, did Saint Maluac from the island of Lismore. It is a curious enough fact, account for it as we may, that not only is the viper not found in Iona, but even if captured ever so gently and conveyed to the sacred isle hale and hearty, from the opposite shores of Mull, where it is common, it will instantly sicken and die."

He goes on to describe how "a friend of our own, a well-known artist," carefully imported into Iona a couple of vipers. They were captured with an angler's landing-net and conveyed without injury to the island; and as soon as they were released they twisted and wriggled about in a lively-enough fashion. The artist stated that "As I was lighting my pipe, however, and preparing to sit down on a knoll above them, so as the better to watch their movements, what was my surprise to see one of them suddenly stretch himself out as straight and stiff as an ellwand, and with a visible shiver and single gasp give up the ghost! In a few seconds the other adder followed suit—died precisely in the same manner as its companion; . . . . I am now persuaded that a viper will not live in Iona."

It would be interesting to know more of the circumstances under which these observations were made. They appear to have been characterised by scarcely sufficient scientific acumen and caution to warrant so broad a generalisation.

THE argument from fossils as to former climatic conditions, has often been recognised as exposed to many chances of error.

Another possible explanation of apparent tropical conditions is suggested by Dr. V. Ball in a paper on the folk-lore of the volcanoes and hot springs of India, published in the *Proceedings of the Royal Irish Academy* (ser. 3, vol. iii., pp. 151–169). No one who has travelled in volcanic districts, where hot springs are still active, can have failed to observe that they have considerable influence on the surrounding vegetation, and even, as has been pointed out by various writers, on the fauna, both aquatic and terrestrial. It is not only the lower animals, but also human beings, who, like the Maoris and the Japanese, flourish in the neighbourhood of hot springs where the purely climatic conditions would otherwise be too severe. Dr. Ball, then, writes as follows:

"Where the fossils of animals or plants are found which seem to indicate tropical or semi-tropical conditions of the climate at the time when they lived, may it not be possible to suggest, especially if there be any facts tending to prove the existence of different climatic conditions elsewhere at the same time, that there may have been widespread fumaroles or hot springs sufficient to have produced local hot-houses in which animals and plants may have flourished, which could not otherwise have existed in the normal conditions of climate belonging properly to the time and place?"

Under certain circumstances the explanation might be plausible; but we doubt whether such circumstances have as yet presented themselves to geologists. It must be remembered that such effects would be extremely local, and that thermal activity would probably leave other traces of its former existence in the shape of chemical deposits.

In the funnel of certain Cephalopods, several authors have noticed a peculiar cushion-like organ, situated a little behind the valve, and this has, for very insufficient reasons, been called "Verrill's organ" by Hoyle and others. Its function and homology have been the subject of some discussion. Ferussac and d'Orbigny confused it with a transverse muscle; H. Müller, in 1852, thought it was a stinging organ; Verrill, in 1882, considered it "the true homologue of the foot of gastropods"; Laurie, in 1888, from rather insufficient material, showed its glandular nature, and believed that it secreted mucus, but his observations were criticised by Brock; Hoyle, in 1889, believed that it served to close the funnel. That it is really a mucous gland is now proved by the careful observations of G. Jatta (*Boll. Soc. Nat. in Napoli*, vol. vii., p. 45, 1893), who has observed it in 32 species belonging to 21 genera, thus bringing the number of genera in which it has been found from 10 to 27. He describes and figures six main modifications of its arrangement, and gives excellent drawings to show its microscopic structure in different stages of its development. He concludes that this funnel organ is a mucous gland homologous with the pedal glands of other mollusca. If this

be so, the organ must be somewhat archaic, and one would expect to find it in *Nautilus*, where, to the best of our knowledge, it has never been described.

AMONG the more important new arrivals at the London Zoological Society's Gardens are two leopards which represent the extremes of colouration to be seen in the Leopard tribe; one of them is the black variety of the common leopard, the other the so-called Snow Leopard or Ounce. The latter animal, presented to the Society, is a valuable acquisition, for the Snow Leopard is a costly animal to buy, and it inhabits a limited tract of country; hence very few specimens have been on view in the Regent's Park.

SIGNOR G. A. DE AMICIS has just published (*Boll. Soc. Geol. Ital.* 1893), "I foraminiferi del pliocene inferiore di Trinité-Victor (Nizzardo)," an important contribution to our knowledge of the Pliocene Foraminifera of Italy. One hundred and twenty-six forms are recorded, to each of which a very full and interesting synonymy is given, while only two forms are recorded as new, an evidence of the extreme care bestowed upon his work by the author, who has swept away many varietal forms recently described as new by other authors from imperfect acquaintance with the literature.

STUDENTS of cell-division will find an interesting article by E. Overton on the reduction in number of the chromosomes in the nucleus of plants, published in the *Vierteljahrschrift der Naturforschenden Gesellschaft in Zürich* (Jahrg. xxxviii., pp. 169–186, 1893).

THE *American Journal of Science* for January contains a paper by Mr. S. P. Langley on "The Internal Work of the Wind." The subject is not physiological except so far as connected with what the author calls the science of Aerodromics, which is nothing more terrible than flying. In the same number E. H. Williams, jun., concludes that they have had but one Ice Age in North America, and that one both short and recent. C. D. Walcott has a note on Cambrian Rocks of Pennsylvania, and J. B. Woodworth, while discussing the erosive action of blown sand in New England, finds it necessary to coin the word "glyptolith" for a wind-carved rock surface. Old England sees no necessity for anything less intelligible than good old English.

THE *Midland Naturalist* ended its career, as announced, with the December number, and *The Field Club* is now incorporated with *Nature Notes*. We are pleased to learn that *Science Gossip*, which ceased to appear last autumn, is shortly to be revived under the editorship of Mr. John T. Carrington, assisted by Mr. Edward Step.

I.

Neuter Insects and Lamarckism.

MR. HERBERT SPENCER, defending Lamarkian views of heredity against Professor Weismann,[1] has to maintain that the special structures and instincts of neuter insects were first developed in fertile ancestors. He is driven to this assumption, because he holds that complex evolution cannot occur without the aid of the inherited effects of use and disuse; and such use-inheritance is obviously excluded in the case of neuter bees, ants, and termites, which cannot transmit acquired characters to posterity. Of course, many of the instincts and structures of neuter insects—such as those needed for obtaining food, sheltering and feeding the larvæ, and so forth—were already present in the primitive queens or fertile females of unsocial and semi-social stages. But in many other cases—and it is these cases which more particularly demand our attention—it seems hardly conceivable that the instincts and structures could ever have been possessed by fertile ancestors. A few examples will illustrate the improbability of Mr. Spencer's assumption.

1. In some species of ants there is a caste of neuters possessing "an enormously developed abdomen which secretes a sort of honey, supplying the place of that excreted by the aphides, or the domestic cattle, as they may be called, which our European ants guard and imprison."[2] Their abdomen is so distended with this honey that they become almost unable to move. Is it credible that parents were thus unfitted for the work of multiplication by becoming living honey jars, or factories and storehouses for the general use of the community or family? Can we believe that queens or drones first evolved so remarkable an unfitness for their special functions, while still continuing the nuptial flights, and retaining the reproductive fertility which would be necessary for transmitting the effects of use and disuse to posterity?

2. In the warrior caste of neuters among the termites—and in this caste alone—the head and jaws are more than a third of the total length of their possessors, and are "almost as big as the rest of their bodies." These huge, heavy heads are carried with apparent

[1] "A Rejoinder to Professor Weismann." *Contemporary Review*, December, 1893.

[2] *Origin of Species*, p. 231.

difficulty. The fighting instincts are developed to a remarkable degree, while the capacity and inclination for ordinary work have been entirely lost. Is it probable that the parents of the race ever evolved the structures and instincts of the warrior in so pre-eminent a degree—thereby correspondingly unfitting themselves for more indispensable functions? If it be alleged that the soldier type was evolved among the males or drones, we must notice that the evolution of heavy heads and of blindness and winglessness would be inconsistent with fitness for the all-important wedding flight, so essential for the diffusion of the species and its preservation from the evils of continued close interbreeding. Such evolution could apparently only take place in proportion as male functions degenerated and disappeared—that is, only as the evolving warrior became a neuter.

3. Among sundry ants there are *three neuter castes*—such as workers, warriors, and living honey-pots, or workers, warriors, and "overseers"—and these are quite distinct in structure, functions, and instincts. Were *three* varieties of instinct and organisation present in the original male and female? Will it be suggested that there were double forms of males or females, or that the soldiers, workers, and honey-makers of *Myrmecocystus mexicanus* are mere gradations?

4. The instincts and the emotions, if we may so call them, of neuter bees centre round the queen in an astonishing manner. The evolution of these special instincts would easily *commence* in fertile daughters who helped the queen-mother at the cost of delay or neglect of their own maternal functions (a step towards the formation of a neuter caste), but could not have been thus completed in the various details of the final stages. Thus, if we allow that the fertile females or queens once loved their rivals as strongly as they now hate them, we cannot also suppose that they encouraged and compelled these rivals to fight till only one of them survived. Females could not imprison both their queen and themselves, nor release themselves only on special and suitable occasions—as is now done by the neuters. Mr. Spencer seeks to explain the "swarming" of bees as a kind of inherited reminiscence of the nuptial flight. But these neuters or workers are imperfectly developed females. If the massacre of the drones was first carried out by queens, whose murderous hatred is now directed solely against their own sex, are we also to believe that a nuptial pursuit of a *female* originated in fertile females whose frantic jealousy of rivals now leads them to pull down cells to destroy even their own daughters? The queen, part of whose sexual instincts the neuters are supposed to retain, does not pursue another bee—the drones pursue her. Are we to suppose that the imperfect females inherit the instinct of pursuit from the males? If swarming is really due to an inherited but perverted sexual instinct derived from either parent, why do the neuters carefully abstain from following their queen during the actual nuptial flight? Why do they only swarm—or take abortive nuptial flights, as we are to suppose—on quite a

different occasion, when the queen is sent forth from the hive and founds a fresh colony? And what could have thus usefully perverted or modified the sexual instinct to such different aims and ends except Natural Selection? Why need we suppose that this great ruling factor has entirely ceased to improve or evolve instincts and intelligence in neuter insects ever since the period, however remote, when the queens began to lose the all-important instincts and capacities which are now preserved only in the neuters?

5. The sterility of the neuters and its intimate association or correlation with the many important characteristics confined to these sterile insects, obviously cannot have been developed in females while they remained fertile. Such sterility, combined with such perfect development in other directions, must evidently have been evolved by the favoured survival of such queens as produced the most efficient communities. The infertility of neuters shows in the clearest possible manner that important potentialities may arise and become established in parental germ-plasm without ever being developed in the actual transmitters.

Besides the difficulty of accounting for complex upward evolution or enlargement of parts in neuters, Mr. Spencer has also to meet the converse difficulty of accounting for equally complex evolution in the direction of a co-ordinated reduction or diminution of parts and instincts. If, as he would have us believe, the one form of evolution is impossible in neuters, the other should be equally so. Mr. Spencer cannot call in panmixia, since he does not admit the reducing power of this factor, but he alleges defective nutrition as a leading cause of downward or degenerative evolution in various organs and instincts in neuters. But we must not confound a merely proximate means with the great over-ruling factor which has utilised and perfected such means as skilfully as man converted rude flints into efficient tools. How could mere quality or quantity of food have proved so wonderfully adaptive in its effects? How could stinted nutrition fit the neuters so exquisitely for their various tasks in all details, positive and negative alike? Must it not have been Natural Selection, rather than poor food, which maintained the health, strength, and efficiency of the poorly-fed neuters and their specially large brain and superior intelligence, and all such instincts and organs as are requisite for their tasks? Must not Natural Selection have been the controlling or guiding influence which so advantageously determined what useful modifications should flourish in connection with certain modifications of nutrition, and what useless organs or instincts should dwindle and disappear? In one species of ants there are two castes of workers, and, in addition to other remarkable differences, the workers in one of these castes are only a third as long as the other, and the head and jaws are reduced to a still greater extent; which is as if one set of women in a nation were sterilised and reduced to, say, three feet in height by ill-feeding while another class of women were reduced

by still poorer feeding to only one foot in height, their heads and jaws being diminished in still greater proportion. Yet these starvelings appear to be just as sound and healthy and as perfectly fitted in all details to their place in the social economy as the queens and males. Can we suppose that insufficient nutrition caused the neuters among termites and in various species of ants to become quite blind as well as wingless, while it had no such effect on the wings and eyesight of neuter bees and wasps? Will degree or quality of nutrition account for variations such as have turned two eyes into a single eye placed in the middle of the head, as happens in one caste, and in the one caste only, of certain ants? Of course, there will be some determining circumstance which decides what sex and caste each egg shall develop; and some of the varying susceptibilities of growing organisms to the influence of food might well be seized upon by Natural Selection, and might be intensified, corrected, and adaptively perfected.[1] But such an attainment of specialised fitness would be complex adaptation of instincts and organs in neuter insects by Natural Selection—a process which is hardly separable from the form of evolution which Mr. Spencer holds to be practically impossible.

If, as Mr. Spencer must assume, the workers, warriors, living honey-pots, and so forth, inherit their positive characteristics from fertile ancestors, in whom they were first developed, why has there been no similar transmission of the long-continued decay and disappearance of such capacities and structures in queens and drones? If the effects of use were transmitted, why are not the effects of disuse transmitted? If, in spite of underfeeding, the neuters, and the neuters alone, continue to inherit the former capacities of the richly-fed queen, why do they not inherit her present incompetence and inferiority? Surely underfeeding does not *strictly preserve* complicated instincts and massive organs which are no longer possessed or exercised by parents. It cannot be so discriminative or adaptive as this. Natural Selection has to be brought in as the only factor capable of thus counteracting the inherited effects of parental disuse. And if Natural Selection can effect complicated and discriminative preservation, why cannot it effect advance? If it can maintain innumerable structures of brain and body at a high level of efficiency in many essential points, in spite of the otherwise disastrous effects of parental disuse, why

[1] Mr. Spencer thinks that the sex of the drones is determined by the defective nourishment of the queen, whereas, in the case of bees, experimental observation shows that all unfertilised eggs yield drones, and that for a long time after fertilisation the queen appears to have the power of determining the sex of the eggs she lays—the female eggs developing, of course, into queens or neuters according to the food and treatment they receive. In the case of termites, Bates concludes that the soldiers and workers are distinct from the egg, and that the differences in these castes do not arise from any difference of food or treatment during their earlier stages. M. Lespès believes that he found imperfect males and females in each of these castes.

may it not in some cases raise the general level of efficiency, and so improve neuter insects independently of use-inheritance?

Those who hold that Natural Selection can preserve highly complicated efficiency by fully counteracting the lowering power of disuse (and underfeeding?) in innumerable co-ordinated points—and yet that it can do no more in the case of higher animals where no such opposition has to be overcome—must face the logical conclusion from their own premises, that the actual power of use-inheritance is precisely nothing—Natural Selection being exactly as powerful either with or without its opposition.

The cherished opinion that complex evolution is impossible without the aid of use-inheritance, is by no means widely accepted. Darwin and Wallace, co-discoverers of the principle of Natural Selection, have deliberately rejected the assumption. It seems, indeed, to be merely one of those artificial difficulties which philosophers in all ages have been wont to construct for themselves—much as Leibnitz convinced himself that the law of gravitation was impossible. The process of complex evolution may be compared to a series of competitive examinations, in which failure in any one of many subjects is fatal to success. Surely the standard of general efficiency must be raised by the rejection of the least efficient candidates. Otherwise, what is the use of competitive examinations? Selection, whether artificial or natural, cannot well be neutral in its effects. Evidently successful or selected candidates will display more than average capacity for carrying out the combined work exacted of them by the examiners. Surely a similar process in nature must similarly raise the level of the *minima* in all essential points or subjects without correspondingly preventing a raising of the *maxima*, which, of course, will often vary upwards, since organic variability is in all directions. Let us take another comparison. The strength of a chain is that of its weakest link. If chains with the weakest links are rejected, the chains that survive the test will manifestly be stronger than those that break. Rigorous elimination of weak links, without any correspondingly rigorous rejection of strong links, must improve the strength or fitness of the links in general, however numerous they may be. Organisms are such chains; and so long as defective links or *minima* are eliminated more often than superfluously strong links or *maxima*, the average of efficiency in all the links may well be a rising one. If defective parts are incessantly eliminated (together, of course, with the organisms which they ruin) Natural Selection may gradually raise the standard of efficiency in *all* the necessary co-operative parts or organs. Such a process cannot fairly be described as an oft-repeated "fortuitous concourse of variations"; for all that is needed is ordinary variations and their extinction, so far as they are hostile to general efficiency and success.

Mr. Spencer at one time represented the chances against complex evolution by Natural Selection as "infinity to one." He has since

more moderately put down the chances against a certain combination as "millions to one," and against a still more complex combination as "billions to one." But a million millions are but ten multiplied into itself eleven times. The selection or survival of one out of ten during only a dozen consecutive generations, is equivalent to the selection or survival of one out of a potential billion. The survival of one out of two for forty generations gives a similar result. Allowing for atavism and many other hindrances which will seriously retard the rate of evolution of complex efficiency, what objection can there be to the supposition of slow complex evolution by survival of the fittest during, not a dozen or forty generations, but a thousand or a million?

Mr. Spencer contrasts the facts that varieties of dogs cannot be produced by differences of nutrition, and that differences of nutrition are known to determine the caste in some of the social insects. He holds that this proves that, in the case of social insects, "the production of their various castes does not result from the Natural Selection of varying germ-plasm." But what else could have evolved the highly peculiar potentialities, or sets of potentialities, of a germ-plasm capable of developing not merely into either of two sexes (which is common to dogs and other animals), but into one, or two, or three additional neuter castes besides? The absence or non-evolution of special susceptibilities to the influence of nutrition in dogs is no disproof of their evolution by Natural Selection in bees and ants. On the contrary, it seems to show that a long process of evolution must have been required for their development in social insects. No such additional complexities and susceptibilities of the germ-plasm appear in mammals, where there have been no special means or steps by which Natural Selection would be likely to adequately favour and evolve them. They only appear in certain egg-laying organisms which feed their helpless larvæ to maturity, so that the chances of the evolution of neuter helpers in the exceptionally arduous maternal task are of a highly favourable character. Delay or loss of the reproductive portion of the maternal functions in some of the offspring, if combined with early appearance of the nursing instincts, would be an advantage in rearing a numerous family of younger sisters and brothers, and this would greatly promote the success of the queen and her fertile progeny. A dog could not be thus readily or materially assisted by her pups either in the growth of embryos within her, or in the suckling or feeding of the young after birth. She does not rear completely helpless offspring to complete maturity; her family breaks up at an early period. The evolution of mammals has been on different lines. They feed their offspring for a time, indeed, but by no means to maturity, as is necessary where the entire larval or caterpillar stage has to be provided for until the full-grown imago or perfect insect emerges. As neuter castes are *only* found where Natural Selection has had special opportunities or facilities for such evolution, we may

very properly infer that Natural Selection has been the determining cause of such evolution.

Neuter castes are confined to a few closely-related families of the social Hymenoptera, and to the termites, or "white ants," as they are popularly called. Seeing how exceptional the peculiarity of this complication is in nature, and yet how extensively it is diffused among the many species of these social insects, we may reasonably conclude that the peculiarity did not arise independently and separately in thousands of species of ants, bees, and wasps, even if it should have had a distinct origin in the case of the termites. Originating, as we may suppose, in exceedingly remote ages, and forming an advantageous basis or accompaniment of social forms of evolution, the peculiarity or susceptibility has been evolved and utilised to an extent which has allowed special scope for many important differences and complexities which otherwise could not have arisen. In some cases, as with some humble bees, comparatively solitary conditions, and widely-scattered nests or cells, may have aided or allowed survival, by the more effectual preservation of the honey and larvæ from marauding enemies. In such cases, Natural Selection may neglect or repress the social tendency and the intimately-related potentialities and instincts involved in the production of a neuter caste. Wasps, being carnivorous, and, therefore, unable to store and preserve food for the winter, have not evolved so thorough a division of labour and function as the hive-bees; for such solitary fertilised queens as survive the winter have to house and feed their first brood of neuters by their own efforts. Bees, ants, and termites commonly avoid this annual destruction of the community, their food being less perishable, so that stores can be laid up for times of scarcity. We find that intelligence, instinct, and special structures are most highly elaborated in the neuters of those species in which the queens and drones have most thoroughly lost such capacities or structures. The whole circumstances tend to show that Natural Selection has simultaneously effected increasing specialisation in neuters and queens alike in the direction of division of labour and the more perfect adaptation of each caste to its own particular functions in the community. The various neuter castes seem only to have been evolved in directions that have adequately aided the prosperity of the stirp; and the continued evolution or improvement of workers, warriors, and other neuters would promote the success of the stock quite independently of any similar development in queens or drones in whom it would be disadvantageous. There seems to be every reason for believing, with Darwin, that complex evolution *has* taken place in neuter insects, and that the case for the supposed necessity of use-inheritance in such evolution falls to the ground.

WM. PLATT BALL.

II.

Natural Science in Japan.

II.—PRESENT.

THE mere mention of the various institutions, in connection with which it will be convenient to describe the scientific development of modern Japan, is enough to show how enormous the advance of the last quarter of a century has been. They are The College of Science of the Imperial University at Tokio, the Geological Survey, the Imperial Museum at Ueno Park, the Learned Societies, and various other educational bodies that have for part of their task the dissemination of natural knowledge. In the above order they will now be considered.

The Imperial University, or Teikoku Daigaku, consists at the present day of the six Colleges of Law, Literature, Science, Medicine, Engineering and Agriculture. These Colleges have had very various origins, and their rather complicated history is detailed in the University Calendar (Z. P. Maruya & Co., Tokio). The Science College, with which we are chiefly concerned, sprang, together with the Colleges of Law and Literature, from an institution of some antiquity founded by the late Tokugawa dynasty and revived by the present Imperial Government after the Restoration of 1868. Up to 1885 the College of Science undertook instruction in many practical and technical subjects; but in that year a Department of Technology was created, to which the courses in Engineering, Mining, Applied Chemistry, Naval Architecture and kindred subjects were transferred. Since then the College has devoted its entire attention to the pure sciences, without too curiously considering whether they were "required for the purposes of the State," as demanded by Article I. of the Imperial Ordinance.

In this College of Science there have been established seven courses of instruction, or what we should call Schools, each of which extends over three years. They are in Mathematics, Astronomy, Physics, Chemistry, Zoology, Botany and Geology; but the last three alone concern us now. Neither of these three courses offers any definite instruction in either Physics or Chemistry, such as would be the case at an English University. But since a student who seeks admission to any College has to produce a certificate from one of the Higher Middle Schools, or to pass an equivalent examination, it is probable

that this very necessary training has been already supplied. In other respects each course is admirably broad, and affords a good grounding for each subsequent specialisation required of those who pass to a doctor's degree. This will be seen from the following sketch of the work done by a student in Zoology. During his first year he attends lectures on General Zoology, on Botany and on Geology, he does practical work in the Zoological and Botanical Laboratories, studies Physiological Chemistry with Laboratory work and learns how to determine Rocks and Minerals. In his second year he attends lectures on Botany, the Comparative Anatomy of Vertebrate animals, Histology, Embryology, Physiology and Palæontology. He does practical work in Botany, Histology and Embryology, and spends a few weeks at the Marine Laboratory. The third year is mainly devoted to work in the Zoological Laboratory, with two afternoons a week in the Bacteriological Laboratory during the third term. The student may also study Palæontology and certain special subjects. The work for a Botanical student is the same during the first two years; but in the third year the Botanical Laboratory is substituted for the Zoological, and a Botanical Colloquium takes the place of the special subjects of the Zoologist. In the same way the Geological student has lectures and practical work in Zoology and Botany, besides those more directly connected with his special subject.

At the end of these courses an examination is held, the passing of which makes the student a graduate of the University, but does not of itself entitle him at any time to the higher degrees. A graduate who desires to proceed to the degree of Doctor, which in the case of Natural Science is called *Rigaku-hakushi*, must attach himself to what is called the University Hall for a period of five years. During the two years immediately following his admission he must pursue a post-graduate course, which consists for the most part of special work either in the laboratory or the field. In the latter case his travelling expenses are defrayed by the College according to a fixed rate. After these two years he is free to devote himself to some special subject and to prepare the thesis that he is bound to present on applying for his doctorate.

This system of specialised practical and field-work, both in close connection with the University, is an admirable one. By its means, in the first place, a set of men are forwarded to the Geological Survey and similar scientific posts already thoroughly trained for their work; secondly, the collections of the University are increased and what is of more importance thoroughly worked out; while, lastly, any papers that may be published by these students pass directly under the eyes of the professors, and therefore do not manifest that inexperience in the art of description and exposition which too often mars the work of our own younger writers and which I have suffered from too much myself not to sympathise with in others.

The teaching organisation of the College of Science is, like that of all the others, very complete. In the subjects with which we are concerned the professors and lecturers are as follow,—in Zoology, Professors Kakichi Mitsukuri and Isao Ijima; in Botany, Professor Jinzō Matsumura and Assistant-Professor Saburō Ōkubo; in Geology, Professor Bunjirō Kotō and Assistant-Professor Yasushi Kikuchi; in Palæontology, Professor Matajirō Yokoyama; and in Seismology, Lecturer Fusakichi Ōmori. Physiology may appear a curious omission, but this is amply provided for in the College of Medicine.

The chief buildings of the University, including the offices, the Library, the Colleges of Law, Medicine, Engineering, Literature and Science, the First Hospital of the College of Medicine and the Dormitories of the Colleges, are situated in somewhat park-like grounds in the north of Tokio, which formerly belonged to the great Daimyo, or Lord, of Kaga. The buildings connected with the Natural Science Schools contain admirable laboratories, workrooms and museums, through which I was shown, with great kindness, by Professors Mitsukuri and Ijima.

The Museums that now concern us are two: the Zoological, and the Geological.

The Zoological Museum, which is under the direction of Professor Mitsukuri, is contained in one large room on the first floor. It contains not only specimens, dissections and models, intended for the instruction of the students, but also valuable collections of types described by graduates and professors, and a large amount of material from the lands and seas of Japan, that will be placed, as occasion offers, in the hands of those students who wish to take up some original investigation for the purpose of obtaining their doctorate. Hitherto many of the groups constituting the Japanese fauna have been worked out by Europeans; but it is hoped that in future this will be accomplished by the Japanese themselves. Among the noticeable collections in this room is one of the parasitic worms of Japan, which contains the types of Ijima and the specimens of *Tristomum* now being described by Goto. Here is exhibited a specimen of considerable human interest, namely a fine *Bothriocephalus latus*. This tape-worm was well-known to exist in Japan, and indeed caused great trouble in many districts; but its source could not be ascertained. At last Professor Ijima thought he had traced it to a fish called *Masu*; but the only way in which he could prove that the parasite infesting that fish was indeed the larva of *Bothriocephalus*, was by swallowing it. The experiment was successful. Twenty-two days afterwards the full-grown worm was obtained, and it now graces the shelves in the Museum of that University of which its erstwhile host is still a living ornament. Another fine collection is that of the Japanese birds, which is very complete and contains the types of Ijima and Stejneger. The collection of fish is also unusually good, a fact which is easily accounted for by the very various kinds of fish

that serve the Japanese as food. A walk through the Tokio fish-market about four o'clock in the morning reveals a wonderland to the eyes of the naturalist. Every possible and impossible variety of fish, flapping and twisting and twining in shallow tubs; sharks and threshers, waiting to be cut up into steaks or pounded into a kind of pemmican; large octopods and cuttlefish angrily writhing in buckets and vainly changing their colours; shell-fish of every shape and hue, but chiefly *Arca* and the beautiful *Haliotis*. Nothing seems to be thrown away, and the consequence is that many rarities are secured for the University, while ordinary dissecting material may be had almost for the asking. Before leaving the Zoological Museum, we must notice some admirable anatomical models by Matsutaro Kikuchi, especially those representing the blood-vascular system of *Bufo japonica* and the blood-vascular and nervous systems of *Palinurus japonicus*. In such work as this the handiness and extreme delicacy of the Japanese are seen to the best advantage.

The Geological Museum is in a large T-shaped room on the ground floor. It has been arranged by the Geologist Koto, the Mineralogist Kikuchi and the Palæontologist Yokoyama. The minerals are arranged in two distinct sets; one to illustrate their physical properties, the other according to their chemical composition. Accompanying them is a catalogue, written in English. As in the British Museum, the general collection of fossils is arranged in zoological order, while the specimens found in the country itself are kept separate. These latter include the types of von Mojsisovics, who has described so many of the Triassic fossils of Japan, and the types of Yokoyama. Owing to the volcanic nature of the Japanese islands, such fossils as do occur are for the most part in a very altered condition, and are neither numerous nor well-preserved enough to be advantageously arranged according to a zoological classification; they are therefore disposed stratigraphically and according to their localities. The Museum also contains a general series of rocks. Besides the specimens of rocks and fossils at present exhibited, there are a large number that have been collected by students and are still kept in boxes downstairs awaiting identification. To judge from the state in which most of the fossils unfortunately occur, this will prove a difficult matter.

Connected with the Science College are the four following institutions, the Astronomical Observatory, the Seismological Observatory, the Botanic Garden and the Marine Biological Station.

The Seismological Observatory is the head-quarters of this branch of science for the whole world, and to this position it has been brought by the labours of Professors J. A. Ewing and John Milne. The instruments designed here and the results obtained by them are so well-known from the publications of those gentlemen and of Seikei Sekiya, the Professor of Seismology, that it is unnecessary to allude to them further in this place. It is enough to mention that the

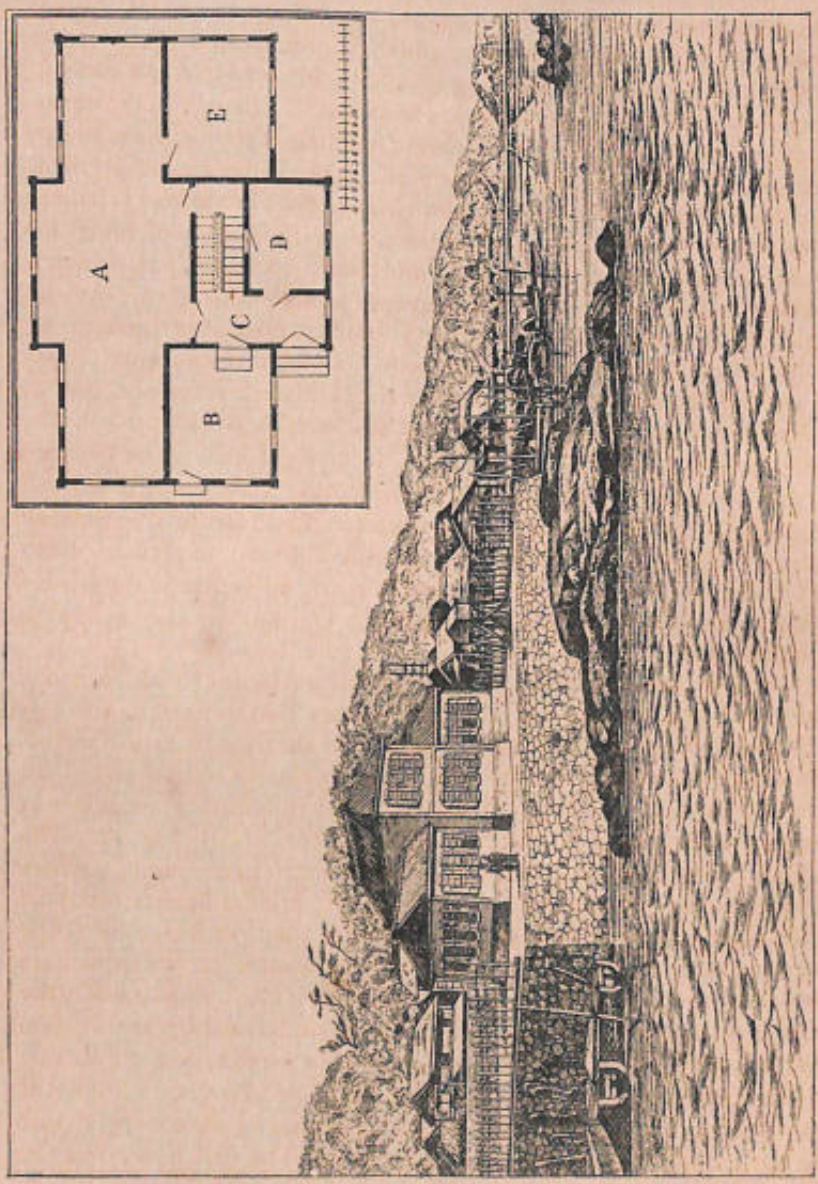


Fig. 1.—THE MARINE BIOLOGICAL STATION AT MISAKI, IN 1887.

investigations have already proved of great practical value, that the Observatory is open for consultation on all matters pertaining to earthquakes, and that the students of geology and other branches of science attend lectures from the professor and his assistant.

The Botanic Garden lies in a pleasant part of Tokio, about a mile from the University. It contains over three thousand species of plants, both native and foreign, which, in the main division of the garden, are distributed according to Bentham and Hooker's classification, as given in their *Genera Plantarum*. There are, however, minor divisions, such as a collection of medicinal plants, those plants that grow in shady places, various rare plants in pots, and a greenhouse with tropical plants. Besides plant-houses in various Japanese styles, there is also a pleasure garden with a building well suited for the social gatherings of scientific and other societies. How much pleasanter it would be for our learned societies to picnic at the Pagoda in Kew Gardens on a summer Sunday, instead of undergoing the boredom of a badly-served dinner in a London restaurant! From the garden, plants are daily sent to the University for the use of students, and on the other hand the students of Botany and Entomology spend a portion of their time in the garden. The Botanic Garden possesses a herbarium of considerable size, though not so complete as that belonging to the Botanic Institute of the Science College, which contains about 4,000 species of Japanese plants. Both the Garden and the Institute are always ready to exchange duplicates with foreign botanists, and the Botanic Garden is also ready to exchange seeds.

The Marine Biological Station is situated at Misaki, a small fishing-village at the end of the promontory that separates the bay of Sagami from that of Tokio. Opposite the village is an island, forming a sheltered strait over two square miles in extent and about eight fathoms in the deepest place. The laboratory, which was opened in 1887, is on the seashore fronting this strait (Fig. 1). The building is of wood, and one story high, except in the middle where it is two stories. The whole sea-front of the ground floor is occupied by the main laboratory (A), which is 48 feet long, 12 feet wide at the two ends and 18 feet in the middle, and is able to accommodate about ten workers. This room is fitted with small aquaria for the use of investigators. At the back of the main laboratory are a store-room (C), a library (E), and a room (B) with a cement floor for sorting out and preserving the specimens as they are brought in from the sea. From a tank outside the building, sea-water is conveyed into this last room and into the main laboratory. The first floor, over the middle of the building, is devoted to sleeping accommodation.

The situation of Misaki, between two bays and sheltered by the island Jōgashima, is extremely favourable to marine life. The strait itself, the neighbouring inlets, and the tide-pools on the ocean side of Jōgashima, furnish all kinds of bottom, while further

out at sea there are beds from which the fishermen dredge up the beautiful glass-rope sponge *Hyalonema* and a magnificent Pentacrinid, the *Metacrinus rotundus* of P. H. Carpenter. According to Professor Mitsukuri (*Journ. Coll. Sci.*, vol. i., p. 383, Tokio, 1887) the following are the more important animals found in this locality. "Foraminifera are likely to furnish a great many species. . . . Of the Radiolarians we have seen some—mostly of the Acanthometridæ. Sponges are well represented. . . . Specimens of *Hyalonema* in museums of Europe and America are in reality mostly from Misaki, although they are marked as from Enoshima where they are bought and sold . . . *Tetilla japonica*, Lampe, is found in great abundance in the harbour of Misaki during the summer months. Of the Coelenterata, hydroid colonies are not very numerous, although we found one species of *Aglaophenia* in great abundance in December. Hydromedusæ, Acalephæ, and Sea-anemones are fairly numerous. Corals are found living, as also *Veretillum* and other Pennatulids. Of the Echinoderms, there are several species of sea-urchins, star-fishes, ophiurans and holothurians, some species being found in great number. A *Comatula* is also found. . . . The Mollusca are exceptionally abundant. Tide-pools etc. may be said to be alive with them in the spring, and their egg-masses form conspicuous objects at the same season of the year. Some of the more noticeable molluscs are *Chiton*, *Haliotis*, *Aplysia*, a curious *Tethys* and other beautifully coloured Nudibranchs, *Patella*, etc. Cephalopoda are caught in abundance by fishermen. Crustaceans are very largely, and worms fairly well, represented. *Lingula* is found here as in almost every part of Japan. The inlet of Muroiso is fairly choked with Ascidians, and their bright red egg-masses form striking objects at the breeding season. Surface collection also furnishes many interesting animals. Besides the usual number of the Crustacean larvæ etc. we have caught *Doliolum*, *Salpa*, Pteropoda, Heteropoda (*Atlanta*, *Pterotrachea*), *Actinotrocha*, *Tornaria*, Siphonophora, *Pilidium*, Loven's larva etc. *Physalia* and *Charybdea* are also found. The Kuroshio ["black tide," a warm current] which passes off the coast of Japan has no doubt some influence on the surface fauna of this part."

How such a description must make the mouths of our Plymouth brethren water! And it must also be remembered that these splendid opportunities are not merely open to advanced investigators, but that all students in the biological school of the University are required to pass at least one term at this station. This seems a feature in which we might very well learn something from Japan.

Now that we know something about the organisation of the College of Science, we have to consider what has resulted from all this. In 1893 the number of students said to be working at the natural sciences was about thirty. There were, as is usual in other places, a larger number studying physics and chemistry. How many of these graduated, I was of course unable to learn at the time I was

in Tokio. In 1891, however, the graduates numbered one each in Physics, Mathematics, Zoology and Botany; and in 1892 they were one each in Mathematics and Astronomy, two in Physics, and one each in Chemistry, Zoology, Botany and Geology, making eight in all. These numbers are, perhaps, not large; at the same time they do not compare unfavourably with those of our old-established English Universities. Nor should we forget that the more practical Colleges of Medicine, Engineering and Agriculture, offering as they do more lucrative prospects, naturally draw many students away from pure science.

The researches carried out in the University laboratories have of late years been published in *The Journal of the College of Science, Imperial University, Japan*, 4to, Tokio. This was started in 1886, as a continuation of the scientific memoirs which had from time to time been published by the Tokio University, and as the channel through which the world at large might receive Japan's own contributions to the progress of science. The languages permitted in this publication are English, German and French; but so far, out of 71 papers, only 5 have been written in German and none in French. This is a contrast to the publications of the College of Medicine, which are entirely in German. Of this periodical a yearly volume is issued, containing about 368 pages and 30 plates in which the artistic patience and enthusiasm of the Japanese are beautifully displayed. In the earlier numbers of the Journal papers on physical and chemical subjects preponderated; but of late there has been a welcome increase of zoological papers. As writers of such we find the names of Goto, Hatta, Ijima, Inaba, Ishikawa, Kishinouye, Mitsukuri, Oka, Sasaki and Watase. In botany there are Miyoshi, Tanaka and Okubo; geological contributions are furnished by Kikuchi, Koto and Sekiya, while Yokoyama is at present the sole palæontologist. For me to express any opinion on the memoirs contained in these volumes would be presumption; enough to say that, in the words of the University Calendar, "they have been highly spoken of by various scientific journals of Europe and America, and many learned societies and institutions have expressed their desire for exchange," a desire, it may be added, which the Japanese are most willing to gratify. Of work done by Japanese but published in foreign journals, the names of Ito (Tokutaro), Iwakawa, Oka (Arajiro), Namiye, and Tsuboi, in addition to others already alluded to, are sufficient evidence, and it may be mentioned that, as its assistant professor of Zoology, the new University of Chicago has chosen Mr. S. Watase.

We pass now to consider the position of Geology in Japan, as exemplified chiefly by the Imperial Geological Survey.

The first portion of Japan to be examined geologically was the region known as Hokkaido, which includes what we Europeans call

Yezo and the Kuriles. In 1862, the Americans P. Blake and R. Pumpelly, who were engaged by the Tokugawa Government, made some observations in the southern part of Yezo; while from 1873 to 1875 various surveys were made under the superintendence of B. S. Lyman. Work was then suspended for thirteen years, till in 1888 Mr. K. Jimbō was appointed Chief Geologist of the Hokkaido and carried on the survey for the local authorities independently of the Imperial Survey, but in accordance with its methods.

Of Japan proper, the first geological survey was undertaken in 1878, by Mr. T. Wada, under the Geographical Bureau. In May 1879, the Imperial Geological Survey was established in accordance with the plans of Dr. Edmund Naumann, then Professor of Geology in the University, who was appointed its head. After some changes, the Survey finally assumed the title of "Chishitsu-chō-sajo," or Geological Survey Institute, T. Wada was appointed Director and E. Naumann Chief Geologist. In 1885 Dr. T. Harada succeeded Dr. E. Naumann, as inspector of both geological and topographical surveys, while the chemical laboratory was transferred from the direction of Mr. O. Korschelt to that of Mr. J. Takayama. At present the only foreigner on the staff is Dr. Max Fesca, the adviser of the agronomical survey.

The work of the Survey is distributed among four Sections,—Topographical, Geological, Agronomical and Chemical.

The Topographical Section makes and publishes maps upon which the geological features may subsequently be laid down. The original survey is done on the scale of 1 : 50,000, and in the office these sheets are reduced to the various scales required for the published maps. Simple topographical maps are published, both in Japanese and English, on the scales of 1 : 200,000 and 1 : 400,000, and each is constructed after the modified Flamsteed's projection, with the middle meridian 136° E. of Greenwich and the middle parallel 36° N. Of these maps, the large scale have contour-lines at distances of 40 metres, and the small scale at distances of 100 metres. Naturally the work of this Section has to be a stage ahead of that of the Geological. In the summer of last year, out of 97 sheets, the surveys of 53 had been finished, 29 sheets had been issued and 6 were in course of preparation.

The Geological Section makes a systematic geological examination of the whole country, with special regard to economic requirements. The surveyor examines the geological deposits and their structure within the region to which he has been appointed, and collects specimens of rocks, minerals and fossils from that region. He makes sketches of the routes he has traversed on the scale of 1 : 500,000, and puts on them detailed geological information, as well as the position of available economic materials. He also sketches profiles and sections, whether artificial or natural. On the return of the geologist to the office, the specimens that he has collected are

determined by the palæontologists and by the Chemical Section, and he himself sets to work to construct a geological map of the surveyed region on the scale of 1 : 100,000, with the help of the maps already prepared by the Topographical Section. He also constructs horizontal sections on either a true or an exaggerated scale. The published geological maps are on two scales, the same as those of the topographical maps. On them the geological formations are shown by different colours, while the positions of marked varieties of special economic substances are represented by conventional signs. Accompanying each map is an explanatory text, which is written in three chapters, the first dealing with the topographical features, the second describing the geological formations, and the last treating of the economic products; and this text is often illustrated by many profiles, sections and maps. There are also published, in conjunction with the other Sections, bulletins, which contain the results of investigations conducted by the Survey. All these are written in Japanese. Up to the beginning of last year 37 sheets out of 97 had been geologically surveyed, and twenty-six sheets had been issued of which all but three were accompanied by explanatory texts; 11 more sheets were in preparation. Of the five maps of the small scale or Reconnaissance survey, three had been published and two were in preparation.

Besides this regular series of maps, detailed surveys have been made of coal fields, oil lands and various mines; of the raw materials of various porcelains; of the sources of water supply for the towns of Tokio, Sakai and Kumamoto; of the geological structure of the Bay of Tokio, with a view to the formation of a harbour; of certain districts shaken or destroyed by landslips or earthquakes; and lastly of the region devastated by the violent eruption of Bandaisan in 1888. The bulletins explanatory of these are, like the preceding ones, in Japanese. Foreigners, therefore, who wish to learn something of Japanese Geology must refer to Dr. E. Naumann's paper "Ueber den Bau und die Entstehung der japanischen Inseln," two papers by Dr. T. Harada, "Versuch einer geotektonischen Gliederung der japanischen Inseln" and "Die japanischen Inseln," and the Explanatory Text and Geological Sketch of Hokkaido by K. Jimbō, which are both in English. Besides these official publications, the student may refer to papers by J. C. H. Godfrey (*Quart. Journ. Geol. Soc.*, xxxiv. p. 542, 1878), C. Gottsche (*Science*, i. p. 166, 1883), R. von Drasche (*Neues Jahrb. für Mineral.* 1879, pt. i., p. 41), D. Brauns (*Memoirs Science Dept., Tokio*, no. 4, 1881), H. van Chappelle (*Tijdschr. Nederl. aardrijkskundig Genootsch.*, iii. p. 436, 1886), H. B. Guppy (*Journ. N. China Branch Roy. Asiatic Soc.* [2] xvii. p. 25, 1882). It is to be hoped that some day the valuable observations buried in the Survey bulletins will be rendered more accessible, and this will be the case if, as the *American Geologist* informs us, they are eventually to be translated into German and English.

When the Geological Section has done its work, it is succeeded by the Agronomical, whose province it is to construct maps showing the various soils, to examine those soils with the view of discovering means by which their fertility may be preserved or improved, and, more especially, to survey those portions of the Empire that are not yet under cultivation, to see how far they are capable of it, and to investigate the quality, abundance and accessibility of the various mineral manures. Although not unknown in other European countries more advanced than ourselves, and although the British government geologists have of late begun to publish maps showing the surface geology, still we have but a small idea of a survey so thoroughly scientific in its working, so eminently practical in its application and so completely fulfilling those promises that were held out to the public when our own Geological Survey and Museum of Practical Geology were first started. For these reasons, an account of the Japanese Agronomical Survey, more detailed than has been given of the other Sections, may be of value as well as interest.

It is recognised that the nature of the solid rocks of any country has directly but a slight effect on its agricultural conditions; and this is especially the case in districts where there is a large amount of alluvium or of drift. On the contrary it is the *soils*, that is the weathered products of the solid rocks, often far removed from the original rock, that have most influence on the fertility of the land. The main object of the agronomical survey in its relation to geology is, therefore, to divide the soils derived from the different kinds of rocks into as many types as possible, and to judge the relative capabilities of those soils for cultivation. Hence the survey is not confined to the cultivated land, but is extended to regions that appear capable of cultivation and also to the forest ground.

First comes the field-work. On the maps already prepared by the Geological Section, on a scale of 1:500,000, the surveyor sketches the distribution of the various soils. He notes the relations of the soils to their mother rocks at different stages of weathering, and collects typical samples of both. The following notes are also made:—the mode of origin and the petrographical character of the soil; the nature of the subsoil, illustrated by sections and borings, to a depth, if possible, of three metres; the height of the land above sea-level; the configuration of the land; the underground water level; local climatic conditions; the registered value of the land; the conditions, methods and results of the farming actually carried on, with particulars as to rotation of crops, manures and similar agricultural details.

The samples of soils collected are next investigated in the laboratory, to find their mechanical composition, chemical constitution, physical properties and absorptive power. To determine its mechanical composition, each sample is quantitatively separated into thirteen parts according to the size of the grains, thus:—

1.	Size of grain larger than 10 mm. in diameter.			Gravels.	
2.	,,	between 10 & 8	,,		
3.	,,	,, 8 & 6	,,		
4.	,,	,, 6 & 4	,,		
5.	,,	,, 4 & 3	,,	Fine soil.	Fine-earthy part.
6.	,,	,, 3 & 2	,,		
7.	,,	,, 2 & 1	,,		
8.	,,	,, 1 & ·5	,,		
9.	,,	,, ·5 & ·25	,,		
10.	,,	,, ·25 & ·1	,,		
11.	,,	,, ·1 & ·05	,,		
12.	,,	,, ·05 & ·01	,,		
13.	,,	smaller than ·01	,,		

A sample of soil is first passed through a sieve with meshes 4 mm. in diameter, and the fine soil thus separated is mechanically analysed by Schöne's apparatus, by which the soil particles from 11 to 13 are washed out one after another with the respective pressures of ·2 mm., 2 mm. and 7 mm. velocity per second. The soil remaining in the cylinder of the apparatus is then separated by a set of sieves which have meshes with the respective diameters of 3, 2, 1, ·5 and ·25 m m.

For the chemical analysis a sample of soil is treated with concentrated hydrochloric acid, having a specific gravity of 1·2, by which the available constituents of soil can be well detected. The fine-earthy part of the soil is boiled in the acid for an hour and, after passing through the usual operation, an extract is made from which Alumina, the Iron oxides, Manganic oxide, Lime, Potash, Magnesia, Soda, Phosphoric acid, Sulphuric acid and Silica (soluble in hydrochloric acid and sodium carbonate) are successively determined Iron in the ferrous state is estimated by the "copper method" (Fesca, *Journ. für Landwirthschaft*, 1884, pp. 407–421) and, for a soil rich in humus, Carbon and Nitrogen are estimated, and in some cases Chlorine is determined. In order to examine the character of clay and its contents in the soil, the insoluble residue left after treating the soil with hydrochloric acid is fluxed with sulphuric acid, and the quantities of Alumina and Silica contained in it are then estimated.

The systematic examination of the physical properties of soil is fully described in Dr. Fesca's "Abhandlungen und Erlaüterungen zur agronomische Karte Provinz Kai," pp. 3–19. Briefly, the weight and permeability have to be noticed. The volume- or absolute weight and the specific gravity of the soil are estimated; and as the volume-weight of the soil varies directly as its porosity, the weight of each sample of soil is thus ascertained either in a loose or a compact state. The permeability of the soil to air and water is examined by ascertaining the water capacity and air contents of the soil, and by observing its imbibing and capillary powers during the process of experimenting.

To answer well the practical questions on manuring, the absorption of phosphoric acid and nitrogen in the soil must be examined. This is effected in the ordinary way, by means of the "bottle method,' using as absorbents monocalcium phosphate and sal ammoniac. However the use of the above absorbents has recently been found unsatisfactory, and at present ammonium phosphate is employed, although its exact standard has not yet been absolutely determined.

The maps published by the Agronomical Section are based upon those already prepared by the topographers, on a scale of 1:100,000.



Instead, however, of being separated into squares, as are the ordinary geological maps, they follow the lines of provincial and prefectural boundaries. On these maps the geological formations are represented by the usual colours, while the characters of the soils, classified according to physical conditions, are shown by inclined lines of different colours or crowded spots of various sizes in darker colours. Sections of the subsoil are delineated on one side of the map, and the spots where they were observed are denoted on the map by corresponding Roman numerals. In the summer of last year 27 prefectures had been surveyed and the maps of 12 published, as shown in the above figure.

With these maps explanatory texts are published, each divided into three chapters. The first describes the topographical features of the district, with special attention to the situation of the agricultural land and its transport facilities. The second contains a general view of the agricultural land with respect to its geological formations, and a detailed account of the soils investigated in the laboratory and of special mineral fertilisers. The third discusses the soils as factors in agriculture and their relations to plant growth, and compares the fertility of soils arising from the different geological formations.

Besides the regular work of the Section, the relations of the principal agricultural products to some of the more important local conditions have been studied in various parts of the Empire, and the results of this work have been embodied in "Beiträge zur Kenntniss der japanischen Landwirthschaft" and "Ueber die landwirthschaftlichen Verhältnisse Japan's und die Kolonization Hokkaido's etc."

The various analyses required by the Agronomical Section are carried out in its own laboratory. The Chemical Section therefore is chiefly occupied in analysing the minerals and rocks and in assaying the ores collected by the Geological Section. Analyses and tests are also conducted, according to a fixed tariff, for the benefit of the public, and materials of technical importance, such as potter's clay, kaolin, lubricating oil, cement and building stone, are examined. Besides the results published in the ordinary bulletins, this Section has issued special reports on Japanese fireclays, coals, limestones, cements, indigoes, lacquer and lubricating oils.

The staff employed in carrying out the multifarious operations of the Geological Survey is constituted as follows:—exclusive of the director and of the adviser of the Agronomical Section, 4 Topographers, 6 Geologists, 10 Agronomists and 9 Chemists. Besides these there are 10 Cartographers and many subordinate assistants. The time allotted for field work during each year is four months, during which time it is reckoned that a single geologist can survey one sheet, and a single agronomist half the area of a prefecture. Among others of the staff to whom I had the honour of being presented, my thanks are especially due to Mr. Kochibé, the learned chief of the Geological Section, for much courteous help in my enquiries and travels.

Connected with the Geological Survey are two Museums. The one, intended for the private use of the staff and for constant reference, is in a large room at the office of the survey. A more elaborate collection is displayed, for public instruction, at the head office of the Department of Agriculture, and corresponds, at least in intention, to our own Museum of Practical Geology. The geological collections of more purely scientific interest are contained either in the museum of the College of Science, which has already been described, or in the geological department of the institution which we have next to consider.

F. A. Bather.

(To be continued.)

III.

The Influence of Volcanic Dykes upon Littoral Life and Scenery.

ALONG the coast of Jersey there stretch for miles great masses of rocky, weed-grown reefs, laid bare with every tide; reefs cut and hewn into every imaginable form; channelled, and divided and sub-divided by numerous broad, artificial-looking roadways, and by interminable, intricate, mazy runnels.

If we examine these, calling to our assistance pick, and shovel, and geological hammer, we find all these paths and bye-paths to be the outward and visible signs of the presence of volcanic dykes that everywhere, in this fire-visited island, intersect and pierce the mother rock. The latter may be diorite, syenite, or schist; all are treated alike, and the resultant features closely approximate. As a rule, the dyke material is diabase, but minette (micaceous porphyry) is not uncommon. This filling, sometimes softer, and always more joint-riven, is much more rapidly disintegrated than the surrounding rock. Exposed to a violent tide—rising sometimes fully 42 feet—and to a sea occasionally very stormy, the disintegrated matter is quickly removed, and a ditch, often extremely well marked, is formed. This may vary in breadth from a few inches up to twenty yards, or even more.

The faunal features of these wave-worn dykes vary considerably, consequent upon whether their direction is at right angles to, or is parallel with, the coast. The former are poor in life, barren, and often sand-choked, but of great value to the agricultural community as cart tracks in the wracking season—that harvest time on the shore, when every farmyard in the island sends out all hands and carts to levy toll from the bleak rock-reefs of their abundant weedy crop. Many of the more marked of the eaten-out dykes serve this purpose, and it is difficult indeed for those who know only shores with a narrow littoral, to appreciate the vast importance of these natural cartways on a coast such as that of Jersey, where a distance of from one-and-a-half to two miles frequently separates high-water mark from the Laminarian zone, whose weedy growth is more greatly prized by farmers than the higher growing and commoner fucus.

Besides thus serving as ready-made roads to the far-distant lower

margin of the littoral, these dykes, piercing at their shore-end deep into the land, form natural passes on to the beach. Used from time immemorial, they now form the slip-ways or slips found in every parish in the island, and upon which the various parish assemblies have spent really considerable sums in improvement. I have examined a great number, and fully 75 per cent. of these slips mark the presence of an intrusive dyke running outwards to the sea.

Why these should be barren of life is easy of explanation. Their trend exposes them directly to the full fury of in-shore gales, which alternately with off-shore winds play a continual see-saw of removing and bringing back the surface layers of sand and shingle along the course of the dyke. Stability of environment, so necessary to the slow-spreading forms of littoral life, is wholly wanting.

In other cases, these dykes serve as determinants in the formation of many of the prettiest of the innumerable bays that line the coast. The great majority of such are small; sometimes independent inlets, but more generally secondary indents that go to make up the larger bays. In each the dyke originally formed a point of weakness, whereon the waves constantly pounding, first tore out a narrow path-way the width of the dyke-filling, and then, when the waves worked with greater force confined between the walls of this channel, the breach was gradually widened by the undermining and wrenching out of blocks of the mother rock. Given the initial weakness of the kind provided by such a dyke, it is difficult to set limits to the destroying, bay-making force of the waves acting continuously during a long lapse of time.

That this necessary time-factor has been present in Jersey, I am well convinced from many personal observations. At several points along the shore and high above the present sea-level I have traced unmistakable raised beaches composed principally of beds of loose, well-rounded pebbles (L'Etacq and along the North coast). I can also testify to *high-lying* sands and gravels, in some cases to be seen even resting upon the old beach pebbles. These sands, I feel certain, were deposited during a time of slow subsidence, and represent, I suggest and believe, the ancient soil and sub-soil torn from the land surface as the sea gradually gained upon the land by contemporaneous sinking. Apparently such deposits synchronise with the high-level (so-called glacial) drifts of Britain. Hence the bay-sculpture of Jersey took place *chiefly* during pre-Glacial times when the land was at a much lower level than at present, and was continued through a portion of the Glacial period when the island was slowly sinking still further. When upheaval came—as I believe it did—towards the close of that time, throwing the land much above its present level, and when a land-connection was brought about with France, the waves ceased to play havoc with the coast-line, retiring well to the eastward of the island. The beginning of this continental period was marked by the sojourn of Palæolithic man in Jersey, as evidenced by

the flint implements found in a sea-worn cave, of raised beach horizon, high up the lofty cliffs east of Grosnez Point. This continental period coming to an end in early historic time, the carving work of the sea has practically but recently recommenced, and at a much lower level than formerly.

Entirely different is the case of those dykes that lie more or less parallel with the coast. Locally known as "gutters," these form systems of canals which, in connection with the dykes at right angles to the shore line, are the potent factors that determine the breaking up of the rock area into innumerable minor reefs and islets. These gutters, humanly speaking, are veritable death-traps. The unwary, penetrating far sea-wards in search of fish or the harvest of wrack, often have their retreat cut off by the filling of these natural ditches long before they become aware of the fact. So constant, indeed, is this danger, that watch-boats, State-paid, hover, during the wracking season, around and among the more dangerous reefs.

Gutters have two very divergent faunas. Where the channels are wide and sandy-bottomed, communicating freely with open water at either end, banks of green, waving, grass-like Zostera—luxuriant sea-meadows—make good the footing, binding the sandy bottom into stable ridges. Such may be termed "canal gutters," as distinguished from the "drainage gutters" that carry off the water from the higher parts of the reefs as the tide runs down. Of the two, the former are the poorer in faunal diversity—but withal they are wonderfully rich. Upon the leafy blades of the Zostera, zoophytes occur in myriads. *Campanularia angulata* disputes place with the feathery *Plumularia similis* and with the graceful *Clytia johnstoni*. And upon these in turn, at certain seasons, brown hosts of diatoms descend in enveloping swathes. *Haliclystus*, also, often decks the green blades with its lovely brown bells. Soft gelatinous compound Ascidians (*Aplidium gelatinosum*) also clothe the blades; and in the breeding months of spring, molluscan and annelidan spawn—*Trochus*, *Nassa*, *Phyllodoce*, etc.—give diversity. The vivid green of the Zostera has had less colour-influence upon its floating population than one would naturally expect. I know of two animals only that are affected, but both are of striking interest. One is the large emerald-hued *Hippolyte viridis*, most lovely of the Prawns; the other, equally brightly-coloured, is a species of the Labridæ or Wrasses.

Quite a distinct and characteristic fauna gathers at the base of the Zostera stems, and upon their half-decayed remains. Most obvious are colonies of the brilliant orange-red Ascidian, *Botrylloides rubrum*, lying profusely about like tiny coral-beaded truncheons, beneath the concealing tangle of green. Little less conspicuous, and growing together with the *Botrylloides*, are snowy-white delicate masses that bespeak to the practised eye the endlessly branching calcareous sponge, *Ascaltis contorta*.

But, after all, interesting and varied as is the life that swarms

upon and among the Zostera, it is as nothing to that finding shelter in the half-loamy, half-sandy soil which the roots of the Zostera, in their increasing growth and decay, largely produce and firmly mat together. Very numerous are the worms. The tube-mouths of the shy *Sabella* (*Branchiomma*)*vesiculosa* opene verywhere. Siphunculids protrude delicate starry crowns in equally large numbers. Errant worms, too, are plentiful, and great gaping holes here and there bespeak the residence of one or other of the burrowing prawns. Molluscs are everywhere.

Examining the fauna of a drainage gutter, we get a wholly different array of life-forms and a hundredfold greater variety. If the great channels traversing the littoral from high- to low-water marks be valuable economically as highways for the wracking carts; if the canal gutters be useful to the fishers as saving weary hours of pulling against wind and tide, it is the drainage gutters—the least important in geological origin—that have the most zoological value as the breeding and sheltering grounds of innumerable animals. With very many creatures the presence of the last-mentioned natural channels makes all the difference between plenty and scarcity—indeed, I am convinced that many species, quite abundant on these coasts, would be difficult to include in the local fauna were it not for the advantages afforded by these gutters. The dykes thus marked are less eaten out than the others I have noticed. The dyke matter often comes to the surface at their upper ends, from whence a rapid fall of level leads to the outlet on the reef margin.

Obliterate the traces of life around and all would betoken the presence of a mountain torrent. Boulders of every size and shape strew the steep and rugged bed; this huge angular one, with riven side, might but just have fallen with thundering din, detached by frost-wedge from a neighbouring precipice; these of lesser size, with smooth surfaces and rounded edges, bespeak a long period of tumbling and rolling hither and thither. Here, too, as in a mountain stream, are bend-corners where, out of reach of the influence of the swift central current, accumulate great patches of gravel and shingle. In these drainage gutters littoral life flourishes in its greatest luxuriance. They form the true centres of the life of the shore, whence proceed in regular and ordered sequence vast colourising swarms of free-swimming larvæ. Two causes contribute. One is that, as these channels run parallel with the coast, their inhabitants are protected from the direct attack of storms, an important direct consideration to the more delicate organisms, and indirectly to the larger, which, in turn, feed upon the lesser. The second cause, and the greater, is, that these gutters, as soon as the tide recedes below their level, become swift-flowing streams, charged with minute animals and food-particles that have drained from the myriad weedy surfaces and chinks that one by one are exposed. This drainage-water, too, is rich in life-giving oxygen, absorbed from the air by the films of water as they drain from the tide-forsaken rocks.

It is difficult to convey even a faint indication of the wealth of life that settles in these gutters relying upon the current bringing plentiful food-particles within the reach of ciliary lashing. A complete list would be impossible, for it would be the cataloguing of three-fourths of the littoral fauna. Sponges and compound Ascidians are strikingly conspicuous; in species multitudinous, in colouring of every imaginable tint and combination—snowy-white, grey, pale yellow, orange, pink, scarlet, lavender, purple, black with white marblings, black with gold stars. These in a thousand grades of tinting occur in mingled patches, of size and form as erratic as the colouring, and give to the overhanging surfaces of the boulders chequered mantles of richness such as even Orient looms never expressed, though governed by minds most subtly appreciative of Nature's brighter aspects. Less striking are the pendant festoons of Polyzoa, hanging half-concealing before and between the crusting Sponges and Ascidians. Among all, wander, and often, too, demand lodging, great hosts of worms and tiny crustaceans not conspicuous as the others, but ever present, ever greatly numerous. Zoophytes and anemones are comparatively few, though *Coryne* at times may occur somewhat plentifully.

These drainage gutters are thus most conspicuously peopled by Sponges, Ascidians, and Polyzoa—animals in all cases living upon very minute food-particles, captured, or rather directed within the body, by the incessant lashing of cilia. Why they are so wonderfully common is due to the fact that the water that rushes past them as the fucus-covered reefs uncover, is laden richly with tiny animals and *débris* just suitable to the lowly mode of feeding practised by these animals. Once established, these colonies attract swarms of the free-moving worms and crustaceans—tiny freebooters, ever on the watch for loot.

Frequently the gutters drain large pools or chains of pools. These nearly always have similar dyke origin, sometimes marking the widening of the dyke fissure, sometimes denoting the point of intersection with another dyke of different direction. Such pools are commonly rich in life of the same species generally as in the gutters, but in lesser profusion. More Zostera grows in the pools, and zoophytes grow in greater numbers. Fishes, too, are more numerous, for the Pipefishes (*Syngnathus*), Sticklebacks (*Gasterosteus spinachia*), and the smaller genera of the Wrasses (Labridæ), and Prawns innumerable find in the dense plant-growth of these quiet, and often deep pools, just the safe hiding places and food preserves best suited to their needs.

JAMES HORNELL.

IV.

The La Plata Museum.

(*Continued from page* 35.)

THE above are some of the most noteworthy of the fossil mammals in the La Plata Museum from the Pampean beds and the somewhat older deposits of the Parana and Monte Hermoso; and I now pass on to the consideration of a few of the more interesting types from the still older Patagonian beds. Putting aside the Edentates, which I had no time to examine in detail, my observations will be in the main confined to the Ungulates, of which I made a special study. The most abundant, and at the same time one of the most interesting, of these early hoofed mammals is the one to which Owen applied the name of *Nesodon*, this genus being represented in the Museum by a vast series of remains, including many perfect skulls, as well as jaws, teeth, and limb-bones. Allied in many respects to *Toxodon*, these Ungulates differed by the closer approximation of their cheek-teeth to the Perissodactyle type of structure; the name of the genus being derived from a well-marked island-like lobe found on the inner side of the upper molars. There are likewise important differences in the conformation of the cutting-teeth, and also in the structure of the skeleton in general, which in many respects is much less specialised than that of the allied Pampean genus. Moreover, all the three species of *Nesodon* which I can alone recognise, were vastly inferior in size to the gigantic *Toxodon*, the smallest of the three being not much larger than a sheep. Hitherto, not much attention has been paid to the limb-bones of this genus; but I have been fortunate enough to identify not only the "long" bones, but likewise the calcaneum and astragalus, and thus to confirm the presumed close relationship of *Nesodon* to *Toxodon*.[1]

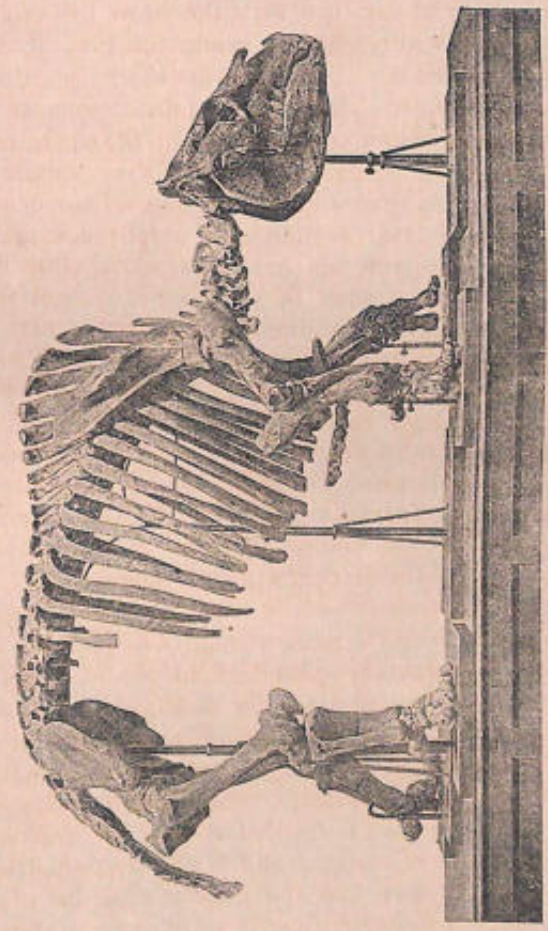
As our palæontological readers are probably aware, Owen described two species of the genus *Nesodon*, together with a third one which has been subsequently ascertained to belong to a totally different type of Ungulate. One of these two species (*N. imbricatus*) was an animal approaching the dimensions of a small rhinoceros, while the second (*N. ovinus*) was, as already said, not very greatly larger than a sheep. Between these two extremes I find an

[1] Some of these bones are described and figured in the forthcoming issue of the *An. Mus. La Plata*, containing an account of the results of my own work.

intermediate form which must apparently be recognised as a third species.

Having two imperfectly-known named species of a genus from a particular formation, one would naturally have thought that the object of the palæontologist would be to endeavour to complete our knowledge of those two species, and to hesitate to name new species (not to mention genera) from remains of the same group of animals from the beds in question, without the clearest possible evidence of their right to distinction. Such a method of procedure seems, however, to be utterly at variance with the views of certain South American so-called palæontologists, to whom the task of describing the fossil mammals in the La Plata Museum has been unfortunately from time to time confided. Instead of endeavouring to find out whether the specimens before them might not belong to one or the other of the two named species of *Nesodon*, they appear to have started on the assumption that almost every single bone or tooth that came under their notice must pertain to a totally new animal. In consequence, we have remains which clearly belong either to one or other of the two Owenian species, or to the above-mentioned intermediate form, assigned to something like a dozen genera (such as *Acrotherium*, *Adinotherium*, *Atrypotherium*, *Colpodon*, *Nesotherium*, *Gronotherium*, *Phoberœotherium*, *Protoxodon*, and *Scopotherium*); while the number of nominal species must, I should think, be fully half-a-hundred. As a result of this extraordinary method of procedure, an enormous proportion of the specimens in the La Plata Museum are "types," whereby that institution is prevented from doing as much in the way of exchange as would otherwise be practicable. This remarkable ignorance of the first principles of odontological anatomy, and of the different forms assumed by teeth according to the ages of their owners, displayed by the palæontologists in question, surpasses belief, and there are certain specimens in the Museum bearing different generic names which even any ordinary student would say were identical. Indeed, on the principle (or, rather, want of principle) which appears to have guided the Argentine palæontologists, about a dozen species and some half-a-dozen genera might easily be made out of remains of the common horse. It is true that *Nesodon* displays an extraordinary degree of variation in the relative proportions of the large incisors in both jaws, but the gradual evolution of the adult from the young stage is indicated over and over again in the collection of the Museum; and with regard to the species founded on the evidence of the cheek-teeth, there is not the least excuse. This, however, is not all, for some of the so-called genera have actually been assigned to families apart from *Nesodon*; while the latter itself is separated, as a family, from the Toxodontidæ without the faintest shadow of justification.

I must, however, do one of the above-mentioned workers the justice of saying that he has at last partially seen the errors of his

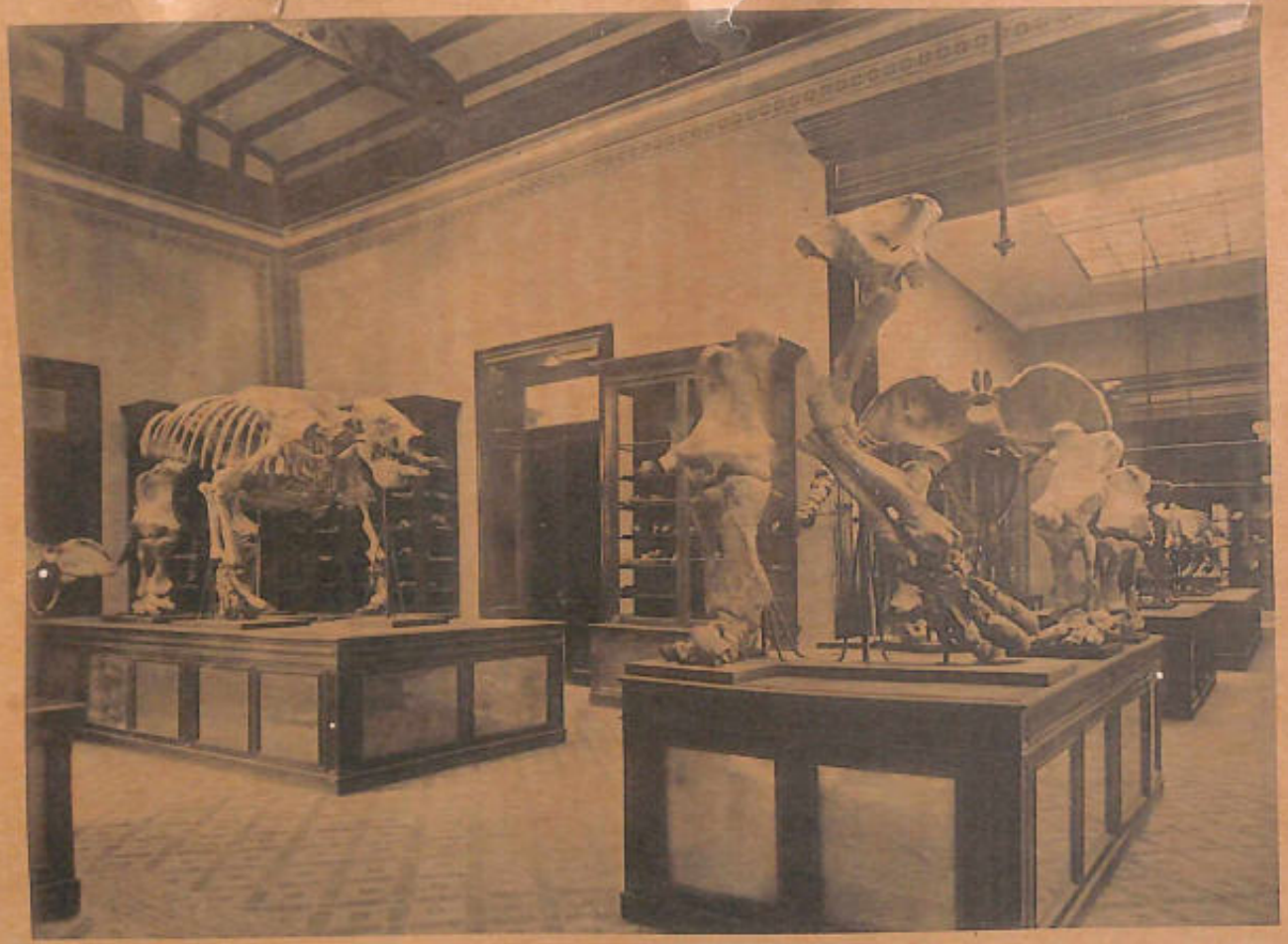


SKELETON OF *Toxodon*,
AS MOUNTED IN THE LA PLATA MUSEUM.

ways, and has tardily abolished some of the superfluous genera made by himself and his colleagues. His repentance comes, however, too late—after all the mischief has been done; and even then it is but half-hearted. He maintains, for instance, the genus *Acrotherium*, which is merely founded on specimens belonging to some of the three species of *Nesodon*, with a superfluous premolar tooth, probably caused by the first premolar having come up in front of the corresponding milk-molar, instead of replacing it; while the list of nominal species remains as long as ever. South American palæontological work has, indeed, already become a bye-word in England, but it is really far worse than I had any idea of previous to my visit to La Plata; and it may be worth the consideration of the Council of the *Zoological Record* whether it will not for the future be advisable to omit all mention of the majority or the whole of the names proposed by the Argentine palæontologists to whom I refer, as being mere useless encumbrances, instead of aids, to science.

Passing from this unpleasant portion of my subject to more agreeable matters, I may now call attention to two very remarkable members of the Toxodont Suborder, both being animals not larger than a rabbit, and exhibiting most marked rodent resemblances. The one of these is *Hegetotherium* from the Santa Cruz beds of Patagonia, and the other *Pachyrucus* from the deposits of Monte Hermoso. The latter of the two is distinguished from its ally by the tympanic bullæ being situated on the superior aspect of the skull and is so Rodent-like in general form and structure that it is almost difficult to believe that it is not an ally of the hares. It is, however, as shown by its teeth, clearly a member of the Toxodont Ungulata, and since it is perfectly evident that such an animal cannot have been the ancestor of the Rodentia, it follows that the Rodent resemblances presented by the more specialised Toxodonts must be due to parallelism.

By many writers the Toxodonts have been placed, with the Proboscidea and certain extinct suborders, in the Subungulata; they have, however, the alternating carpus of the Perissodactyla and Artiodactyla, coupled with the linear tarsus of the Proboscidea, while the astragalus is but slightly grooved, and the calcaneum carries a large facet for the fibula, as in the suborder last mentioned. These features clearly indicate that the Toxodonts (which are further characterised by some or all of their teeth growing for the greater portion or the whole of life) must form a subordinal group of equal rank with the Artiodactyla, Perissodactyla, and Proboscidea. Another subordinal group of extinct South American Ungulates, for which I have suggested the name Astrapotheria, is formed by the genera *Homalodontotherium* and *Astrapotherium*. Differing from the Toxodonts in having their teeth rooted at an early age, these Ungulates are further distinguished by having a perfectly flat astragalus with a head at the lower end for the navicular, while it is probable that both the carpus and tarsus were of the linear type. The cheek-



LA PLATA MUSEUM.

teeth are, moreover, exceedingly like those of the Perissodactyla, and more especially the Rhinocerotidæ, to which the members of this group approximated in point of size. Although there seems to be but one species of the first-mentioned genus and only two or three of the latter, the list of synonyms in the case of *Astrapotherium* is of the usual appalling length. As its name implies, the genus *Homalodontotherium* is characterised by the teeth being forty-four in number and forming an uninterrupted series, with the canines not longer than the incisors. Until recently this genus was known only by the teeth and jaws, but the La Plata Museum contains numerous specimens of the vertebræ and limb-bones. Among these, the humerus is remarkable for the great development of its deltoid crest, which recalls that of the wombat.

A very different animal is the gigantic *Astrapotherium*, the type species of which was originally described by Owen as *Nesodon magnum*. In this creature the dentition is reduced, and each jaw furnished with a huge pair of tusks, while the upper molars are extraordinarily like those of the rhinoceroses. There are no teeth between the huge upper tusks, which I have reason to believe are incisors; but in the lower jaw there are three pairs of small incisors, with curious spatulate crowns, situated between the pig-like tusks, which are here clearly canines. *Astrapotherium* has been placed among the so-called Dinocerata, but it is certain that such resemblances as it presents to that group must be attributed to parallelism, while its relationship to *Homalodontotherium* (as proved by the limb-bones in the Museum) is perfectly clear. It cannot, moreover, have any direct relationship with the Rhinocerotidæ, so that the resemblance of its molar teeth to those of that group is again apparently due to parallelism.

A third subordinal group of extinct Ungulates peculiar to South America is represented by *Macrauchenia* in the Pampean deposits, and by *Proterotherium* and certain allied forms in the Patagonian Tertiaries. These animals have been placed by some writers with the Perissodactyla, but it is certain that Professor Cope is perfectly correct in regarding them as representing a distinct suborder—the Litopterna. Agreeing with the Perissodactyles in having an odd number of toes, with the middle one symmetrical in itself, and likewise in the pulley-like upper surface of the astragalus, these Ungulates differ from that group in having both the carpus and the tarsus of the linear type,[2] and likewise by the fibula articulating to a small facet on the calcaneum (as in the Artiodactyla). Moreover, in those cases where they are known, the vertebræ of the neck are much elongated, and have the sides of the neural arch pierced by the canal for the vertebral artery in a manner now

[2] It may be well to mention that in the linear type of carpus and tarsus, the bones of the two horizontal rows are set directly one over the other (as in the Proboscidea), whereas in the alternating type the bones of the upper row are placed over the divisions between those of the lower.

solely characteristic of the Camel family. Like the other suborders of extinct Ungulates peculiar to South America, the Litopterna further differ from both the Artiodactyla and Perissodactyla in having the bodies of the cervical vertebræ articulating together by flat terminal surfaces, instead of by a ball-and-socket joint. They likewise present the same strongly-marked similarity to the Perissodactyla in the structure of their cheek-teeth—a feature doubtless inherited from a common ancestor among the Condylarthrous Ungulates of the Eocene, but more or less specially developed subsequently by parallelism. The Litopterna are divisible into the two families of the Macraucheniidæ and Proterotheriidæ, the former being distinguished by the full and uninterrupted dentition; while in the latter the teeth are reduced in number and interrupted. An ancestral form of *Macrauchenia* is represented by the species of *Oxyodontotherium (Theosodon)* of the Patagonian Tertiaries, which were much smaller creatures than the Pampean animals, while an intermediate type existed in the Parana beds.[3] In this family, as well as in the next, I have again to deplore a superabundance of names, both specific and generic, as I have pointed out in the memoir referred to.

Among all these curious types of Ungulates, none are more remarkable than the Proterotheriidæ, as represented by the genera *Proterotherium* and *Diadiaphorus* of the Patagonian Tertiaries and the Parana beds. These were animals varying in size from a peccari to a tapir, with molar teeth more or less closely resembling those of the European Oligocene genus *Palæotherium*, but having only a single pair of tusk-like incisors in the upper jaw, and two pairs of lower incisors, one of which was much larger than the other. From the researches of Señor Ameghino, it is already known that in one member of this family (*Epitherium*), occurring in beds above the horizon of the Patagonian deposits, the feet were of the general type of those of *Hipparion*—that is to say, the middle toe was greatly developed at the expense of the two lateral ones, which were small and functionless. I find, however, from the evidence of the specimens in the La Plata Museum, that some at least of the Patagonian representatives of the family were likewise provided with feet of the same highly-specialised type, while I have no evidence that any of them had functional lateral digits.

This extreme specialisation of the feet of these otherwise generalised Ungulates is a feature interesting enough in itself, but it is of still more importance in regard to the relative age of the strata in which their remains occur. The Patagonian Tertiaries of Santa Cruz, from which the remains of Proterotheriidæ are obtained, appear to be nearly, if not quite, the oldest South American deposits yielding remains of land mammals. They are correlated by Señor Ameghino (who, by the way, suggests that *Proterotherium* and *Diadiaphorus* were

[3] For this form Señor Ameghino has proposed the barbarous name *Scalabrinitherium*, a term which may be changed to *Scalabrinia*.

animals provided with three functional toes to each foot) with the lower Eocene of Europe, while the Parana, Monte Hermoso, and other intermediate beds are assigned to the Oligocene and Miocene, and the Pampean deposits identified with the Pliocene. Now the fact that in the reputed lower Eocene beds we meet with animals having a foot as specialised as is that of *Proterotherium*, serves, to my mind, at least, to show the utter untenability of the hypothesis in question. We know that in the lower Eocene of both Europe and North America the Ungulates were all five-toed animals with brachydont, and generally tritubercular teeth; and if the South American Ungulates with feet of the *Proterotherium* type, hypsodont molars like those of *Nesodon*, or tusks of the length of those of *Astrapotherium*, were also of lower Eocene age, it would involve the existence of a mammalian fauna like that of the Puerco Eocene and London Clay in some part of the world during the Cretaceous epoch, from which the Patagonian Ungulates had originated. Of the existence of such a fauna there is, I need scarcely say, not only a total lack of positive proof, but likewise very strong evidence to the contrary. Then, again, the existence of a member of the existing genus *Dasypus (Zaëdius)* in the Santa Cruz beds renders it impossible to regard them as of lower Eocene age.

Moreover, in my forthcoming memoir on the fossil Cetaceans in the La Plata Museum, I have called attention to the circumstance that in one part of Patagonia there occurs a bed yielding Cetacean remains which appears to underlie the Santa Cruz deposits. Now this Cetacean bed most certainly is not of lower Eocene age, and is, indeed, probably Miocene, an identification which, if established, will at once overthrow the Eocene, or, indeed, Oligocene hypothesis of the Patagonian beds. Apart from this evidence, I am, however, quite convinced that the Patagonian Ungulates, owing to the specialisation of the feet in some cases and of the teeth in others, are not lower Eocene, or even Eocene at all, but are far more probably of Miocene age. The correlation of some of the beds lying between the Santa Cruz and Pampean deposits with the European Oligocene and Miocene likewise will not bear critical observation, and can, indeed, only be maintained by the creation of species or genera which have no existence save in the minds of their founders. For instance, I find it impossible to distinguish specifically the remains of *Typotherium* found in the reputed Miocene strata of Monte Hermoso from those of the typical Pampean form, while, as I have already shown, the so-called *Hipphaplus* of the supposed Oligocene Parana deposits is nothing more than a species of the Pampean genus *Hippidium*; and if we are to have Oligocene strata with a genus so close to *Equus* as to be doubtfully distinct therefrom, what possible grounds can there be for correlating them with the horizon so-named in Europe? I can believe, indeed, in the late survival of a generalised genus, but I utterly refuse to credit the occurrence of a

specialised one on a horizon far below its proper one. The proposal to regard the Pampean beds (which are some of the most recent-looking deposits I have seen in any part of the world, and contain evidence of the existence of man contemporaneously with the extinct mammals) as of Pliocene rather than Pleistocene age, is on a par with the above wild conjectures—for I can scarcely call them theories. In my own opinion, indeed, the whole of the series of fossiliferous strata from the Cetacean beds and the Santa Cruz deposits of Patagonia to the topmost Pampeans, may in all probability be included within the period occupied by the Miocene (perhaps inclusive of the upper Oligocene), Pliocene, and Pleistocene beds of Europe.

Another Patagonian mammal of great interest is one for which Señor Ameghino has proposed the name of *Pyrotherium*, and which he places among the Eocene Coryphodonts, although I fail to see the reason for the association. The type specimens include a premolar and molar tooth, as well as a tusk, but I have reason to believe that the latter pertained to *Astrapotherium*. The molars of this gigantic animal resemble those of the Australian extinct *Diprotodon*, and the last two molars of the Proboscidean genus *Dinotherium*; and it hence seems that these teeth are insufficient to determine the affinities of this strange creature. The type specimens were obtained from Neuquen in Patagonia, but others in the Museum come from Chubut, in the same country. The latter were found in association with remains of *Astrapotherium*, *Homalodontotherium*, and *Nesodon*, thus showing that the horizon of these beds is identical with, or very near to, that of the Santa Cruz deposits. In a paper published some time ago in *La Revue Scientifique* by Dr. Trouessart, from notes supplied by Señor Ameghino, it is stated that *Pyrotherium* occurs in beds yielding Dinosaurian remains; but this must, I think, be now regarded as incorrect. Possibly a fragment of a very large tusk from Chubut of a Proboscidean type may belong to *Pyrotherium*, in which case the genus would probably have to be regarded as allied to *Dinotherium*. The section of this tusk is egg-shaped, with a maximum diameter of about four inches. As in *Dinotherium*, the dentine does not show decussating striæ.

Omitting any reference to the large collection of Edentates, Rodents, and Marsupials from the Santa Cruz beds of Patagonia contained in the Museum, we may pass on to the Cetacean remains mentioned above, all of which are contained in the same gallery as the land-mammals from Patagonia. Several of these Cetaceans are of especial interest, on account of their exhibiting generalised features unknown in any of their living relatives, and thus afford very important evidence in regard to the phylogeny of the two existing subordinal groups of this order. Although of less wide interest than most of the others, one of the finest specimens in this series is the nearly entire skull of a small baleen-whale, which, from the evidence of the tympanic bone, I have assigned to the European Tertiary genus *Cetotherium*. Like the other remains, this skull

was obtained from a sandy deposit a short distance from the shore at Chubut. Most of the other remains are those of toothed-whales, among which a fine, though somewhat imperfect, skull of a small form allied to the sperm-whale claims special attention. As our readers are doubtless aware, the whole of the existing members of the sperm-whale family are characterised by the absence of functional teeth in the upper jaw, those in the lower jaw varying in number from more than twenty on each side to a single pair. The Patagonian skull shows, however, a full series of large conical teeth in both the upper and lower jaws, these teeth being not unlike those of the sperm-whale, although furnished with thin caps of enamel on their crowns. The skull has the same general form as that of the cachalot, displaying a large and deep frontal cavity for spermaceti. From the structure of the teeth I have identified this skull with the European Tertiary genus *Physodon*, which has hitherto been but very imperfectly known; and since its inclusion in the Physeteridæ would render that group very difficult to define, I have suggested that it should constitute a family by itself. Another member of the same family is represented in the Museum by a smaller cranium, to which I have assigned the name of *Hypocetus*. A totally different type of Cetacean is presented by a small skull with teeth of the type of those of the European Tertiary genus *Squalodon*, but differing from the latter in number. This difference alone I should not have regarded as of generic value, but an examination of the nasal region showed the presence of prominent nasal bones projecting over the nasal cavity in a manner quite unknown in any living member of the suborder, and I accordingly consider this form as the representative of a new genus, with the name of *Prosqualodon*. Precisely the same feature, although in a more exaggerated degree, is displayed in the nasal region of an exceedingly elongated and dolphin-like skull, with simple teeth, which I have described under the name of *Argyrocetus*. From its general characters, I refer this skull to the Platanistidæ, but it differs from that of the three existing genera of that family by the symmetry of the narial region, and the projecting, wedge-shaped, and roof-like nasal-bones. Although the discovery, sooner or later, of toothed-whales with projecting nasals and symmetrical skulls was a thing to be expected, yet the absence of any evidence of the existence of such forms hitherto has been regarded as a bar to the derivation of the baleen-whales from the toothed-whales. This obstacle has now been removed by the discovery of these two extinct genera in the Patagonian Tertiaries, and it is possible that future investigations will show that certain other features, which have been regarded as indicating a dual origin for the two groups in question, admit of another explanation. The inclusion of these two forms in the Odontoceti (and they certainly cannot be regarded as representing a distinct group) must, to some extent, modify the ordinarily-

accepted definition of that suborder. The last of the Patagonian Cetaceans is represented by two skulls, which indicate a rather large member of the Delphinidæ, with a somewhat elongated snout. This form, which I have proposed to designate by the name of *Argyrodelphis*, differs, however, from all existing dolphins in that the hinder teeth are furnished with minute fore-and-aft cusps, thus showing another ancestral feature among the Patagonian Cetaceans.

Concerning the collection of remains of giant birds from the Santa Cruz deposits in the La Plata Museum, so much has of late years been written, and the plates accompanying the memoir of of Señores Moreno and Mercerat are so excellent, that it will be unnecessary to say much in this place. It is, however, certain that the number of generic names which have been published is much too large, and that the name *Phorosrhachus*, originally proposed by Señor Ameghino, has the right of priority. Apart from their gigantic size, these birds claim especial attention on account of the extraordinary size and massiveness of their skulls, as attested by the form of the mandibular symphysis, of which there are several examples in the Museum. Although, in the memoir referred to above, these birds were arranged under several family and generic heads, I am in accord with Señor Ameghino in regarding the whole of them as pertaining to a single family, the larger members of which may be subdivided into two genera, *Phororhacus* and *Brontornis*. In the former the symphysis of the lower jaw was long and narrow, its length when entire being probably about 7½ inches, and its maximum width 2¾ inches. In the more massively built *Brontornis*, on the other hand, the sympyhsis was very broad and short, while the margin of the jaw was remarkable for its extreme curvature, the tip being sharply inclined upwards. The approximate length of the whole symphysis is 5½ inches, and the width about four inches. This type of jaw seems quite unlike that of any living group of birds.

Of the cranium, the Museum possesses two fragments, neither of which are figured in the memoir of Señores Moreno and Mercerat. One of these comprises the occipital and parietal regions, imperfect on the left side, where it shows a cast of a portion of the brain; while the other is a part of the left side of the cranial box, with the quadrate in position. From the latter I was fortunately enabled to detach the greater part of the quadrate, and was thus able to learn that this bone was articulated to the cranium by two distinct heads, and that it was apparently not overlapped by a descending process of the squamosal. Both these being essentially Carinate characters, it seems evident that the Stereornithes cannot be included in the Ratitæ; and that they must consequently either be placed among the Carinatæ or form a group by themselves.

This group, in which *Gastornis* may have to be included, will perhaps turn out to form the connecting link between Carinates and Ratites.

Their vertebræ were highly pneumatic; but the hollow leg-bones appear to have been devoid of pneumatic foramina, and during life were probably filled with marrow, like those of existing Ratitæ. In *Brontornis* the tibia has a length of 30 inches, while the metatarsus measures 15¾ inches in length, with a width of 5½ inches at the upper end, and 3 inches in the middle of the shaft. Although displaying a similar depression at the upper part of the front of the shaft, the metatarsus of *Phororhacus* is a much more slender bone, the length in one species being 15¾ inches, with a maximum width at the upper end of 3¼ inches, and of 1½ inches at the middle of the shaft.

The much smaller imperfect metatarsus figured under the name of *Palæociconia* is regarded by Señor Ameghino as inseparable from *Phororhacus*; but from the circumstance that the foramen between the third and fourth trochleæ perforates the bone at right angles, instead of descending obliquely so as to open inferiorly on the lower aspect of the bone between the two trochleæ, I am inclined to think that it has a right to generic distinction.

The whole of the remains noticed above are from formations of Tertiary age, but the collection of fossil vertebrates does not end with that period. From certain deposits in the districts of Chubut and Neuquen, in Patagonia, which are probably of Cretaceous age, there have been obtained a large series of Dinosaurian bones pertaining to reptiles rivalling in size their most gigantic European and North American allies. One of these creatures, although by no means the largest, I have referred, in a memoir about to be published by the Museum, to the genus *Titanosaurus*, originally founded, on the evidence of caudal vertebræ, from the Cretaceous rocks of India. These vertebræ differed from those of all other gigantic Dinosaurs in having a cup at the anterior end of the centrum, and a ball at the opposite extremity, thus resembling those of existing crocodiles. The large series of specimens in the La Plata Museum serves to show that *Titanosaurus*, as had been previously suspected, is really a member of that group of Dinosaurs to which the name of Sauropoda has been applied. This is clearly shown by a fine dorsal vertebra exhibiting the well-known lateral pits characterising that suborder.

In these vertebræ, it may be observed, the cup is situated at the hinder end of the centrum, and the change of type is effected by means of the first caudal vertebra, which, as in crocodiles, is biconvex. The bones of an enormous fore-limb, together with an imperfect femur and two caudal vertebræ, indicate a still more stupendous member of the same family, for which I have suggested the name of *Argyrosaurus*. In the type specimen the length of the humerus is nearly the same as in the gigantic humerus from the Kimmeridge Clay preserved in the British Museum, and mentioned in the *Catalogue of Fossil Reptiles* under the name of *Pelorosaurus humerocristatus*. A smaller Dinosaur, characterised by the slight development of the lateral pits in the vertebræ of the trunk,

and hence named *Microcœlus*, appears to indicate a type unknown either in Europe or North America, while two vertebræ point to the existence in Patagonia of a Dinosaur more or less closely allied to the European *Megalosaurus*. With the discovery of these interesting Dinosaurs in Patagonia, we have now evidence that this extraordinary group of reptiles was represented during the upper half of the Secondary epoch by allied forms throughout the greater part of the world, their remains having now been obtained from Europe, India, Australia, South Africa, and North and South America. The vertebrate land fauna of the world seems therefore at that comparatively early epoch to have been much more homogeneous than it has ever been since, while there is no evidence of any marked distinction between the types of life inhabiting the northern and southern hemispheres.

To give an exhaustive account of all the treasures of the La Plata Museum would entail an amount of space far beyond that which is here available, but I trust that the foregoing brief sketch may convey to the palæontologists of Europe some idea of the richness and interest of the collections which are stored in the handsome building in La Plata. The Government are, indeed, to be congratulated on having founded such a noble institution as the Museum; and it is to be hoped that, when the scientific value and importance of its contents are more fully realised, they will not refuse such financial support as may be necessary for ensuring their preservation, and for making them known to the world at large by means of suitable publications.

It is not, however, solely as a geological, palæontological, and zoological institution that the Museum of La Plata demands admiration and support. It is likewise a great printing and chartographical establishment, where Government documents and maps are produced with a speed worthy of all commendation. The aim of the Director is, indeed, that the Museum should eventually display the entire evolutionary history and the whole of the natural products of Argentina; while it should at the same time be the depository of the whole of the data relating to the geography and topography of the country, and the place where all information on such subjects should be readily accessible to the public. For the success of this grand and noble scheme the Director has, as he deserves, our most cordial good wishes.

In conclusion, I have the pleasure of tendering my most hearty thanks to Dr. and Mrs. Moreno for the hospitality and unvarying kindness which I received at their hands during my brief but pleasant sojourn in La Plata.

RICHARD LYDEKKER.

Las Bandurrias, La Colina,
Buenos Aires,
November 3rd, 1893.

V.

Plant Diseases and Bacteria.

BOTANISTS have always been justly proud of the fact that they had a fully-established germ-theory of plant diseases at a time when this explanation of the diseases of man and animals was only beginning to dawn on pathology; that they showed the way, and in many cases (not the least important) invented the methods which the modern bacteriologist has re-discovered and certainly perfected. This botanical germ-theory was, in one respect, more obvious and easy of discovery than the animal one. It dealt with fungi, which though minute and elusive enough to defy all but the most patient and cunning investigation, are yet far more tangible organisms than bacteria. The brilliant work of de Bary and others in tracing the life-histories of parasitic fungi through generations inhabiting successive host-plants of diverse kinds has no parallel among the investigations of the bacterial diseases of man and animals. On the other hand, botanists must admit the reproach that, while they have concentrated their energies on the micro-organisms that produce the disease—naturally enough, since these are plants as well as the hosts—they have neglected the study of the diseased host, and that consequently plant pathology in the strict sense is in a comparatively backward state. This, fortunately, has shown signs in recent years of being remedied by increased attention; and one of the fruits of such attention most to be desired, viz., a better understanding of what is meant by *immunity* and *predisposition*, can be reached only by this path. There is hardly any subject in the whole of scientific literature and conversation about which there is more vague writing and talk than predisposition. There is a very small measure of bread and "an intolerable deal of sack." How it may be exactly with human beings and animals, I am not qualified to say, and I expressly exclude them from these remarks; but no burning and shining light on the subject has reached botanists from this source, and I strongly suspect, from my insufficient knowledge of the matter, that here too there is little to be said but much to be doubted.

Some years ago, when there was more public, and perhaps less private, attention paid to such subjects as the diseases of the potato, cereals, and other crops, it was a favourite view of farmers, gardeners, and even of more exalted personages, that these cultivated crops had

developed, through what was vaguely called "over-cultivation," an inherent tendency to disease, just as we hear a great deal about the "diseases of civilisation," and so on. There may be something in this, but no one has ever proved that there is, and, meantime, one must be content with the apparently sufficient explanation that the cultivation of crops in close contiguity gives the parasitic organisms opportunities of spreading superior to those commonly found in nature (though not so much so as might readily be supposed), and also affords the spectator a better opportunity of witnessing the effects, and attracting his attention, from the fact that the diseased plants were valuable to him, much more than wild plants, for example. One cannot resist suspecting that some of the so-called "diseases of civilisation" are in the same boat. We know that such as are infectious are often infinitely worse among uncivilised communities, among whom they possibly originated, and such as are not infectious are sometimes, at least, imaginary ailments, or ascribed to civilisation on wholly inadequate grounds. But these things are leading me away. We certainly do not know that there is an "inherent tendency to disease produced by over-cultivation," and we do not even know if there be such a thing as an inherent tendency to disease at all. The fact that one species is taken by a parasite and another left, that one variety even may escape wholly or partially while others suffer, shows us that there may be something of the kind; but, on the other hand, the tendency is better described as on the part of the parasite; the predisposition may be in the environment of the host, and, lastly, need not be a sickly or enfeebled condition of the host, for that is generally what is meant.

It may not be too tedious if I venture to recall that these fungi and bacteria all gain their living as *parasites* producing diseases in living bodies of plants and animals, or as *saprophytes* feeding on their decaying bodies or dead organic substances. Between the *strict parasites* and *strict saprophytes* there are, however, intermediate forms, such as parasites that are *facultative saprophytes* and saprophytes that are *facultative parasites*. Finally, we have in the lichen-forming fungi, for example, cases of commensalism in which a lasting nutritive partnership is struck with the host instead of a one-sided arrangement, as in the other cases. It will be seen, therefore, that there is a considerable range of nutritive adaptations among these organisms—a certain elasticity of accommodation. Among the parasites, which principally concern us, we have such as are facultative saprophytes and the converse, and we have others that are almost omnivorous parasites, attacking plants of diverse groups; others confined to an allied group of species; others to a single species. Among them there are noteworthy instances of parasites that attack hosts outside the group mostly affected, and others again that exempt particular species and even varieties within the group of hosts. Consideration of such instances rouses curiosity as to the exact cause of immunity,

or of predisposition, on the one hand; or, to put the matter otherwise, as to the gradations in the aggressive behaviour of a parasite to the different varieties or even individuals of a host. It is just here that we leave sure ground in most cases, but there is one remarkable instance, at all events, of which our knowledge is sufficient—that of *Pythium*, which, as a facultative parasite, attacks flowering plants. The host here displays degrees of predisposition or power of resistance in proportion to the amount of water it contains—a condition not in itself sickly. We know further—it is abundantly evident that sickliness is not always necessary to predisposition in plants, but, on the other hand, it may frequently be so. At one extreme we have the parasite which is the complete master of its host under favourable external conditions—as much its master as the animal that devours it in more summary fashion; and, at the other extreme, we have parasites which need such adventitious aids of access to their hosts as wounds and abraded surfaces, etc. The subject is a very wide one, and merely the salient points are being touched on here, but I must cite the facultative parasitism of moulds that grow on fruits, to be referred to again later.

One of the most interesting general truths in all this web of fact is the exemption of living plants from the assaults of bacteria. I reserve certain possible exceptions, such as Wakker's hyacinth disease. This exemption has been generally accounted for by the acid reaction of plants, and I might venture to suggest that, since more causes than one commonly co-operate in such matters, the comparatively strong illumination of the vitally active above-ground parts of plants may partially aid in this direction. Recent researches have demonstrated the exceptional powers of sunlight as a retarder and destroyer of Bacteria,[1] and Professor Ward, who has re-examined the whole question fruitfully, has pointed to the protective powers of the pigments of such parasitic fungi as Uredineæ, etc., against this influence—a protection not shared by bacteria.

The question of the relations of bacteria to plant tissues has recently been discussed by Dr. H. L. Russell[2] in a thesis for his degree at the Johns Hopkins University. He complains at the outset of the neglect or the sceptical reception by European writers of the case of the pear-blight caused by *Bacillus amylovorus* which, he contends, has been established by Professor Burrill, and he cites in an appendix tables of bacterial plant diseases, many of them, it is to be feared, in like case with the pear-blight. In some of these diseases the bacteria

[1] *See* Raum. "Der gegenwärtige Stand unserer Kenntnisse über den Einfluss des Lichtes auf Bacterien, etc." *Zeitschr. f. Hygiene*, Bd. vi., 1889. He cites some 150 papers dealing with the influence of light on bacteria, etc., and more or less agreeing in this—that light exerts an adverse influence on pathogenic bacteria, and a stimulating one on healthy processes in the animal body.

[2] "Bacteria in their Relation to Vegetable Tissues" (The Friedenwald Co., Baltimore).

attack the starchy stores of reserve-material or the sugary fruits of the hosts, and it is precisely here that, as has been mentioned above in the case of fungi, thin partitions divide the saprophyte from the parasite. On the other hand, there are cases, like the blight on oats, where all parts of the young plants are affected, and, in short, there is a pleasing variety in the organs attacked. I am far from denying all these cases, but in matters of this kind it is, above all, necessary to "prove all things," and I prefer to retain, at all events in some cases, a "skeptical" attitude, especially as the data we possess are "meager." Not so, however, with Dr. Russell's own experiments, which are a valuable contribution to the literature of the subject, and a most promising performance. Some of his statements are open to comment, if not to criticism, on minor points, but this is hardly the place for a discussion of methods, etc., and the whole thing is so interesting a study in immunity that I summarise his conclusions. "The artificial inoculation of higher plants with different micro-organisms (not known to be pathogenic for plants) reveals the fact, contrary to the usually-accepted idea, that quite a goodly number of different species are able to withstand the action of the living plant organism for a not inconsiderable length of time." Of these he finds saprophytes particularly prominent—but not all equally so. Facultative parasites on the animal body were not found to be adapted to live in plant tissue, with the exception of *B. pyocyaneus* and the Schweineseuche *Bacillus*. "The inoculation of plants, not taxonomically related to the natural hosts of bacterial plant parasites, with species of micro-organisms naturally parasitic on vegetable tissue, showed that while the bacteria were unable to spread, they could survive at the inoculation point in large numbers. . . . Not only were numbers of different species of bacteria able to *live* in the plant from 40 to 80 days or more, but many of them (mostly saprophytes) were able to *spread* throughout the tissue of the plant to a limited extent (20 to 50 mm. or more)." This spreading was generally intracellular, not intercellular, and always in an upward direction. Dr. Russell thinks that this was not owing to the transpiration stream, but to the actual growth of the micro-organism. In the case of bacteria entering by wounds, he thinks it possible that they could enter through lesions so small as to escape notice, and that they might even live in the tissue after the wound has healed over. "In the case of parasitic species on plants, they sometimes effect an entrance into tissues without the intervention of wounds of any sort." The exemption of plants from bacteria in general, he distinguishes as *resistance*; while he reserves the term *immunity* for "the ability of a certain group of plants to be refractory towards a disease germ that is able to cause a pathological condition in closely-allied forms of plant life." This certainly seems an unnecessary distinction, because it is scarcely possible in any case to draw the line for immunity unless one is a "harbitrary gent," though the

idea that underlies this proposal is one generally recognised. At the present stage of our knowledge, or ignorance rather, of resistance, immunity and predisposition, it is hardly wise to introduce binding definitions of useful general terms. His other conclusions are of a more general and obvious kind, but the whole paper is permeated with the true spirit of research and experiment, and its author, if as yet somewhat credulous, deserves congratulation on a very promising piece of work.

GEORGE MURRAY.

VI.

The Causes of Variation in the Composition of Igneous Rocks.

FEW questions occupy a more important place in the mind of the physical geologist at the present time than why, within certain limits, the composition of eruptive rocks varies. Numerous opinions have at different times been expressed, but none of them have stood the test of time, nor have they at any epoch been universally accepted.

When we come to consider in general these different views—maintained, many of them, by both careful observers and clear thinkers—we find that they fall into two groups. There are those who hold the existence of two extreme pastes, the mixing together of which affords all the intermediate varieties of eruptive rocks; while the opposite school are equally confident that from a single original paste have been derived the numerous varieties of igneous rocks.

Numerous theories have been suggested to explain the differentiation of the two extreme pastes as held by the first school, and these explanations, with others, have been based on the action of the same agents as those which differentiate the common magma believed in by the second school.

Many of the hypotheses grade into each other, and some authors hold more than one agency at work; while others are somewhat mixed in their ideas as to the influence of one cause as separate from another.

As for the first school, they maintain that either whole or part of the fluid rock portion of our earth was, at one time or another, separated into a number of concentric shells, just as we find air separate from water.

This, at first sight, seems highly plausible; and I think we can concede they are right when they consider forms of matter of totally different molecular structure, such as air, water, oxides, and metals, just as we could confirm the same truth with such substances as mercury, chloroform, water, and oil, which remain separate when poured into a tube, in the order of their density, and, however much they are mixed together, separate again.

With volcanic rocks the question is somewhat different. The fundamental paste of these is composed almost entirely of oxides—some basic, others acid, but the complete intersolution or miscibility of which, in a fused state, has never been disproved, nor does it seem likely to be, as they all have a common tie in oxygen. When they cool, then other conditions are introduced, which do not concern us at present. Soret's principle will, no doubt, be urged against this statement; but in that case we have to deal with experiments on bodies much less nearly related, and under conditions of far less viscosity than those of an igneous paste, and with extremes of temperature far different to those likely to occur in masses of the enormous dimensions of the earth. Neither must we neglect the influence of tidal action, convection currents, and many other mechanical disturbances, such as shearing between the different fluid shells in the direction of the earth's rotation, supposing such continuous fluid shells to exist within our earth at present. In the case of localised reservoirs, there would be the absorption of water, vesiculation in the paste, currents sent up by the injection of earth fissures, or eruption of some of the material on the earth's surface, and other mechanical disturbances.

The oxides fused together in the manufacture of glass, which vary still more in their relative specific gravities than those which constitute igneous rocks, are not known to separate in the glass pots, however long they are left quiet. It may be objected that the depth, time, and even, perhaps, temperature are hardly comparable in the two cases, but we must not forget that even the most basic glasses are markedly viscous at high temperatures. These views may conveniently be styled the "differential paste and alloy hypothesis."

Mr. C. E. Dutton[1] says, "We know of no natural processes capable of separating the more acid parts of such a magma except the chemistry of the atmosphere acting at temperatures far below the melting-points of the silicates." He urges the following chemical argument against such a hypothesis:—The separation of a magma into two or more degrees of acidity is disproved by the low percentage of silica in basalt not being confined to the felspar and augite, but being also in the base, while the high percentage in rhyolite is in the felspar, and still more in the base. Hence the segregation must have affected the base more than the crystals.

The only way out of this difficulty is to suppose crystal segregation and refusion without subsequent mixing. I do not deny the possibility of such a process occurring occasionally; but to imagine that under every volcanic region, and under a large number of individual vents, there is an apparatus as complex as a chemical factory or an iron foundry, can only be relegated to those fantastic theories of volcanic magic that pass for science.

[1] "Geology of the High Plateaus of Utah," 4to, Washington, 1880, p. 124.

Neither is the idea of the segregation and settling down or deposition of certain crystals a very comprehensible one. It is hard to deny that such a process might take place, but it seems highly doubtful that any but very large crystals could undergo sinkage in a highly viscous medium; besides, it is not true that minerals separate in the order of their basicity or of their specific gravities, a fact I pointed out several years ago.[2] Leucite, for instance, is only formed in the open chimney of a volcano, and depends, like some of the augite, with probably sodalite, haüyne and nepheline (in some cases only), on the liberation of the alkalies and alkali-earths from their chlorides and sulphates by the dispersal and escape of their acid radicle vapours. These bases can then take up the silica from the residual basic glass, thus forming leucite and magnetite.

Again, we find that several mineral species become individualised contemporaneously. In many rocks, felspar and magnetite or augite, or again augite and leucite, were certainly separating at the same time; now if these sink together we shall have a rock of rather curious composition. Brögger and Vogt seem to believe that segregation takes place at certain points of a reservoir where the conditions are more favourable to the crystallisation of the one or the other mineral species. This much appears not improbable, and would easily explain the peculiar iron ores described by Vogt, and many rocks that have cooled in place, but it is hardly applicable to igneous effusive rocks in general.

Other suggestions that have been offered may be denominated the "fusion hypothesis," and the "osmotic hypothesis." Professor Sollas has demonstrated how, in some Irish igneous rocks, fragments of other igneous rocks of different composition and earlier date have been dissolved in a later magma.[3] Bäckström[4] and Fromm[5] have likewise described the local changes produced by quartz and felspar inclusions in basic rocks, but their observations are limited to the immediate neighbourhood of the enclosures. The former author found three kinds of augite in the enclosing rock. I have likewise shown that in basic and andesitic rocks of Stromboli quartz is fluxed down, forming with the residual basic glass an augite which becomes an important rock constituent.[6]

These facts in themselves are not of paramount value in the question, but they are clues to an important line of investigation that, when more carefully pursued, may afford valuable evidence as to change of composition in a primitive paste by adulteration with extraneous additions.

Ricciardi, on the evidence of his careful and elaborate analyses of

[2] *Scient. Proc. R. Dublin Soc.*, n.s., vol. v., p. 143, *et seq.*
[3] *Rep. Brit. Assoc.*, 1893.
[4] *Bihang t. k. Svensk. Vet. Akad. Handl.*, vol. xvi., pt. ii., no. 1.
[5] *Zeitschr. deutsch. Geol. Ges.*, vol. xliii., p. 43.
[6] *Rep. Brit. Assoc.*, 1893.

Italian volcanic rocks, suggested that the original paste was acid in composition, and that it became converted into basic rocks by the fusion of the Apennine limestone which supplied it with lime, magnesia, and iron. Herrick, Clarke, and Deming, although they do not go so far as Ricciardi, believe that basic rocks may be made more acid by the influence of the enveloping or "country rock," as they call it. They make no definite statement as to whether this is by fusion or chemical interchange.

The feeble point of Ricciardi's hypothesis is that, however magnesian or ferriferous be the Apennine limestone (for the numerous analyses of which we are indebted to that author and Abich), the amount of lime would be excessive in proportion to the other bases taken up by the lava.

For years I have been strongly impressed by the probability that, after all, we may have only one fundamental paste. It is not unlikely, admitting either a fluid shell between the earth's crust and nucleus, or even large isolated portions of such a shell, that this, during long ages since our globe consolidated, has been modified in composition by the rounding off of projecting inequalities of the undersurface of the crust or roof where thickenings and crumplings had taken place while cooling. Not only might the primitive cooled fusion-crust be reabsorbed, but the earlier sedimentary deposits might be likewise consumed, so that anterior to any fissure-filling a large extent of igneous paste would have changed in composition; but in such conditions I have little faith. Physicists will have it that our earth is practically a rigid mass; that pressure raises the temperature of solidification; and that therefore igneous rock at any considerable distance from the surface is solid, although at a very high temperature. In what way, therefore, can the "focal diffusion" and "segregation" processes have gone on? So far we have no evidence of segregation in a pure solid, but only in those solids wetted or permeated by a liquid. One escape from this difficulty is suggested by the last remark—namely, that perhaps this solidified mass of incandescent rock is composed of quite different minerals to those which reached the surface, undergoing liquefaction on the way; and that some silicates remain fluid and act as the diffusion agent. In fact, there is a complete analogy to what occurs in a marl, limestone, or clay, when water serves as the medium for the concretion of limestone, pyrite, flint, and other nodules; or for the more complete crystallisation of pyrites, selenite, hauerite, etc. But in all such cases the segregation is diffuse; by which I mean that in a stratum of marl we do not find the lime at one locality or part of the country and clay at another, but we encounter the resulting concretions scattered through the clay. If we imagine this to be suddenly fused, the resulting rock would have the composition of the original mud, and supposing that all issued by one vent, or cooled in place, we should not have a limestone rock here and a clay there.

But it will be argued that this segregation will occur in the primitive igneous material after it has liquefied by diminished pressure, and is on its way towards the surface. Then, it is true, it frequently occupies tortuous fissures where differentiation and segregation or sedimentation of crystals of the constituents might go on; but this could hardly explain the regular flow of definite but very different types of rocks from numerous points along a line of active vents, such, for instance, as the Lipari Islands. Here we observe, along a stretch of some 50 miles in length, a series of numerous vents from which outpours of basic dolerites and andesites simultaneously took place, and overlapped effusions of intensely acid lavas. In Vulcano for 2,000 years eruptions of an obsidian lava and a dolerite have been going on at vents only a mile apart. Is it conceivable that differentiation could progress, in the great fissure supplying these volcanoes, to such a complete extent that, without a difference of time, and in such a limited area, two extreme rocks could be poured forth and almost practically none of intermediate composition? What is said of this region is no less applicable to many others.

There yet remains one other possible cause, foreshadowed as far back as 1876 by the late Mr. J. C. Ward; that is, the interaction of the primitive igneous paste and the rocks it traverses or comes in contact with in its way into and through the earth's crust. Mr. Ward did not state clearly whether he meant by simple fusion of the walls of a volcanic canal or by a process of osmosis.

In my paper on Vesuvius and Monte Somma,[7] I drew particular attention to the subject, and in later writings those views have been extended and confirmed, but it will be advisable here to discuss the question in some detail. When we observe the denuded roots of volcanoes we find a certain amount of rock that has disappeared and has been replaced by the igneous intruder. We have little evidence in most cases to tell us whether the removal has been by mechanical export, fluxion, or fusion, though in many cases evidence points to the first as the principal means. I think it may be stated as a general law that contact metamorphism increases in intensity with the bulk of the igneous intrusion and the coarseness of its crystallisation. In other words, it is greater the larger is the amount of the heated medium, and the longer time this takes before solidifying, and, therefore, before its subsequent cooling. In contact metamorphism in regions which show evidence of being for a long time active volcanic centres, very marked chemical changes have been wrought in the solid rock—*elements have been introduced and* OTHER ELEMENTS HAVE BEEN REMOVED. All the elements introduced are derived, I presume no one will deny, from the neighbouring paste, which will have been modified in composition according to the percentages of those elements that passed into the cavity walls.

[7] *Quart. Journ. Geol. Soc.*, vol. xl., p. 54.

I claim, also, that the same paste has acquired those elements that have disappeared from the encasing rock. The change in composition of a lava by one only of these processes would not be very great, but when both are considered the change of an acid to a basic rock, or *vice-versâ* is quite possible.

In my paper on the ejected blocks of Monte Somma[8] (I. stratified limestones) and that on eozoonal structure in the same, by Dr. J. W. Gregory and myself, now being published by the Royal Dublin Society, it has been shown that calcareous rock acquires silica and later alkalies, and loses lime and magnesia in such a way that the magma, actually cooling in contact with it, is often reduced to an ultrabasic one. Are we not to suppose, therefore, that a less intense change does take place at a greater distance and under less favourable circumstances than these extreme and rather localised ones? Again, when we examine great areas of intrusive acid rocks, we often find their peripheral regions becoming more and more basic, and, perhaps, cases may be recorded of the reverse, according to the country rock. That these gradations are irregular is nothing more than we should expect, for the country rock surrounding such a mass will also vary in composition, and then circulation of the paste towards volcanic vents may have been greater at some spots causing mixing or carrying away the material. For succinctness we may call this the "osmotic" hypothesis.

If we suppose a number of vents, scattered over such an area, to open, the outpouring lava would differ according as it was derived from the purer or from the altered paste. A vent might drain out all one kind of paste, and following this might flow another of a different composition. The order in the composition of the rocks that issued might depend, therefore, on quite accidental circumstances. That not uncommon order of some regions commencing by the emission of basic rocks that become more and more acid and then, after an interval, terminate by a final phase of very basic rock, is quite explicable on this hypothesis. Let us imagine a great laccolite of an acid rock taking up its position among limestones. During a considerable period interaction takes place and a peripheral layer of basic rock is formed, being naturally basic at the surface near the limestone. This outer layer also becomes more and more aquiferous by the assimilation of H_2O from the enclosing rocks, until its tension rises enough for it to burst its way to the surface. The first outpour will be eminently basic, but as this drains off, the more and more central and acid layers will rise and follow. When sufficient has escaped to reduce the tension of the whole laccolite, and this becomes so viscous as to be unable to issue, the volcano will become quiescent, during which basification of the acid rock and absorption of H_2O will still progress, rendering it more fusible, and then, becoming more fluid

[8] *Proc. Geol. Soc.*, 1888, pp. 94–96, and *Trans. Edinb. Geol. Soc.*, vol. vi., p. 314, 1893.

and of higher tension, it will burst forth, forming the final phase of the volcano.

What may be the composition of the fundamental paste is still an open question, but I believe that it differs in different regions, so that in one volcanic province potash may be a dominant alkali, and in another soda, and so on. So far as my own individual opinion goes, I believe it was a rather acid rock, corresponding to a trachyte, and that either extremes have been caused by chemical alteration.

In fine, I do not deny that the modern hypotheses may each in part explain the cause of differentiations of pastes, but I do maintain that neither the "focal diffusion," "segregation," nor "sedimentation" processes are sufficient by themselves to do so.

H. J. Johnston-Lavis.

SOME NEW BOOKS.

THE MACLEAY MEMORIAL VOLUME, edited by J. J. Fletcher. Published by the Linnean Society of New South Wales. Pp. lii. and 308, frontispiece, and 42 plates. 4to. Sydney and London: Dulau & Co., 1893. Price 3 guineas.

SIR WILLIAM MACLEAY, who died on December 7, 1891, in his seventy-second year, was pre-eminently the patron of Natural Science in Australia. To his efforts were largely due the foundation and continuance of the Linnean Society of New South Wales, and to commemorate his munificence that Society now issues the present handsome volume, to which naturalists from various parts of Australia and New Zealand contribute a somewhat heterogeneous assemblage of articles of very various value.

The editor, Mr. J. J. Fletcher, prefaces the volume with an interesting account of the Macleays, and especially the eponymous William. As characteristic both of the man and of his biographer's literary style, the following description of an encounter with outlaws in 1864 may be quoted:—"His courageous conduct on this occasion and his commendable example in successfully asserting, rifle in hand, his right to travel on the high road when three desperate ruffians, Gilbert, Hall, and Dunn, one of them a recent murderer of police, held possession of it on the hill overlooking the inn, and having just finished with the Goulburn coach were actively engaged in the process of 'sticking-up' several teams and a number of travellers when Sir William, accompanied only by a boy who was driving the buggy, came on the scene and raised the siege, afterwards received official recognition by his being chosen one of seven gentlemen to whom in 1875 the Government awarded gold medals 'granted for gallant and faithful services' rendered during the period when bushranging was rife."

Professor W. B. Spencer contributes to our knowledge of *Ceratodus* a very thorough account of the blood-vessels of that interesting lung-bearing fish. He finds that both veins and arteries, while showing unmistakable connection with primitive shark-like types, have developed to a certain extent along lines parallel to those of amphibians. He considers that in the earliest forms in which lungs were developed for respiration, the mode of life was very similar to that of *Ceratodus*. The animal lived in water, and, at first at any rate and possibly for long, the lungs were only accessory to the gills. This condition is paralleled in the early life of the frog. In *Ceratodus* the lung is always in use, although the animal does not, as some have supposed, emerge from the water; but this organ is chiefly of service when the river water is thickly charged with sand or fouled by decomposing vegetation. The paper is illustrated by five plates in Mr. Spencer's admirably clear, though somewhat over-diagrammatic, style.

Captain F. W. Hutton follows with a systematic account of the Pliocene Mollusca of New Zealand, which he characterises as "the

remains of an earlier fauna disappearing rapidly before the conquering host of the recent fauna, which had invaded New Zealand some time previously; and if this idea is correct we might expect to find some of the Pliocene forms, which are now extinct on the shores of New Zealand, still lingering in the outlying islands." This appears to be the case, but further investigation is necessary. We hope that the four plates accompanying the paper will be more satisfying to the eye of the conchologist than they are to that of the artist.

Professor W. A. Haswell monographs the Temnocephaleæ, a peculiar group of worms parasitic on the outer surfaces of many kinds of water animals, to which they adhere by a sucker. Like those of most parasitic forms, the affinities of this group are hard to determine. The author concludes that they are probably Trematode rather than Turbellarian, although he sees little reason against regarding them as aberrant Rhabdocœlous Turbellaria, specially modified for a peculiar mode of life. Mr. Haswell also describes, as *Actinodactylus*, a new genus of flatworm, found on the burrowing crayfish of Gippsland; its most remarkable feature is a retractile proboscis. This author's seven plates, drawn by himself, are both clear and artistic.

Professor T. J. Parker and Josephine Gordon Rich describe the muscles of the New Zealand sea crayfish (*Palinurus edwardsi*), a piece of work that should prove of service to Australasian students of the Crustacea. Their chief conclusion of general importance is that "the great ventral mass of muscle in the abdomen, usually considered to act exclusively as a flexor, gives rise to slips which, being inserted into the terga of the segments above the hinges, and pulling in an almost horizontal direction, must act as extensors." The drawings are remarkably clear.

In the course of two papers, Messrs. J. T. Wilson and C. J. Martin show that the name "duck-bill" as applied to Ornithorhynchus is somewhat erroneous, since the muzzle of that animal is neither horny nor even leathery, but covered with a soft skin, like that of a dog's nose, richly supplied with sensitive organs of touch. The object of this is evident when we remember that the animal usually procures its food by raking away in the mud at the bottom of rivers for small larvæ, shell-fish, insects, and worms. The snout is supported by cartilage, which the authors regard as a persistent prolongation of the embryonic cartilaginous skull. The papers are illustrated by five plates containing some excellent micro-photographs.

Professor R. Tate endeavours to show, from an examination of the floras, that Norfolk and Lord Howe Islands are connected with the New Zealand rather than the Australian region, a conclusion that harmonises with the results obtained from the birds and land-shells.

Mr. R. Etheridge, jun., contributes an interesting description of some of the implements and weapons of the Alligator tribe, Port Errington, N. Australia, which were sent to the Chicago Exhibition.

Eighty-two species of Nematode worms, mostly Australian and Fijian, are described by Mr. N. A. Cobb. About half the species are new, and considerable attention is paid to anatomical details. Many of these worms are hurtful to vegetation, and the paper may, therefore, prove of economic importance; otherwise it contains little of general interest.

The volume also contains notes by Baron von Müller, Mr. J. H.

Maiden, and Mr. C. Hedley. Besides being a worthy memorial of a notable man, it does great credit to the Linnean Society of New South Wales, to the printers and lithographers of Sydney and Melbourne, and to all our Australasian colleagues.

[THE EARTH BEFORE THE APPEARANCE OF MAN.] La Terre avant l'apparition de l'homme. Périodes géologiques, Faunes et Flores fossiles, Géologie régionale de la France. By Fernand Priem. Pp. 716, text illustrated. Large 8vo. Paris: Baillière & Sons, 1893. Price 12 francs.

THIS volume forms part of a series entitled *Merveilles de la Nature*. As one of these marvels a restoration of *Triceratops* now graces the title page; but the author might very well have placed his own portrait there instead. For, only last year he produced a companion volume, *La Terre, les Mers et les Continents*, noticed in NATURAL SCIENCE, vol. ii., p. 314, and now he has given birth to the present monster, which contains more matter than 13 numbers of this Journal. The explanation of course is that these volumes are little more than compilations from well-known works in other languages, such as Neumayr's *Erdgeschichte* and Suess's *Antlitz der Erde;* and as such they demand little notice from us.

In some respects Mr. Priem essays to do for the French public what Canon Bonney has done for our own in the book reviewed last month. His descriptions are, however, far more technical and he nowhere achieves that distinction of style so characteristic of the English writer. This one would expect to militate against the popularity of his work, did one not know how much greater the acquaintance of the ordinary Frenchman with such matters is than that of the ordinary Englishman. On the other hand, if Mr. Priem's book is intended for serious students, it is by no means careful enough. Facts of which he was ignorant when he wrote *L'Evolution des Formes Animales* in 1890, he remains ignorant of to-day, through no fault of his critics (see *Geol. Mag.*, Dec. III., vol. viii., p. 515: 1891).

It is, however, only the first 453 pages of the present volume that are devoted to the popularisation of geology. The latter part, which is far more valuable to the world at large, consists of an account of the geology of France. The method adopted, which is the most useful one for the foreign visitor, is to describe the various regions rather than the formations. First is described the Central Plateau, then follow the Western Massif, Ardenne, the Vosges, the mountains of Maures and Esterel, the Alps, the Jura, the Pyrenees, the Sub-Pyrenean plain, the Paris Basin and, lastly, the Basin of the Saone and Rhone. This part is illustrated by numerous maps and sections, and supplies a want, which, so far as we are aware, had not been previously filled. It can hardly be called a pocket guide; at the same time our readers will be glad to learn that the work has been published in 24 Series at 50 centimes, and in 4 Fascicules at 3 francs, and that each series or fascicule may be bought separately. For this last fascicule, at least, we predict a good sale.

As a picture-book the whole volume may attract some attention, since it contains no less than 900 figures. All, however, are not equally good. Most of the interiors of Brachiopods are wooden in the extreme. *Dinornis parvus* has a miserable appearance. Fig. 637, *Megatherium*, fig. 624, the Mammoth, and fig. 355, a Pterodactyl, would have been destroyed long ago by any right-minded person, so full of errors are they. Some of the blocks seem to have been put into a lucky bag, and drawn out at hazard at the last moment: thus

we find *Osteolepis* masquerading as *Holoptychius* (fig. 164), while *Holoptychius* poses as *Osteolepis* (fig. 166); *Ctenodus* has lent his lower jaw to *Dipterus*, while *Coccosteus* and *Cephalaspis* have jumped into one another's shoes. It is to be hoped that no satire is meant when the author ascribes all these wrong determinations to Traquair. But Ammonites as well as fish have gone astray, so that on page 235 *Schlotheimia* and *Arietites* have changed places. Presumably Mr. Priem was already engaged in further stupendous labours that prevented him from seeing this work through the press. The man that takes all the world as his oyster will inevitably suffer from indigestion.

F. A. B.

SUICIDE AND INSANITY. By S. A. K. Strahan, M.D. 8vo. London: Swan Sonnenschein & Co., 1893.

THIS book, which Dr. Strahan calls a "Psychological and Sociological study," has been written to teach a lesson and to prove a theory. The lesson is one of charity and a broader sympathy with self-destroyers than the Church, and consequently public opinion, inculcate. The theory is one that has been gradually shaping itself in the minds of thinkers and gives the lie to the formal coroner's verdict, "Suicide while temporarily insane." Dr. Strahan divides suicides into two classes, Rational and Irrational. To the former belong those whose sanity has never been called in question. To the latter belong the vast proportion of those whose deaths are daily recorded in the newspapers or the registers of prisons and lunatic asylums, and are attributed to mental derangement. It is this class, naturally, that is subjected to physiological analysis. In a very lucid exposition of the causation of suicide, Dr. Strahan proves that suicide is no more an effect of insanity than are the criminal or homicidal impulse, epilepsy or alcoholism, but is, with these, a special manifestation of degenerate vitality of the race. In a tainted family we see, side by side with insanity and epilepsy, the predisposition to suicide. Each is a special form independent of the others, although a combination is frequently found in the same case. Starting from the two fundamental instincts of animal life, self-preservation and procreation of species, Dr. Strahan shows that voluntary death, being a confession of the absence of the former of these instincts, "becomes merely one of the eliminative processes of Natural Selection." In aiming at a popular style, the author sometimes seems to fall below the dignity of scientific dialectics. To give footnote references to well-known Shakesperian quotations, for instance, lends an unpleasant suggestion of amateurishness in literary work. This fault, and an unfortunate misprint, "*æsthetics*" for *anæsthetics* (p. 212), it would be well to remedy in a second edition. But these minor blemishes are lost in the soundness of Dr. Strahan's argument and the earnestness of his purpose. On the whole, his book is the most significant one yet written on the subject.

THE DISPERSAL OF SHELLS. By H. W. Kew, with a preface by A. R. Wallace, F.R.S., etc. (International Scientific Series.) London: Kegan Paul, Trübner and Co., 1893. Price 5s.

IN recommending this book to the public, Mr. Wallace, in our opinion, says rather more than is necessary. Many books, he remarks

in effect, even when full of descriptions of scores of new species with anatomical details to match, and worked out in the most elaborate and accurate fashion, are of less interest to the philosophical naturalist than Mr. Kew's little book. Now Mr. Kew's book is the work of a diligent collector of facts bearing upon the means of dispersal possessed by land- and fresh-water Mollusca; the facts are some of them new, but the general gist of the book is far from new; it is merely a slight extension of a matter which, in its main outlines and even in details, is already well-known. A naturalist, moreover, whose interests are limited to the phenomena of Distribution—important though that subject is—can hardly be called a "philosophical naturalist." If Mr. Wallace had not directly challenged comparison, we should have contented ourselves with an expression of opinion that Mr. Kew has done an extremely useful piece of work. But we must say now that it is neither highly original nor abounding in new and important truths. The fact of the matter is, that the kind of naturalist to whom Mr. Wallace refers when he uses the expression "philosophical," is apt to be alarmingly unacquainted with the structure of animals, and to be a dabbler in what is generally known as bionomics. There is a tendency, which we deplore, to unduly exalt trifling observations upon the "habits" and "intelligence" of animals. To make such observations needs comparatively little knowledge; to write monographs like those which Mr. Wallace passes over so lightly, needs not only skill of eye and hand, but wide knowledge, and, above all, reasoning power and a sense of proportion. We are far from thinking otherwise than well of Mr. Kew's book, but it is not exactly epoch-making.

ROMANCE OF THE INSECT WORLD. By L. N. Badenoch. London: Macmillan and Co., 1893. Price 6s.

THIS is a nicely got-up little book describing in a pleasant fashion some of the more familiar facts about insects. It is not quite obvious why what the author has to tell us should be deemed worthy of gilt edges and cloth sides; we should have thought that a magazine article or two would have sufficed to unburden the writer of his accumulated stores of information. A deliberate book calls for some special acquaintance on the writer's part with the subject upon which he proposes to enlighten his readers; but there is not much evidence of such a special—or, indeed, even a more general—knowledge in this work. The author should not venture upon the quicksands of technicalities without some more defensive protection in the way of fact than he appears to possess. Insects, for example, are defined as "animals formed of a series of rings or segments." This is, doubtless, perfectly correct; but the definition is truly, as he terms it, "broad." It is equivalent to defining man as an animal with a backbone. The concluding section deals with a much debated matter—the questions of "protective resemblance," "mimicry," etc. But Mr. Badenoch evidently has not the slightest idea that there are any debated points relating to the facts which he so glibly sets down. The facts, too, are of the most worn-out character, which have done duty again and again. Those over-worked Heliconidæ cause the gorge to rise in more than one sense. Mr. Badenoch seems to have read Mr. Poulton's "The Colour of Animals," but he should also have consulted Mr. Beddard's "Animal Colouration," which has at least the merit of impartiality. A "Glossary" at the end of the book contains definitions "of the principal scientific terms used." It may be necessary, though we

should have doubted it, to inform the reader that *concentric* signifies "having one common centre"; but surely even the most benighted of "general readers" knows what *oval* means. If he does not he will not get very accurate information from Mr. Badenoch, who considers that it is the equivalent of "oblong"!

PÊCHES ET CHASSES ZOOLOGIQUES. Par le Marquis de Folin. Bibliothèque Scientifique Contemporaine. Pp. 330, 117 figures. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

THIS is a charming little book on natural history, illustrated by quaint pen and ink sketches of animals, places, and apparatus. It is designed evidently to interest the amateur in the familiar objects of the animal world, and to guide him in the methods of looking for and capturing animals and plants. The reader is first taken to the sea-shore, and when he has spent an hour at the limits of the receding tide, his whetted curiosity is taught to satisfy itself with drag-nets and tow-nets. Thereafter he works his way through Algæ and Fungi and Flowering Plants, and then up through the animal kingdom from Infusoria to Mammals. Excursions to the bottom of the sea, discussions of phosphorescence and of animal electricity, enliven his progress, and a great deal of accurate classification and external anatomy is administered to him in gentle doses. A special and most valuable feature is that the book deals, so far as possible, with exact places, the special features of which are described, and the animals which may be found there specially mentioned. It is a book to be commended to local natural history societies for its method and treatment.

OUTLINE OF THE GEOLOGY AND PHYSICAL FEATURES OF MARYLAND, with a coloured geological map of the State and 16 plates and charts. By Professor G. H. Williams and Dr. W. B. Clark. 4to. Pp. 67. Baltimore: Johns Hopkins Press, 1893. Price $1.

THIS is an extra edition of the description of the Physical Geography and Geology of Maryland recently prepared for the World's Fair Book on the State's resources. It embraces the topography, climate, geology of the plateau, mountain and plain, and a brief account of the distribution of mines and minerals. This text is illustrated by a new geological map of Maryland, six coloured charts to show the distribution of rainfall and temperature, and ten full-page plates of various types of scenery having a topographic or geological significance.

The map, which is on a scale of 1:500,000, approximately eight miles to the inch, represents in twenty-nine colour distinctions the present state of our knowledge of Maryland's geology, and the harmonious arrangement of these colours renders it a most pleasing specimen of chromo-lithography. It is based upon the work of Tyson, I. C. White, Geiger, Keith, Darton, and G. H. Williams, and contains much information never before published. It is accompanied by a tabulation of the soils belonging to the different geological formations by Professor Milton Whitney.

The work may prove of some general interest, since the State of Maryland contains geological formations in almost unbroken sequence from Archæan to Recent. The ancient crystalline and semi-crystalline rocks of the Piedmont plateau appear to be metamorphosed rocks of Archæan, Cambrian, and Ordovician age. Fossils, it is true, have not been found, neither is there any direct evidence of their sedimen-

tary origin, beyond the fact that beds of different composition rapidly alternate, and have marbles and quartz-schists intercalated. The Appalachians and Alleghanies consist of folded Palæozoic rocks, from Cambrian to Carboniferous, with older eruptive rocks in places. The eastern part of the State is known as the Coastal plain, and is composed of slightly consolidated rocks of Cretaceous and Tertiary age. The numerous unconformities which these present are evidence of successive periods of elevation and depression. English students of the Tertiary rocks would be interested in the highly fossiliferous Pamunkey and Chesapeake formations, which contain many species of mollusca hardly to be distinguished from our own.

The charts are based upon a full investigation of all data relating to the climate of Maryland. Much information has been gathered on this subject, especially since the organisation of the State Weather Service, and these charts and the accompanying tables of observations, now for the first time bring the results together in a compact form. The climate, like the surface of the country, is greatly diversified, and the comparisons instituted will prove of interest to meteorologists. We miss, however, the promised section on Medical Climatology.

The style of the work is to be commended for its clearness and conciseness; it will probably obtain for it more readers than would have been found for a more ambitious and more technical description. After all a scientific work may be intelligible sometimes.

We may also add that we have received a "Guide to Baltimore," originally prepared by a local committee for the use of the American Institute of Mining Engineers at their Baltimore meeting. This contains an account of the geology of Baltimore and its vicinity, more detailed and more special than that in the work just reviewed. The account of the crystalline rocks is by Professor G. H. Williams, and that of the sedimentary rocks by Mr. N. H. Darton. This book is accompanied by two of the Geological Survey maps of the neighbourhood—one bringing out the features of the crystalline, and the other those of the sedimentary rocks.

F. A. B.

THE MINERAL RESOURCES OF WESTERN AUSTRALIA. By Albert F. Calvert. Pp. 179. London: George Philip & Son, 1893. Price 2s.

WESTERN Australia comprises about one-third of the great island-continent, but it is a colony which is said to have "slumbered for over fifty years." It is now slowly waking up, though its population in 1891 was estimated at a little under 50,000 persons, exclusive of Aborigines; or one individual to about every twenty square miles. There is room therefore for some of the unemployed who are prepared to do hard work in return for a moderate income; and there is room also for the capitalist whose aim would be to assist in developing the resources of the country, without seeking to make enormous profits. The colony is not well off for water; but this present drawback to progress can be overcome by the construction of reservoirs and the sinking of deep wells.

In the little volume before us Mr. Calvert gives accounts of each of the gold-fields of Western Australia; a subject which occupies the greater part of this work. In addition, there are brief notes on tin-fields, the Collie coal-mines, etc. He acknowledges assistance from the reports of the present Government geologist, Mr. H. P. Woodward, and of his predecessor, the late Mr. Hardman, in whose reports

full particulars of the known mineral resources of the colony will be found.

There is no doubt that rich gold-fields exist, some of unknown extent, but enterprise has been arrested by the scarcity of water, and the want of railway communication.

WOODWARDIAN MUSEUM, CAMBRIDGE.—CATALOGUE OF THE FOSSILS IN THE STUDENTS' STRATIGRAPHICAL SERIES. By H. Woods. 8vo. Pp. 24 (interleaved). Cambridge, 1893.

THIS little pamphlet contains a list of the typical fossils of each formation, together with a note as to their class or order. We recall a collection of a similar nature in the Edinburgh Museum, which has been arranged by Mr. Goodchild, and at the present moment Mr. Etheridge is soliciting specimens for a like purpose in the British Museum. We would suggest that Mr. Woods, in his next edition, might add the authors' names to the species, for it is most important that the student should be trained to associate the literature of the fossil with its name.

LIEFERUNGEN 7 to 9 of the new edition of the Molluscan portion of Bronn's "Klassen," edited by Simroth, have just lately been received. They contain the completion of the Aplacophora and the first few pages of the Polyplacophora.

The following is the author's subdivision of the former order so far as the families are concerned :—

SUB-ORDER.	FAMILY.
I. Chætodermatina	1. Chætodermatidæ.
II. Neomeniina	1. Neomeniidæ 2. Proneomeniidæ 3. Dondersiidæ 4. Parameniidæ

We regret, however, to notice that new genera, subgenera, and even species are introduced and not always fully described. New forms, even in such a small group as this, should, we venture to think, be previously described elsewhere: their inclusion in the first instance in a work of this character, where they are out of harmony, is a mistake, and unnecessarily complicates the subject-matter, which should be kept strictly to its proper broad lines.

VERY useful to students of Ammonites is a pamphlet by Dr. J. F. Pompeckj (Beiträge zu einer Revision der Ammoniten des Schwäbischen Jura. Lief. i. Stuttgart: E. Schweizerbartsche Verlagshandlung, pp. 1–94, pls. i.–vii., 1893). This is nothing less than the translation of the remarkable trinomial and sometimes quadrinomial nomenclature, employed by Quenstedt in his great works on Ammonites, into the modern system. Thus, *Ammonites angulatus intermedius gigas*, as Quenstedt named one important species, is concisely rendered into *Schlotheimia intermedia*. The present part deals with the genera *Phylloceras*, *Psiloceras*, and *Schlotheimia*; and besides the translation of its name, each species is critically considered, and mistakes in identification corrected; while new species of Swabian Ammonites are described. Future instalments of the work will be awaited with interest. We only hope that the author will not treat *Arietites*, which he incidentally mentions, as a valid genus. It is forestalled by Hyatt's *Arniceras*, *Coroniceras*, etc., whose

limits and characters are distinctly defined, on correct morphological principles, in that author's "Genesis of the Arietidæ" (*Smithsonian Contrib.*, 673).

The *Società Malacologica Italiana* has published in its *Bolletino* (xviii., pp. 73–108) a paper by Guido Bonarelli, which is of considerable value to students of Ammonites, being a descriptive catalogue of over thirty species, which the author unites under the new generic name *Hecticoceras*. The type-species of the genus is *A. hecticus*, Reinecke, while *A. lunula*, Reinecke, serves as type-species of a subgenus *Lunuloceras*. It is to be hoped that these new genera will prove to have been formed better than their names. Professor Bonarelli considers that *Hecticoceras* belongs to the family Oppelidæ, and that it was derived from *Oecotraustes*. This opinion is probably correct; but then *Oecotraustes*, as at present constituted, includes rather a heterogeneous assemblage of Oppelidæ, and would, in fact, form a worthy object for the learned author's further investigations. Meanwhile, the present paper marks one more forward step towards the true classification of the Ammonites.

The Royal Scottish Geographical Society's Atlas of Scotland is announced to be published by the Edinburgh Geographical Institute (J. G. Bartholomew & Co.) next June. It will comprise a series of sixty-two plates of maps and plans, illustrating the topography, physiography, geology, natural history, and climate of the country; these being accompanied by an explanatory and statistical text. The completion of the publication of the Ordnance Survey Maps, the advanced progress of the Admiralty and Geological Surveys, and the large collection of data, produced by the recent activity of Scottish scientific societies, in the departments of Meteorology, Natural History, and Archæology, seem to make the present time most favourable for the production of an Atlas in which that valuable work shall be summarised and incorporated.

In Natural Science, vol. ii., p. 385, we noticed M. Guinard's book, "Précis de tératologie"; we have now received a smaller volume, by Louis Blanc, "Les Anomalies chez l'homme et des mammifères" (Ballière, Paris, 1893, 328 pp., price 3 fr. 50), in which the student will find a great amount of useful and interesting information. The volume is fully illustrated.

In referring last month (p. 77) to Mr. Minchin's translation of Professor Bütschli's work on Protoplasm, we erroneously ascribed its publication to the Clarendon Press. English-speaking students are indebted to the firm of Messrs. A. & C. Black for this enterprise, and the volume will probably be issued early this month.

Captain H. G. T. Swayne, who has made seventeen shooting and exploring trips into Somali Land, North-East Africa, is writing an account of the country, which will shortly be published by Rowland Ward & Co., of 166 Piccadilly, W.

Messrs. Dulau & Co. have issued part xxxi. (Reptilia and Amphibia) of their Catalogue of Zoological and Palæontological Works. Numerous rare pamphlets are included in this useful sale-catalogue.

OBITUARY.

ARTHUR MILNES MARSHALL.

BORN JUNE 8, 1852. DIED DECEMBER 31, 1893.

THE Owens College and the Victoria University have suffered, by the sad death of Professor Marshall, an irreparable loss; in him they have lost not only one of their most brilliant professors, but also one of the most loyal of their servants.

Ever bright and cheerful, no difficulty ever daunted him, and no amount of work appeared to be too much for him. He was one of



ARTHUR MILNES MARSHALL.

(From a photograph by Warwick Brookes, Manchester.)

those rare men who are always working and never tired, although they do more work than any average man could do in twice the time—always busy, but never too busy to give help to those who needed it. His clear insight into the practical problems which had to be solved, combined with unusual foresight and tact, enabled him to avoid the difficulties in which others were constantly entangled; but when a serious difficulty was actually encountered, no man was more

ready than he to attack it or give his aid to those by whom it had to be faced.

The obstacles which have to be surmounted in the founding of a university are familiar to those who have exerted themselves to promote the schemes for the foundation of the Victoria University, and of a teaching university for London, but those obstacles present very different aspects to the two bodies of men. When the difficulty has been overcome, the contemplation of it becomes a source of pleasure instead of a cause for anxiety. To Marshall, a difficulty ahead served as a pleasant stimulus; he rejoiced in overcoming difficulties, just as he rejoiced in climbing a mountain peak. Cautious and expert as a mountaineer, caution and expertness enabled him to contemplate a great difficulty, if not with pleasure, at least, without fear. Difficulty as a matter of history was familiar to him: unconquerable difficulty almost unknown. And to this, no doubt, his perennial happiness was largely due, for he was always happy.

Too clear-sighted not to see what was his own personal interest, he did not hesitate to sink that interest when it was necessary for the success of the undertaking; and the readiness and cheerfulness with which he did it led others to promptly make similar sacrifices. But he did not regard them as sacrifices, for his great aim was never personal gain, and he had the good fortune to be associated with others who equalled, though they could not surpass, him in unflinching loyalty to the College, to the new University, and to the cause of higher education generally.

The success of the Victoria University Extension movement has been due largely if not mainly to his efforts and his great power of organisation, and especially to his tact in adjusting conflicting interests. He thoroughly believed in the usefulness of the extension scheme, and threw himself heartily into the work—and the scheme has succeeded. He did not make it a success single-handed, but those who co-operated with him did so the more heartily because of his encouragement.

Many of the zoologists who knew him, and probably they alone, looked with disapproval upon this expenditure of his time—the energy did not much matter, for he seemed to have an unlimited supply of it. From a purely zoological point of view, this organising work was no doubt a loss to science. He himself ardently wished to find time for more scientific work, and he had at the time of his death already made preparations for new and elaborate researches. He was, however,—as befits a professor—always ready to devote his time to helping others in any kind of biological research.

Two volumes of "Studies from the Biological Laboratories of the Owens College" have already been published, and it was intended to publish another very shortly. Of the twenty-one papers included in these two volumes only four were prepared without his aid and inspiration, and of these three are botanical.

His enthusiasm was infectious, and he attracted large audiences to his popular lectures—audiences composed chiefly, not of "those who with itching ears run about after popular preachers," but of those who had been inspired with interest in biological science by his previous lectures.

The Manchester Microscopical Society, of which he was president for the last seven years of his life, is composed of enthusiastic naturalists, and it is largely because of the earnestness which his example has called forth that wealthy mill-owners and justices of the peace sit in the meetings of the society, unconscious of social inequality, cheek-by-jowl with poor men who work for a weekly wage; and to that same beneficent influence may be ascribed the fact that, under the auspices of the society, systematic courses of biological instruction are undertaken by teachers whose services are given gratuitously and willingly.

Marshall was only 41 years of age, and there was every reason to believe that in the next few years he would have accomplished far greater scientific work than he had as yet done; but there are few men who have done so much for the advancement of education in so short a time as he has, and yet have found time for even a little scientific work—and his scientific work is more than a little.

Like his friend, F. M. Balfour, who met with a similar untimely death on the mountains, he was a keen embryologist. He thoroughly believed in the theory of "recapitulation," and never lost an opportunity of convincing others of its truth. At Leeds, in 1890, he said:—

"That ontogeny really is a repetition of phylogeny must, I think, be admitted, in spite of the numerous and various ways in which the ancestral history may be distorted during actual development."

That sentence expresses the belief which formed the basis of his teaching. That theory was the source of the inspiration under which his latest published work was written, *i.e.*, his "Vertebrate Embryology," published only a few months before his death.

Of that work no review has appeared in the pages of NATURAL SCIENCE, but Professor Lankester, in reviewing it in *Nature*, says:—"It is not too much to say that he has produced a most valuable, clear, and masterly exposition of the known facts of the developmental history of leading types of vertebrata." It may be added that the book is very liable to give the impression to the reader that it is chiefly a compilation. But this is not correct. He repeated the observations of his predecessors wherever it was possible to do so, and published the results. It is not surprising that his predecessors were often right, and where they were right, he reproduced their accounts, and even their figures. Many of the figures, however, were corrected before they were reproduced, and the book contains

considerably more than a hundred new ones, drawn by the author himself direct from nature.

Like other books which Marshall published either alone or conjointly, this was intended as a laboratory manual, and hence the "type system" had to be adopted; but it was really adopted for another reason. He was keenly alive to a source of error which is too commonly ignored—the assumption that what is true of one animal is true also of its allies, or nearly so. It is this assumption which seems to be taken implicitly as a justification for the description of changes in the course of development of man, which, whether they occur or not, have not been seen by those who have described them, or by those who have been quoted. It is this, also, that leads to the wholesale illustration of books on human anatomy by figures intended to illustrate the structure of man, though they have been drawn from animals sometimes as unlike man as dogfishes and birds. "Comparative Embryology" is one thing: the indiscriminate confusion of the embryonic development of one animal with that of another is another thing altogether. Marshall has produced the first book on the development of man and other vertebrates which is free from this confusion. He felt strongly the necessity for having a straightforward description of the development from beginning to end of a few typical vertebrates before attacking anew the larger problems of comparative embryology. That he did not live to attack anew those larger problems is a pity too sad for expression in words.

Besides this great embryological work, he published, alone and conjointly with others, several papers on vertebrate morphology, and especially on the morphology of the head. Other papers dealt with the development of the kidneys and fat-bodies, the abnormal conditions of the reproductive organs, and the development of the blood-vessels of the frog.

His chief invertebrate work was upon the Pennatulida, and one of the papers on this subject was written in collaboration with his father. He worked also upon the structure and physiology of *Antedon* (*Comatula*), and published a paper on its nervous system.

His published lectures and addresses are numerous, the presidential address to the Biological Section of the British Association (Leeds, 1890) and a paper on the morphology of the sexual organs of *Hydra* being, perhaps, the most important of them.

This may seem a small amount of scientific work for a man of his age, but those who knew Marshall best, know best how small a fraction it is of the work he did for the advancement of learning. They who would judge him by his published work would do him a gross injustice.

There is no great scarcity of zoologists who might efficiently perform the strictly zoological work of the professorship, but it is not probable that a successor will be found who is willing to devote so much time and energy to other business of the College and University

as he did. It is even less probable that one will be found who is able to render such services with like tact and good humour.

"Few men have enjoyed life more, still fewer have used it better."

C. H. H.

RICHARD SPRUCE.

BORN 1817. DIED DECEMBER 29, 1893.

THE botanical world has sustained an irreparable loss in the death of the famous botanist and traveller, Richard Spruce, who succumbed to an attack of influenza on the 29th of December last. Mr. Spruce was born in the year 1817, and was a native of the North Riding. It is nearly fifty years since he published his first paper, which dealt with the Mosses and Hepatics of Teesdale. Shortly afterwards he spent a year in the Pyrenees, collecting the flowering and cryptogamic plants of that region; his paper on the Mosses and Hepatics, published in 1849, was a very valuable addition to our knowledge of the European flora. In the same year he went out to South America, and travelled up the river Amazon, making extensive botanical collections. During two years he was, more or less, with Dr. Alfred Russel Wallace; and, when the latter came home, Mr. Spruce proceeded up the Rio Negro, crossed over to the Orinoco, returned to the Rio Negro, and exploring various of its tributaries, gradually made his way up into the Andes. Here he received a commission from the India Office to collect seeds and plants of the Cinchona, which was then rapidly becoming exterminated. In this he was most successful, and hundreds of young plants were safely transferred under the fostering care of Mr. Robert Cross from the western slope of Chimborazo to India, and established in the Himalaya. Mr. Spruce's travels are described at some length in the *Journal of Botany* between the years 1849 and 1864. He returned to England in 1863 a permanent invalid. His collections stand unrivalled, both in the immense number and the beautiful completeness of the specimens. His bad health prevented him from working out more than the Palms and the Hepatics. Mr. Bentham did the Phanerogams; Mr. Mitten the Mosses; the Rev. W. A. Leighton the Lichens; and the Rev. M. J. Berkeley the Fungi. Mr. Spruce's Hepatics of the Amazons and Andes occupies the whole of vol. xv. of the *Trans. Bot. Soc. Edinburgh*, and is a novel and most scientific treatment of the subject. The majority of his other papers were devoted to his favourite study—the Hepatics. The last thirty years of his life, rendered grievous by his chronic state of ill-health, were spent at Coneysthorpe, in North Yorkshire, where he died at the close of 1893, at the age of 76.

ROBERT BENTLEY.

BORN 1821. DIED DECEMBER 24, 1893.

THE late Professor Bentley was born at Hitchin, in 1821. After leaving school he was apprenticed to William Maddock, a chemist at Tunbridge Wells, and, having served his term, became an assistant at the establishment of John Bell & Co., in Oxford Street. He joined the School of Pharmacy in Bloomsbury Square at its foundation, and subsequently entered as a medical student at King's College, becoming, in due course, a member of the College of Surgeons. He commenced the study of botany during his apprenticeship, and when at Bloomsbury attended Dr. A. T. Thompson's lectures, and gained the first botanical prize awarded by the institution. His connection with the Pharmaceutical Society was almost lifelong, and it was to members and associates of the Society that he was best known. In 1887 he resigned the chair of Botany, and was shortly after elected Emeritus Professor. He was also for some time Professor of Botany and Dean of the Medical School at King's College, and Professor of Botany at the London Institution. He was twice President of the Pharmaceutical Conference, at Nottingham in 1866, and in the following year at Dundee, and for many years acted as chairman to the Garden Committee of the Royal Botanic Society, Regent's Park, where he annually gave a course of botanical lectures. His most important scientific work was that on "Medicinal Plants," which he shared with Mr. Henry Trimen, a most valuable book of deservedly wide reputation. Bentley also produced a small text-book on Botany, which passed through several editions, and was the author of numerous papers, chiefly of botanical interest, in the Pharmaceutical Society's *Journal*, the editorship of which he shared for about ten years. He was buried on December 30 at Kensal Green.

We are indebted for several of our facts to an obituary notice by Mr. O. Corder in the *Pharmaceutical Journal* of January 6.

THE deaths are also announced of SIR SAMUEL BAKER, the eminent African explorer; of PIERRE VAN BENEDEN, the veteran zoologist, of Louvain; of DR. J. BOEHM, the botanist, of Vienna, at the age of 62; of BARON KARL VON KÜSTER, the botanist; and of MAJOR JOHN PLANT, late of the Salford Museum. We hope to give some account of the work of SIR SAMUEL BAKER and other recently-deceased African explorers next month.

A LONG obituary notice of the late HENDRIK RINK, by Dr. Robert Brown, appears in the January number of the *Geographical Journal*.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE Trustees of the George Henry Lewes Studentship in Animal Physiology, tenable for three years at Cambridge, London, or any Continental University, have elected Dr. John W. Pickering.

AMONG the lectures in Natural Science at Oxford during this term, the following are announced:—Professor Lankester and Mr. W. B. Benham, an advanced course on Reptiles and Birds; Mr. Benham on the Oligochaeta; Mr. G. C. Bourne on the History of Zoology; Professor Burdon-Sanderson and Mr. J. S. Haldane on the Physiology of the Nervous System; Mr. Haldane on Physiological Chemistry; Mr. E. B. Tylor on Races of Mankind, as classified by Language, Civilisation, and History; Mr. A. Thomson, the new Professor of Human Anatomy, on the Elements of Physical Anthropology as bearing on Classification of Races; Mr. H. Balfour on Progress in the Arts of Mankind, particularly as illustrated by the Pitt-Rivers collection. Professor Green lectures and gives practical instruction in Geology, Professor Vines in Botany, and Mr. M. S. Pembrey in Physiology and Histology, while Mr. Barclay Thompson lectures, as usual, on the skeleton of the Sauropsida.

THE Natural History Laboratories of the State University of Iowa started a Bulletin about two years ago, which often contains papers of interest. No. 4 of vol. ii., which has just come to hand, includes "Observations on the development of the *hypophysis cerebri* and *processus infundibuli* in the domestic cat," by F. S. Aby, some papers on Coleoptera by H. F. Wickham, an interesting account of a botanical expedition to Nicaragua by B. Shimek, and studies of Myxomycetes by T. H. McBride. We have been greatly struck by the excellent applications of the half-tone process, sometimes known as "Meisenbach," to scientific illustration, and the plates, especially to the botanical papers, show what this process is capable of when the drawings are properly prepared, and the blocks carefully printed.

TOYNBEE HALL continues to do good educational work in Natural Science, as well as in the numerous other subjects with which its classes deal. The University Extension courses in connection with this centre are:—"Recent discoveries with the Telescope and Spectroscope," by Dr. A. H. Fison; "The Geology of the British Islands," by F. W. Rudler; and "The Senses and Nerves," by E. A. Parkyn. Classes are also held, in Botany by G. May, in Biology by Miss Mitchell, and in Geology by Miss Raisin, while Miss Hall takes a Sunday class on "Forms of Vegetable Life." The Toynbee Natural History Society announces the following papers:—On February 5, "African Experiences," by Dr. J. W. Gregory; on March 5, "Water Fleas of Wanstead Park," by D. Scourfield; and April 2, "Formation of Crystals in Rocks," by A. M. Davies.

A VALUABLE herbarium has been presented to the Nottingham Natural History Museum by Mr. H. Fisher, late of Newark. Some idea of the nature and

extent of the collection may be gathered from the following enumeration of the more important series included in it: (1) A complete herbarium of British plants, comprising about 2,000 species and varieties, and about 10,000 specimens. (2) A European collection, comprising many thousand species from France, Germany, Switzerland, Austria, Roumania, Russia, Norway, Sweden, &c. (3) Several thousand species from North America. (4) A very fine collection from the Bombay Presidency. (5) About 1,500 species from Natal, the Transvaal, and other plants of South Africa. (6) A small collection from Australia. Of the above collections that from Russia is of quite exceptional value and interest. It comprises species from all parts of the Russian Empire—from St. Petersburg, Lapland, and the Crimea, through Siberia to Kamskatka and Turkestan, also from the Trans-Caucasus and the Caspian region. The Spanish collection is an extremely fine and valuable one—probably one of the best in existence. In order to hand over the collection to the town in as complete and accessible a form as possible, Mr. Fisher is himself arranging and labelling the collection, a work which will take many weeks of continuous application to complete.

THE Royal Irish Academy has issued its *Proceedings* for 1893 (ser. 3, vol. iii., no. 1). Among other papers, it contains a list of the Hepaticæ of the Hill of Howth, by D. McArdle, and two papers by J. E. Duerden; the first on some new and rare Irish Polyzoa, and the second on the Hydroida collected by the Royal Irish Academy Survey of the south-west coast of Ireland, 1885, 1886, and 1888, both of which will be of importance to others besides British marine zoologists. Dr. V. Ball has an interesting article on the volcanoes and hot springs of India, to which we allude in another place.

WE have received the first and second parts of vol. iii. of the *Actes de la Société Scientifique du Chili* (Santiago, 1893). Among other papers in this number are Lataste's "Rhythme vaginale des Mammifères," Germain's "Coleoptères du Chili," and Grez's "Los jeroglificos de la Piedra de la Batalla," and "La Piedra del Olimpo," an interesting account of the picture-writings of the aborigines of Chili, with plates of the inscriptions. The concluding portions of Borne's paper on the spider *Latrodectus*, will shortly appear as parts 4 and 5 of vol. ii., and will complete that volume.

THE Royal Academy of Denmark have published *Oversigt over der K. Danske Videnskab. Selsk. Forhandlinger*, 1893, no. 2, which contains an interesting biological study of the leaf of the South American Vellosiacea, by E. Warming, and a systematic description of the larval forms of the water-insect *Acilius*, by F. Meinert. J. Lange continues his contributions to the Flora of Spain.

THE Wisconsin Academy have just issued vol. ix., part 1, of their *Transactions*. As a contribution to our knowledge of lake faunas, the paper by C. D. Marsh on the Cyclopidæ and Calamidæ of Central Wisconsin will be of importance. The other Natural History papers are mostly lists of local faunas and floras.

AT the fourteenth annual meeting of the Biological Society of Washington, the officers for 1894 were elected, as follows:—President, Professor C. V. Riley, U.S. Entomologist; Vice-Presidents, Dr. F. Baker, Superintendent of the National Zoological Park, Mr. B. E. Fernow, Chief of the Forestry Bureau, Mr. R. Rathbun, of the U.S. Fish Commission, and Mr. C. D. Walcott, of the Geological Survey; Recording Secretary, Mr. F. V. Coville, of the Department of Agriculture; Corresponding Secretary, Mr. F. A. Lucas, of the National Museum; Treasurer, Mr. F. H. Knowlton, of the same institution. The Biological Society is one of the oldest of the scientific societies of Washington, and devotes its meetings to the discussion of original scientific facts rather than to the popular exposition of biology.

The following awards have been made by the Council of the Geological Society of London, to be presented at the annual meeting on February 16:—The Wollaston Medal to Professor Dr. K. A. von Zittel; the Murchison Medal to Mr. W. T. Aveline; the Lyell Medal to Professor John Milne; the Wollaston, Murchison, and Lyell Funds respectively to Messrs. A. Strahan, G. Barrow, and W. Hill; and a portion of the Barlow-Jameson Fund to Mr. Charles Davison.

The Committee of the Natural History Society of Northumberland has organised a series of six popular lectures on Natural History, now being delivered on alternate Saturday evenings in the Newcastle Museum. This month Dr. G. S. Brady discourses on Natural History in Norway, and Professor M. C. Potter gives an illustrated lecture on Tropical Vegetation in Ceylon. Subsequent lectures relate to forest trees, the ancestry of the horse, and the history of photography. Mr. A. H. Dickinson has succeeded the late Mr. Dinning as hon. secretary.

The Palæontographical Society have issued their volume of Monographs for 1893. It comprises instalments of Fossil Sponges, by Dr. G. J. Hinde; Ammonites, by Mr. S. S. Buckman; Cretaceous Star-fishes, by Mr. W. Percy Sladen; and the Devonian Fauna of S.E. England, by Rev. G. F. Whidborne.

At the moment of going to press we learn of the election of Mr. Henry O. Forbes to the Curatorship of the Liverpool Museum, in succession to the late Mr. T. J. Moore.

CORRESPONDENCE.

Olive-Brown Seaweeds.

In the January number of Natural Science, Miss Barton has set forth exhaustively, if in somewhat severe language, the present state of our knowledge of the development of the cryptostomata and conceptacles of the Phaeophyceæ. I do not wish to dispute any single fact she has mentioned, or to question her presentment of the whole body of them, but since the matter is one of considerable interest, a little further discussion of the bearing of the facts and of her suggested interpretations may be profitable. At first sight, the discussion might appear to wear the aspect of a contention for the shell of a nut, while the kernel is despised—that the contents (when there are any) of these bodies might serve us better for discussion, but I do not find that this view, in effect, has been neglected, though, perhaps, she has presumed the reader to understand more than he commonly does. It might be objected also that this is, to some extent in any case, an affair of growing-points which hardly afford stable characters otherwise in the groups in question. Here, again, one might easily be misled, as there is no diversity of the kind unaccompanied by other characters. A third objection might be raised that, in some of the cases cited, the hairs of the cryptostomata may possibly be discharging the function of absorption, for example, and I may anticipate such an objection by saying that whatever their present function may be, it has little or nothing to do with what the original one may have been, if these are indeed "ancestral structures," as Miss Barton possibly considers them. So far none of these views seriously touch the matter, and I bring them forward to dispose of them merely as a supplement to her article.

I have no doubt merited Miss Barton's criticisms of my contempt for the ancestors of the Fucaceæ, since she shows that some observations of my own might be exhibited as having a genealogical interest in this direction. I think I have spoken of them elsewhere with respect. No one can, indeed, dispute the fact of seaweeds, like other beings, having had ancestors, but my faith has not given me the eye to see these interesting organisms gilded with the romantic attributes that Miss Barton has ascribed to them. Cold facts do not carry us far enough. It may be profitable to ask ourselves the questions with which she concludes her article, but we may also ask them of Nature. We shall certainly get no answer until we do. That may be, and no doubt is, a sententious remark, but without striking too decadent a note, one may at least give warning of the danger of applying such broad principles to an insufficient array of facts. In Miss Barton's case it is, no doubt, a symptom merely of the enthusiasm which will enable her to add to the facts she has already accumulated not the least by her own investigations.

George Murray.

Scientific Volapük.

In your useful article on "Scientific Volapuk," which should, by the way, be "Volapük" (World-speech), you quote Professor Hyatt's terms mostly from *Zoologischer Anzeiger* for August, 1893. You seem to have overlooked the original paper "Bioplastology and the related branches of biologic research" (*Proc. Boston Soc. Nat. Hist.*, vol. xxvi., pp. 59-125), of which the publication was begun in August and completed in September, 1893. Since this paper contains a few terms that you

have not mentioned, I respond to your request for additions by sending them herewith.

Autotemnon (αὐτός, self; τέμνω, I divide), a single free Protozoon, so called in allusion to the common mode of multiplication, and in distinction to *zoön* which is the unit of the Metazoa.

Epembryonic (ἐπί, after; ἔμβρυον, embryo), a term characterising all stages of ontogeny after the embryonic.

Mnemegenesis was less correctly Mnemogenesis in the original paper.

Tachygenesis (ταχύς, quick; γένεσις, formation or development), the phenomenon previously known as "acceleration of development."

Tachygenetic, adjectival form of preceding, applied to normal types in which acceleration of development occurs.

It may also be noted that *Kinetogeny* of Ryder is merely a variant of *Kinetogenesis* of Cope, with the same meaning and corrupt etymology, only more so. Some people are never satisfied.

You are doubtless correct in criticising *Cotype*; for *typus*, though used by the Romans just as *type* is by us, was always regarded by them as a Greek word and they would certainly have used the prefix *syn-*. At the same time Oldfield Thomas is not altogether to blame, since he gives the credit (or discredit) for the word to C. O. Waterhouse, and merely retains for himself the credit of defining it more exactly.

Although one of those who have before now fallen under the lash of your criticism in respect of word-making, permit me to say that with this your last article I fully agree, and especially with the last paragraph of it which relates to the popularising of science. After reading the article that preceded it, I dreamed all night that I was a sorus of plurilocular sporangia and Laminarian paraphyses rolled up in a Fucaceous conceptacle to protect me from the attacks of oogonia, antheridia and filamentous cryptostomata.

F. A. BATHER.

FONA OR FUDDLEITE.—A NEW MINERAL.

A GOOD-NATURED friend has just drawn my attention to the note under this heading which appears in your last issue.

Mrs. French-Sheldon was kind enough to send me a copy of her interesting work, and, like your contributor, I was somewhat amused at, and for a moment exercised to understand, the extraordinary collocation of symbols which is there made to do duty for the formula of *Trona*, which, in my letter to the fair authoress, I had represented—or perhaps I had better say, attempted to represent—as

$$2\,Na_2O\ 3\,CO_2\ 4\,H_2O.$$

The matter, I think, is hardly worth the space it occupies in a periodical of the dignity and importance of NATURAL SCIENCE; but, in justice to the lady, may I suggest to your contributor, whose acumen would seem to lag behind his sense of humour, that he might exercise, and possibly even improve, his critical *flair* by ascertaining, *currente calamo*, how this expression could be calligraphically transformed into something akin to that which has so greatly puzzled and disturbed him.

T. E. THORPE.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, 5 JOHN STREET, BEDFORD ROW, LONDON, W.C.

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NOTES AND COMMENTS.

Haeckel.

"It gives furiously to think," as the *Saturday Review* delights to put it, that Ernst Haeckel was born only sixty years ago; for those of us whose scientific education dates within the last two decades have been accustomed to regard him as a necessary pillar of the world of thought. Morphology, promorphology, the categories of individuality, the gastræa theory, the comparisons of vertebrate embryos, the natural history of creation, and the blundering incapacity of those who doubted it, were no small part of the intellectual pabulum of our scientific youth. We learned from him that the animal kingdom was a set of pieces to be arranged in orderly procession from Protista to Man; that to study the embryology of an animal was to unwrap an Indian puzzle-box, piece within piece, and each piece suggesting the animal next behind in the procession of all the animals. We learned from him to invent logically hypothetical ancestors, and to find living animals to tally with them. It was all so cogent and so stimulating, and now, with fuller knowledge, so easy to jeer at and discredit. No one goes now to Naples to discover in the development of an animal exactly what he knows should be there: even a new discovery of the ancestor of all the vertebrates is somewhat blown upon. We elaborate methods, work out details, and mutter sadly:—

> "Embryologists we,
> But we haven't got everything down in the sea."

It is a history common to all great ideas. They explain so much at first, open so many new vistas, lead to advances so great that they seem master-keys to all the secrets of nature. And then a time comes when the potent explanation becomes impotent; a time when one set of men seeing only how much it has explained, distort things it cannot explain into formal harmony, while another set, seeing only what it cannot explain, attempt to discredit it altogether. The cell theory and the recapitulation theory are now in this condition, and Haeckel, partly because of the intimate connection of so much of his

work with the recapitulation theory, is, perhaps, less of a moving force in Biology now than ever he has been.

None the less, when the history of the Biological Renaissance comes to be written, the naturalist of Jena will be among the three or four most conspicuous figures. Since Johannes Müller at Berlin, no teacher has turned out so brilliant a set of pupils, has founded so energetic, so militant, so flexible a school. It is astonishing, but only at first sight, that his school should have been so flexible and the paths of his pupils so divergent. For those who have worked in his laboratory and talked with him under the lindens at his fascinating



ERNST HAECKEL, from a photograph by F. Haack, Jena.

combinations of Seminar and Kneipe, know that Haeckel is a man not of one idea, but of ideas. However one may admire patient empirical work, it is not patient empirical work, but patient labour pointed by preconceptions, made alive by ideas, that is stimulating and aggressive, moving towards new branches of investigation. He is a dull man whom all speak well of, and it is dull work that evokes no antagonism. For the present, we are well content to cease from controversy, to let Calcareous sponges, the Gastrula, the "Plankton," and Haeckelismus take care of themselves, and to join hands in celebrating the festival of Haeckel. Hoch! Hoch! Hoch!

Von Zittel.

Another eminent German, just now receiving well-merited honours at the hands of both his and our own countrymen, is Karl Alfred von Zittel, the first palæontologist of the day. In another part of this number (p. 222) we review the great "Handbook of Palæontology," which he has just brought to a conclusion. Here we give a few facts of his personal history, which we hope may prove of interest.

Von Zittel's father was a Protestant minister and head man of the Liberal Party at Bahlingen in Baden. Here Alfred was born on September 25, 1839, and here, amid the volcanic mountains of the Kaiserstuhlgebirge, he passed the first ten years of his life. Then the father was transferred to Heidelberg, where he sent his son to the Lyceum and afterwards to the University. The young student came into very intimate relations with the Professors of Geology and Palæontology, Cesar von Leonhard and H. G. Bronn, and in 1860 took his degree as Doctor of Philosophy. The following year he spent in France and particularly in Paris, where he made the acquaintance of Elie de Beaumont, the Eudes-Deslongchamps, father and son, and many others then or afterwards leaders in the development of geology and the allied sciences. In 1862, Zittel joined as a volunteer the Austrian Geological Survey at Vienna, and during 1863 was employed as an assistant in the royal "Hof-Mineralien-Cabinet." In the latter year he became Professor of Mineralogy and Geology at the Polytechnic in Carlsruhe (Baden), whence, in 1866, he removed to Munich as Professor of Palæontology in the University. In this post he has since remained, adding to it since 1880 the duties of Professor of Geology. Professor von Zittel has travelled much in the interests of science, his most notable expedition being that to the Libyan Desert in 1873-4. In his Professorial capacity, as editor of *Paleontographica*, as editor and part author of the *Handbuch der Palæontologie*, and by his numerous original publications, he has probably done more than any living man to advance his favourite science. He is a member of the Royal Bavarian Academy of Science, Director of the Palæontological Museum at Munich, and Chevalier of the Order of the Crown. He served as Vice-President of the International Congress of Geologists at its Washington meeting, in 1889 he was elected a Foreign Member of the Geological Society of London, and at its Anniversary Meeting this year the same Society awarded him the Wollaston Medal, the highest honour in their power. Lastly, he has been appointed a Privy Councillor of the German Emperor.

In 1863 von Zittel married a daughter of Schirmer, the well-known Düsseldorf painter, then Director of the Academy of Arts at Carlsruhe. An excellent musician, and he is as charming and accomplished in society as he is profound in his own special studies. He speaks our language readily, and has more than once visited England, where he has a friend in everyone that has the honour of his acquaintance.

DARWIN.

MEANWHILE the bidding to "praise famous men" is not being forgotten by Englishmen. The inhabitants of Shrewsbury, in which town Charles Darwin's father practised as a physician and where he himself was born, have, in a numerously-attended public meeting, determined to establish some memorial of the great naturalist. At present nothing definite has been decided, as it was felt that the memorial should not be merely local but national and indeed world-wide. A committee has therefore been appointed to make arrangements for a public meeting of scientific men and others interested in the object, which meeting it is hoped to hold early in March. Anyone who wishes to attend the meeting should communicate with Mr. H. C. Clarke, Town Clerk's Office, Shrewsbury. The suggestions already made are mainly three:—First, to place a bronze statue of Darwin in front of the old school buildings, which are now used for a public library and museum; no more appropriate site could be imagined. Second, to found some scholarship in connection with Shrewsbury School; if the intention be to encourage science, we would suggest that the scholarship should be given to boys on leaving rather than on entering the school. Third, to erect and endow in Shrewsbury a hall for technical and scientific teaching. All schemes are excellent, but the last seems a little ambitious; a combination of the two former seems most likely to meet with general approval. However this may be, we wish all success to the good people of Shrewsbury, believing that such public and visible honour to great workers in science will stir future youth to follow their example.

HARVARD MUSEUM AND SCIENTIFIC CO-OPERATION.

MENTION of "the Pope of Jena," as Haeckel has been bitingly nicknamed by his eminent opponent in "plankton" controversies, Alexander Agassiz, reminds us that we have received the annual report of the latter as Curator of the Museum of Comparative Zoology at Harvard College. Unlike most museum reports, these publications of Professor Agassiz always prove very interesting reading, and this seems to us to arise from the fact that this Museum is not merely a place for the storing of specimens, valuable though its collections are, but is a growing and living institution actively connected with the acquisition of fresh knowledge in all branches of biological and geological research, and with the imparting of that knowledge to eager classes of university students as well as to the larger public. In these respects a museum attached to a university, with properly appointed laboratories and a staff of broad-minded investigators, must always have some advantage over an establishment that is a museum and nothing more. In museums of the latter kind there is an inevitable tendency to ignore the philosophical aspects of the natural history sciences and to exalt the purely

systematic sides of them; and thus the curators of such a museum become finicking specialists, keener to find one spot more on a leopard's coat or one scale less on a butterfly's wing than they are to discover and exemplify fundamental features of organic structure. That this deficiency is due to no causes necessarily attached to museum work is clearly shown by the instructive and beautiful cases placed in the entrance hall of the Natural History Museum at South Kensington, some of which it is now intended to imitate in the Harvard Museum. On the contrary, we believe that the deficiency, where it occurs, is caused by the severance of the museum from the active workers, by the consequent absence of laboratories with proper facilities, and by the financial difficulties in the way of keeping up a complete library of other than merely systematic literature. The only way to overcome these difficulties, which we know to be in force in all parts of the world, is to bring about a more intelligent co-operation of the various government and private establishments with kindred aims, and to resolutely sweep aside the petty jealousies and red-tape restrictions that have no other effect than to hinder true progress.

In this very report before us is a case in point. "It is to be hoped," writes Professor Agassiz, "that some arrangement may yet be made between the representatives of the leading Universities and the Fish Commissioner by which the exceptional facilities for marine research now existing at the Fish Commission Station at Wood's Holl, may be made available for original investigation." The Fish Commission itself cannot spend money on questions with no direct practical bearing on fishery; at the same time it has at Wood's Holl an expensive plant, too extensive for its own needs. If, however, the government bureau and the leading universities could only be induced to co-operate, it would be possible to raise a fund large enough to relieve the Commission of its purely scientific work, and to afford sufficient means for its publication.

Birds' Eggs at the British Museum.

While alluding to the introductory educational series at the Natural History Museum, we should not omit to notice the admirably instructive exhibit of birds' eggs, with which Sir William Flower has recently enriched the central hall. After a description of the structure of an egg, illustrated by a diagrammatic section, the characters of number, form, size, texture and colour, are successively elucidated by specimens and labels. The number of eggs laid in one nest, and sat upon together, though tolerably uniform in each species, varies greatly in different species. Thus, the Manx Shearwater has but one egg, the Swift and Ring-dove two, while clutches of the long-tailed Tit and the red-legged Partridge are exhibited with nine and twelve eggs respectively. In form, eggs may vary from almost spherical, as in

the Scops Owl, to different modifications of elliptical or oval. The latter form, in which one end is smaller and more pointed than the other, though not universal, is the most frequent, and distinguishes the eggs of birds from those of reptiles. If there are many eggs in the nest it is obvious that the conical form makes close packing more easy. Where only two eggs are laid they are seldom conical. Eggs having a pyriform shape, or narrowing very rapidly towards the smaller end are mostly those of wading birds (*Limicolæ*) which lay four in a nest, and are large in proportion to the size of the bird, as exemplified by four Lapwing's eggs. Their pointed ends being turned inwards, they occupy as little space as possible, and are thus more easily covered by the brooding parent. A conical egg placed on the ground or a ledge of rock is less liable to roll away from its place if disturbed, than one of a spherical form. The size of the egg has generally, but by no means constantly, some relation to that of the parent bird. It also depends very much on the degree of development of the young bird at the time of hatching. When the hatched young are immature and helpless the eggs are small relatively to the size of the parent. Such birds usually build carefully-constructed nests. But when the young are well clothed with down and can run and feed themselves, the eggs are large, and laid on the ground or in imperfect nests. The Curlew and Raven are instances of birds of equal size with differently-sized eggs. The Cuckoo, which lays eggs in the nests of birds smaller than itself, has a relatively smaller egg than any other bird. The surface of the shell varies much in texture in the eggs of different birds. Among other examples are the smooth polished egg of the Tinamou, the smooth and glossy egg of the Kingfisher, the dull and chalky eggs of the Ibis and Duck, the eggs of the Ani and Flamingo with a distinct calcareous film, and the rough, pitted egg of the Emeu. The colour of the egg has no relation to that of the parent bird. White is probably the primitive colour of birds' eggs, as it is that of the eggs of all reptiles. The eggs of birds laid in holes, either in the earth or trees, entirely concealed from the light, are mostly white. The larger number of eggs are variously coloured by the deposition of pigment on or near the outer surface of the shell. The colour may be uniform throughout the surface of the shell, or it may be in irregular washes, blotches, more or less circular spots or lines, upon either a white or uniformly coloured ground. The signification of the various modes of colouration is very little understood at present. It often happens that the different species of a natural group of birds present a general similarity in the style of colouration of their eggs, or in other words that nearly-allied birds have similar eggs, a fact exemplified by sets of Warblers' and Buntings' eggs; but the exceptions to this rule are very numerous as shown by eggs of different species of Thrush. In certain cases there is evidently an adaptation of the colour of the eggs to their natural surroundings for the purpose of concealment. Excel-

lent examples of this may be seen in the groups illustrating the nesting habits of British birds in the Bird Gallery. Though the greater number of species of birds lay eggs all of which are of tolerably uniform character, varying only within narrow limits, there are some cases in which the eggs of different individuals of one species present a remarkable diversity, as shown by the eggs of the Guillemot (*Lomvia troile*).

This beautiful exhibit is rather hidden in a dark corner; but we hope that the rapacious schoolboy will ferret it out, and learn that there is something more to be done with eggs than merely to rob the nests of diminishing species. All who see it must acknowledge what beautiful results can be attained in museum work by any curator who spends little money, but much time and brains.

Deficiencies in Museums.

That is one side of the picture; but there is another, not so pleasing. At a recent meeting of the Geological Society of London an eminent palæontologist read a most interesting paper on the wonderful series of bones lately obtained by Mr. Lewis Abbott from a fissure-deposit, apparently of Pleistocene age. He had, however, occasion to lament that the value of his paper was considerably marred by the great difficulty he had met with in obtaining skeletons of many very common species for the purpose of comparison, and by the consequent uncertainty of some of his specific determinations. Some of his hearers were probably astonished to learn how large a number of our common British birds were unrepresented by a single bone in either our great National Collection or in that of the College of Surgeons. Among the species so conspicuous by their absence, the speaker mentioned the Swallow, the Song-thrush, the hairy- and feather-legged Buzzard, not to mention many species of Ducks and Gulls. Other of his hearers may have been filled with sorrow rather than surprise, remembering that this is not the first time such a complaint has been made. The Catalogue of Fossil Birds, officially published by the Trustees of the British Museum itself, remarks on the absence of skeletons of the existing species of *Anas*, and publishes a list of the fossil species which is, for that reason, avowedly imperfect and probably incorrect. Three years ago the author of this Catalogue said, at a meeting of the Zoological Society, whose President is, our readers will remember, the Director of the Natural History Branch of the British Museum: "With regard to the question of extinct species, the unfortunate imperfection of our English collections of recent avian skeletons renders it in some cases impossible to determine definitely the species to which the specimens belong."

It is all very well to attract the public with superb specimens of the taxidermist's art; it is most important that the collections should

be enriched by the acquisition of type-specimens, of rarities and of bird skins from all parts of the world; we do not even grudge the spending of thousands of pounds on illustrated catalogues of these same skins: but surely a Museum that can afford to spend many pounds on a single specimen of a white rhinoceros, might set aside a few shillings for the purchase of skeletons of our common British birds. It is all very well to say that money must rather be spent on rare specimens such as are not to be had every day, that the common things will wait and that people can see them elsewhere if they take the trouble. It should be remembered that for one person who wants to study the rare thing, there are a dozen who want to study the common thing, that these things, common enough now, are rapidly becoming rarities themselves, and that the complaints here quoted are clear evidence of a difficulty actually experienced, not by amateurs, not by private students, not by tyros ignorant of the proper places to go to, but by experienced scientific men themselves in the employ of the State.

It is not for us to enquire who is right or who is wrong; we would merely point out that scientific investigations of the deepest interest, and of importance, even to museums, are constantly hampered by these lamentable deficiencies which might so easily be remedied. We commend the subject to that energetic body the Museums' Association, and especially to its President, Sir William Flower.

THE MODERN MUSEUM.

WE are pleased to learn from its report, that the Association just mentioned is in a very satisfactory state of life and growth. The number of museums officially connected with the Association has risen from 35 to 40, while the number of unofficial associates has risen from 28 to 63. This sudden increase was due partly to the last meeting having been held in London, and is therefore not likely to be permanent. It is more worthy of note that many colonial museums and curators are now to be found on the list. As yet there is only one foreigner, Dr. A. B. Meyer of Dresden; but we see no reason why this Association should not become in time the recognised centre of museum workers all over the world. Local branches and committees would of course be necessary; but if there were a single publication and a supreme council to assist in questions of exchange, the employment of specialists, the distribution of labels and the like, we cannot doubt that great advantage would accrue, not only to museums, but to the arts and sciences illustrated by them.

The report opens with the very interesting address delivered by the President, on Modern Museums. This is chiefly devoted to an examination of the changes rendered necessary in the construction and planning of museums by the altered ideas as to the functions of a museum that have come so much to the front of late years. These functions are two, as expressed by Dr. J. E. Gray at the British Association in 1864,—"first, the diffusion of instruction and rational

amusement among the mass of the people, and, secondly, to afford the scientific student every possible means of examining and studying the specimens of which the museum consists." Sir W. Flower maintains that most local museums should confine themselves to the former function, and that only in national museums can the fulfilment of both functions in fairly equal proportions be expected. In connection with what we have said above on the deficiencies of museums, the following passage is of interest.—"As the actual comparison of specimen with specimen is the basis of zoological and botanical research, and as work done with imperfect materials is necessarily imperfect in itself, it is far the wisest policy to concentrate in a few great central institutions, . . . all the collections which are required for the prosecution of original research," especially collections "containing author's types . . . which must be appealed to through all time to settle vexed questions of nomenclature."

The address concludes with a plan suitable for any museum that proposes to follow up what is here called "the new museum idea." The chief features in this plan are the separation of the reserve collections from those intended for public exhibition, while retaining them in convenient proximity, and the separation of the officers' studies from the galleries in such a manner that they can stay at their work after the museum has been closed to the public. In a footnote, dated November 2, 1893, it is stated that a plan which "closely resembles" this "in essential features" has been found among the papers of the late Sir Richard Owen. It also has the advantage that the recent and extinct forms of each group, though not absolutely incorporated into one series, are brought into nearer relationship to each other than they are at present in the British Museum. We shall never cease to deplore that this plan of Owen's was not adopted, and we learn with interest that it is to be made public in the life of the great Anatomist, already announced by Mr. John Murray.

The House Beautiful.

But perhaps the most interesting paper contained in this report of the Museums' Association is that in which Mr. William White, the curator of the Ruskin Museum at Sheffield, gives an account of Mr. Ruskin's views on the function of museums; and the paper is interesting, not because the ideas in it are new, but because they were, for the most part, put forward by Mr. Ruskin so many as thirty years ago, and yet are a long way in advance of what has since been attempted in the larger number of museums. Indeed, so lofty is the height from which Mr. Ruskin looks at these matters, that we doubt whether even remotest ages will ever see some of his ideas put into practice. So long, for instance, as brains and body require proper nourishment, so long will the students and officials at

a great public museum demand that it shall be furnished with a "cooking apparatus for carnivora."

When Mr. Ruskin requires beauty in a museum we are wholly with him. What can be better than this:—"The first function of a museum is to give example of perfect order and perfect elegance, in the true sense of that test word, to the disorderly and rude populace. Everything in its own place, everything looking its best because it is *there*; nothing crowded, nothing unnecessary, nothing puzzling." Or again, "Above all, let all things for popular use be *beautifully* exhibited." Such a sentence should be borne as phylactery on the apron of every curator. But when Mr. Ruskin restricts beauty to "creatures in their perfectness"; when he denies "anatomical illustrations to the general public"; when he writes such pure Ruskinese as this: "Neither must you ever show bones, or guts, or any other charnel-house stuff. Teach your children to know the lark's note from the nightingale's; the length of their larynxes is their own business, and God's"; then we do think that this honey-tongued prophet of beauty proves himself no true seer. The beauty of an animal or a plant is no more confined to its external form and colouring than are the so-called diagnostic characters which distinguish it as a species apart. A sea-urchin "in its perfectness" is a beautiful object, so is the single richly-ornamented spine of such an animal; but this beauty is entirely the expression of a complicated internal structure, which, when viewed under the microscope, reveals a further beauty that should compel admiration even from Mr. Ruskin. Take him on his own ground. He goes to the British Museum (in 1880) to see a duck's wing, and does not find exactly what he wants. Away, then, with the shells and the monkey skeletons; he must have a gallery full of birds in flight! "What on earth, or in air, is the use to me of seeing their boiled sternums?" Had Mr. Ruskin's flights ever extended beyond his rhetoric, he would probably have found a sternum quite as useful as a wing. Next he will be urging artists to draw ships in full sail with never a spar to desecrate their canvas; the length of their masts is their own business, and the shipwright's.

Apart from this feminine horror of "nasty skeletons," the paper contains much that is valuably suggestive, and it is to be regretted that no discussion took place on it. It is interesting to learn that Mr. Ruskin does not approve of the Sunday opening of museums, that while he would abjure all idea of financial profit, he would charge a small sum for admission in order to keep out "the utterly squalid and ill-bred portion of the people," and that, in his opinion, names of the donors of objects "should cease to encumber either the cases or the scientific guides to them." On points such as these the opinions of professional museum curators would have been worth recording.

In these matters it should be remembered that Mr. Ruskin is no mere visionary schemer. He has given evidence before Parlia-

mentary Commissions on Galleries and Public Institutions, besides his museum at Sheffield he has arranged collections in various parts of the country, and the beautiful book reviewed in another part of this number reminds us how large a share he took in the establishment of the Oxford Museum. This latter was indeed an earnest-hearted attempt to produce a building, which, though devoted to Science, should be a true temple of the Muses, worthy in all respects to be called a House Beautiful. The attempt failed, many will say; failed before a concerted opposition and an unworthy economy. And yet we think that they who have on a summer evening walked in the meadows of the Cherwell, and have seen the pointed outline of the Museum dark against a sunset sky, have then beheld, among all the beautiful sights of Oxford, one that is second to none in either grandeur or beauty.

Winchester College.

One word more on Museums. In this number we are pleased to present our readers with the second article of our promised series on School Museums, in which that of Eton College is described by its present Curator. We hope it will not be long before we deal with the Museums of other of our great schools. But we fear that we shall have to wait long for any account of the Museum of Winchester College. In December last we alluded to the admirable scheme by which it was proposed to commemorate the quingentenary of the School. We regret now to learn that the proposal is not meeting with the warm acceptance that the patriotism of Wykehamists led us to expect. Up to the end of last year only 230 persons had subscribed. It is indeed to their credit that they contributed £3,367 between them, and since then a few more people have promised some £700. This, however, will not go very far, and one cannot help wondering what the many thousand old Wykehamists, who must be in existence, are thinking about. We cannot suppose a want of *esprit de corps* in so united a body; rather we infer a want of sympathy with modern educational needs, similar to that which accounts for the deficiency we have next to notice.

Absence of Experimental Psychology in England.

Some time ago, in reference to a preposterous magazine started by Mr. Stead, sensation-monger-in-chief to the British Philistine, we directed the attention of all interested in problems of mind and consciousness to the scientific organs for the discussion of these problems. In our Reviews for the month is noticed the first number of a worthy addition to these organs,—*The Psychological Review*, edited by J. McKeen Cattell, of Columbia College; and J. Mark Baldwin, of Princeton University, with the co-operation of many well-known

psychologists. We welcome this new magazine heartily, and congratulate its editors on their excellent beginning. But an interesting if somewhat dolorous problem suggests itself here. A large part of modern psychology concerns itself with laboratory work of the kind known as experimental physiological psychology, the application of the exact methods of science to the problems of memory, sensation and so forth. Although this branch of science is comparatively recent, there are well-equipped laboratories for it all over the continent, and the greater colleges and universities of America are no whit beyond. We know of none such in England. It is true that valuable work has been done by Francis Galton, and others, and we may expect more from the new Professor at Owens College. There was a rumour that Professor Burdon Sanderson hoped to establish a new department in connection with the Physiological Laboratory at Oxford, where already a good deal of psychological work has been done under his auspices. But it remains a rumour, and indeed we cannot see how Dr. Sanderson, with a growing medical school and an increasing number of students and graduates engaged on pure physiology, can spare either time or space. England has the men; cannot it find the money? No doubt it is an excellent thing for the race that the distribution of the endowments of Oxford and Cambridge should promote early marriage among those who have distinguished themselves in the classical schools. But there are more excellent and directer methods of dealing with endowments intended to promote learning. One of these, as NATURAL SCIENCE has taken repeated occasion to say, is the assignment of fellowships for branches of learning and research that are of themselves valuable, but open no direct avenues to lucrative employment. So much for Oxford and Cambridge. For other seats of learning devoid of so many pious benefactors in the past, we must hope for practical benefactors in the present.

THEORIES AND FACTS IN BIOLOGY.

LAST month, under the heading "Mathematical Biology," we referred to several interesting studies on variation as it actually occurs. Professor Weldon's insistence on the necessity for accurate information about variation might well be extended to various other chains of facts that form the bases of theories of evolution. Books, papers, and controversies on evolution abound, and in all of them variation, influence of environment, heredity, effects of crossing are the tricks to be made as the cards are dealt and shuffled, shuffled and redealt. The giraffe, fancy pigeons, the tails of lizards, and neuter insects invariably are the court cards, and it would not be difficult to assign equally well-known examples to the plain cards. It is an excellent game, and, as each player holds a full pack and may begin betting when he chooses; the playing is as complicated as the rules are lax. But the giraffe is always the trump card. Lamarck first played it for use-inheritance,

assigning its long neck to the inherited effects of reaching up to high branches; Darwin played it for Natural Selection; Herbert Spencer attempted to take one trick with its neck, another with its legs; Nägeli used it as a trump against selection; Weismann for selection; Professor Henry B. Orr, of Louisiana, U.S.A., in a book noticed among our Reviews, plays it for a system of his own. "The giraffe," he says, "seems to present the most remarkable illustration of the lengthening of the bones as the result of the frequent repetition of shocks. As is well-known, this animal feeds on the foliage of trees. From the earliest youth of the species and the earliest youth of the individual, it must have been stretching upwards for food, and, as is the custom of such quadrupeds, it must have constantly raised itself off its fore feet, and as it dropped must have received a shock that made itself felt from the hoofs through the legs and vertical neck to the head. In the hind legs the shock would not be felt." We are less sorry now that the giraffe is becoming extinct.

But, seriously, this fashion of dealing with the problems of evolution is the most degenerate formalism. No doubt the ingenious schoolman of the middle ages who introduced the burning question of the number of angels who could dance on the point of a needle, introduced it seriously to bring a metaphysical argument to an exact issue. But the example was taken up and argued through centuries till memory of the original question has been lost in laughter at the folly of early logicians. Let us make a concerted effort; let the giraffe be a name not to be named by the votaries of science. Else the laughter of our descendants will be at those word-spinners of the nineteenth century who, having seen no giraffe (save perchance in a menagerie), indulged in copious controversy as to whether or no it distorted its neck and legs by bumping and stretching.

A Whale-Fight in the South Atlantic.

Mr. R. Lydekker, who has recently returned from his journey to Argentina, favours us with the account of an interesting incident of his outward voyage. It is to be hoped that some of our readers, who may have witnessed a similar scene, will be able to throw light on points obscure to our correspondent, who writes as follows:—

"It was on the afternoon of September 9th in last year, about half-past four, that I was standing, in company with a lady fellow-passenger, on the poop of the Royal Mail Company's s.s. "Magdalena," watching the taking of soundings, the vessel being then off the coast of Minas Geraes, Brazil, at no great distance from the islands of Los Abrolhos, in long. 39° W., lat. 18° S. The soundings had just been completed when the attention of my companion and myself was attracted by the appearance of a whale and some other creatures at a distance of apparently something less than a quarter-of-a-mile from the ship. The whale was a 'finner,' of no very great size, and was

seen spouting, and again descending. Immediately after its first descent there appeared above the surface of the sea what seemed to be the tail-fin of some animal unknown. This supposed fin was raised in a vertical position, where it remained vibrating for some seconds and then suddenly disappeared. In colour it was a pure glistening white; while in form it appeared to be laterally compressed, with sharp edges and an acute termination. It gave the impression of belonging to some animal which was engaged in attacking the whale beneath the surface; and I should estimate its height above the water approximately at five or six feet. Soon after the disappearance of this strange white object,—which, both to my companion and myself, seemed most weird-like,—the broad black head of what I presume to have been a killer-whale was seen above the water; and in a few seconds the whale itself again rose to spout.

"The speed that the vessel was going soon left the whole of the group behind; and on going forward I was informed by the commander of the ship (Captain Rigaud), who had been on the bridge during the scene, that one of the animals, which he described as being black, had leapt clean out of the water.

"That these black animals, which appeared to be harassing and attacking the whale, were killers, I have no reasonable doubt; but the question arises as to the nature of the animal to which the strange white tail-like object seen standing above the water could have belonged. My impression at the time was that it must be the upper lobe of the tail of some enormous shark allied to the threshers (*Alopecias*); and this impression has been confirmed by a subsequent examination of the stuffed specimens of that genus in the British Museum. The thresher is, however, a black shark; while, as pointed out by Dr. Günther, the minute size of its teeth seems to discredit the common accounts of its attacking whales. Still, however, I cannot imagine to what animal the aforesaid white tail could have belonged, unless to some gigantic shark allied to the thresher, but of a white colour, and probably armed with much larger teeth. If so, we have evidence of a fish at present unknown to science; and even if I am wrong in this conjecture, the animal to which the object in question belonged would still appear to be unknown, and, in this event, quite unlike any with which we are now acquainted.

"The whole scene, weird and impressive as it was both to my companion and myself, suggested how easily a story of the sea-serpent might have originated therefrom. The black head and body of the presumed killer would have done duty for the head and body of the serpent; while the glistening white object standing perpendicularly above the water would have served for the tail of the same."

ARMOURED WHALES.

THE foregoing narrative shows that we have yet much to learn about even the existing monsters of the deep. Meanwhile, our know-

ledge of the whales of old time is rapidly increasing. Palæontologists have recently been exercised to some extent regarding the affinities of those remarkable gigantic Eocene mammals known as Zeuglodonts, the remains of which have now been discovered in so many parts of the world. Till a short time ago, these animals were pretty generally admitted to be members of the Cetacean order, in which they were regarded as constituting a separate subordinal group —the *Archæoceti*. In a memoir published in the "Studies from the Dundee Museum" for 1890, Professor D'Arcy Thompson took, however, a decided exception to this view, maintaining that the Zeuglodonts had nothing whatever to do with whales, but that their affinities lay rather with the seals. Two years later, Mr. Lydekker, when describing in the *Proc. Zool. Soc.*, some Zeuglodont remains from the Caucasus, again discussed the question, and came to the conclusions that there was no justification for the proposed innovation, and that the Cetacean nature of the group in question was undoubted.

In a recent communication (*Palæontologische Abhandlungen*, ser. 2, vol. i., pt. 5, 1893), Professor Dames has "gone one better" than this, concluding that not only should the Zeuglodonts be classed among the Cetacea, but that there are no grounds for separating them as a distinct suborder from the Odontoceti. He accordingly proposes the following scheme of classification for the latter order :—

1. ARCHÆOCETI ;—teeth few and differentiated. (Zeuglodontidæ.)
2. MESOCETI ;—teeth numerous and differentiated. (Squalodontidæ.)
3. EUODONTOCETI ;—teeth numerous and uniform. { Platanistidæ. / Delphinidæ. / Physeteridæ. }

In the resemblance presented by their skulls to ordinary terrestrial mammals, coupled with the small number of their teeth, the Zeuglodonts are sharply defined from the Squalodonts, and thus form the most primitive of all Cetaceans. The Squalodonts appear, however, on the whole, to occupy an intermediate position between the Zeuglodonts and Delphinoids, and thus serve to connect the three groups of toothed whales.

Not the least noteworthy portion of the Professor's memoir is the one in which he describes certain bony plates found in association with Zeuglodont remains, which are believed to indicate the presence of a dorsal bony dermal armour. That such an armour may have existed in primitive Cetaceans, is rendered the more probable by Dr. Kukenthal's recent discovery of the vestiges of such a structure in the skin of the existing *Phocæna* (*Neomeris*) *phocænoides*.

Possibly the ancient Cetaceans were more exposed to such attacks as the one above described by Mr. Lydekker, and, for this reason, required greater protection. It is well known that the early forms of many groups of animals were provided with some such armour, or evolved some equally adequate means of defence. As

instances, we may mention many of the Edentates of South America, the Carboniferous Camerate Crinoids, the fish of the Old Red Sandstone, the Cephalopods with external shells, and the Operculate Corals of the Silurian. The dermal armour of these ancestral Cetaceans may have been a secondarily-acquired character, from which no particular argument as to their relationships can be drawn.

ÆPYORNIS.

KNOWLEDGE of yet another group of extinct monsters is rapidly progressing. Recent researches in Madagascar, of some of which we gave an account in NATURAL SCIENCE, vol. iii., p. 192, have brought to light many remains of the *Æpyornis*, a bird which some suppose to have suggested the Roc of Sindbad the Sailor. Some of these remains have found their way to the British Museum and have been described by Mr. Charles W. Andrews (*Geol. Mag.*, Jan., 1894). Others have reached Paris and fallen into the hands of Messrs. Milne-Edwards and Grandidier, who, apparently stimulated by the paper of Mr. Andrews, have published an all too brief account in *Comptes Rendus* for January 15. Names are here given to no less than four new species, but, though some measurements are published, we have sought in vain for any adequate diagnosis. Moreover, a new genus, *Müllerornis*, is proposed for three new species of more slender form than those referred to *Æpyornis*, but of this "genus" no further definition is attempted. It also appears that no particular bone is taken as the type-specimen, but that names are applied to collections of limb-bones which may or may not belong to the same species. If this kind of thing continues the confusion of nomenclature in the Æpyornithidæ will soon put into the shade that already obtaining in the Dinornithidæ. Our only remedy is to refuse recognition to all such undescribed "species." There is, however, much of interest in this paper, as it gives, for the first time, a description, though a short one, of the skulls, sternum, and coraco-scapula. The skull is said to be less depressed, longer and narrower than that of *Dinornis*; the temporal fossæ are deep and narrow, while the basi-sphenoid bears strong basi-pterygoid processes as in other Ratite birds. The mandible is straight and strong, with a long compressed symphysis, hollowed like a spoon. The sternum much resembles that of *Apteryx*, being a thin, broad, flat plate, with grooves for the coracoids like those of that bird. The coraco-scapula is much reduced and bears a slight depression for the articulation of a rudimentary humerus. It is to be hoped that we shall not have to wait so long as usual for figures and full descriptions of these important specimens. Meanwhile, it is more clear than ever that, if *Æpyornis* really is the Roc, then it was Sindbad's imagination and not the bird that flew away with him.

I.

The Fossil Plants of Canada as Tests of Climate and Age.[1]

IMPORTANT questions respecting the value of fossil plants as tests of climate and geological age have arisen of late and have been discussed in scientific periodicals and in special works, especially in Seward's "Fossil Plants as Tests of Climate," and Lesquereux's "Final Report on the Plants of the Dakota Group." A consideration of what we have already learned on these subjects from the fossil flora of Canada may therefore prove of service.

In the first instance, it will be necessary to glance, by way of contrast, at the condition of the vegetable kingdom in the Palæozoic period. In this part of the earth's history the problem is complicated by the peculiar character of many of the plants, as well as by the probability that the meteorological conditions were very dissimilar from those now prevailing. We may say in general terms that a flora of tree-ferns, giant lycopods and pines is akin to that of modern oceanic islands in warm climates. This is true; but the Devonian and Carboniferous plants did not grow exclusively on oceanic islands, but on continental areas of considerable magnitude. They flourished also in all latitudes, from the polar region to the equator, and though there are some generic differences in the plants of the period in the Southern Hemisphere, yet these do not seriously affect the general facies. There are characteristic Lepidodendroids, for example, in the Carboniferous of Brazil and South Africa and Australia, and though in the latter region there are certain ferns allied to those of Mesozoic Europe, this is merely a local difference, not materially affecting climate, and corresponding with the fact that the European Mesozoic flora originated in the south. Nor does the doctrine of homotaxis seriously affect the question. Each geological period was sufficiently long to permit plants to migrate to every station they

[1 This article is extracted from the advanced proof of a paper "On the Cretaceous Plants of Vancouver Island," which will appear in the *Transactions of the Royal Society of Canada*, and will end, for the present, the valuable series of papers on the Mesozoic and Cainozoic plants of Canada, with which Sir William Dawson has since 1882 enriched that publication. The present portion, which we have the kind permission of Sir William to reprint, gives the general conclusions based on the whole series.—Ed.]

could occupy, and in every case the temporary and local climate must be indicated by the local flora, while the succession in any one place may be relied on as holding good over a very extensive area.

Looking at the Palæozoic plants a little more in detail, we remember that coniferous and taxine trees grow now in very different latitudes and climates. There is, therefore, nothing so very remarkable in their occurrence. The great group of *Cordaites* may have been equally hardy, but it is noteworthy that its geographical distribution is more limited. In Europe, for example, the genus is much more characteristic in France than in Britain, perhaps in connection with the warmer climate to the southward. Ferns and lycopods and mare's-tails are also cosmopolitan; but the larger species now belong to the warmer climates, and nowhere in the present day do they become so woody and so complex in structure as in the older geological periods.

The natural inference would be that in the old coal period the geographical and other conditions must have conspired to give a somewhat uniform and moist climate over a great part of the earth's surface. The geographical arrangements, so far as known, indicate this, and the distribution of animals points in the same direction. In America, for example, the great eastern and western ranges of mountains were yet in embryo, and a large part of the continent was occupied with shallow water or with swampy plains scarcely above the sea-level. The batrachians and insects of the land and the varied forms of animal life in the sea alike point to a climate at least mild and uniform. We must also take into account the probability that there was a larger amount of carbon-dioxide in the atmosphere than at present, which would have greatly impeded radiation from the ground, while the moisture exhaled from the vast swamps and morasses of the period would have produced a similar effect.

It would, however, be a mistake to suppose that there were not local differences of climate. I have in my "Acadian Geology" advocated the theory that the great ridge of conglomerate on the northern border of the coal-field of Pictou, in Nova Scotia, may have been an ice-formed ridge on the margin of the deep morass in which the thirty-six feet seam was deposited. In this case a sea occasionally ice-laden may have approached within a short distance of forests of Sigillariæ and Lepidodendra, and this in the middle of the coal period. On the whole, therefore, we should postulate for the Palæozoic flora not so much a high temperature as uniformity and moisture. This seems also to accord with the prevalent character of the foliage and the structures of the remarkable acrogenous trees of this period.

As to the early Mesozoic flora, the indications are that it was an invader from the Southern Hemisphere, for which the intervening Permian period had prepared the way by destroying the preceding Palæozoic forests. This was probably effected through the agency of

great earth-movements changing the geographical and climatal conditions. But as the Mesozoic ages advanced, the old conditions to some extent returned, and enabled the cycads, pines, and ferns of this age to push their way almost to the Arctic regions. Being, however, derivatives from warmer climates, their vitality and powers of variation were probably not great. They flourished luxuriantly and became considerable coal-producers, and their reign was probably of long duration, extending through the Triassic and Jurassic periods and into the Lower Cretaceous. In the north they met with a new and far more advanced and varied flora, originating there, and destined, in the Middle and Upper Cretaceous, to replace them throughout the whole Northern Hemisphere. This new and most important change was undoubtedly accompanied with climatal amelioration, giving a mean temperature of probably 55° Fahr. to regions within the Arctic Circle; and this, as we shall see, probably depended on geographical arrangements introducing the warm waters of the equatorial current into a vast land-locked basin in the interior of the American Continent, with corresponding though probably not so simple or easily-understood arrangements in the Eastern Continent as well.

Thus, when we ascend from the base of the Cretaceous, we find a remarkable and apparently sudden influx of angiospermous exogens of modern generic types. The aspect of suddenness is given not merely by the rapidity with which from a very few forms the new flora expands in richness and variety, but also by the simultaneous appearance, in the case of many genera, such as *Sassafras*, *Liriodendron*, *Magnolia*, *Quercus*, etc., of as many species in the Middle Cretaceous as the modern world can yet boast, and in some cases of a greater number. On the other hand, it is true that other genera, as *Populus*, *Betula*, etc., appear at first in fewer forms, and are more largely represented in the modern world. This difference is apparently not connected so much with the botanical rank of the several genera as with their degree of adaptation to the more equable climate of the earlier Cretaceous or to the more extreme climates that have succeeded. This climatal change has not only required the removal of some genera to southern habitats and the diminution of their species, but has required that they should be replaced in the north by new species of more hardy types. The southward movement of the whole flora in later Cretaceous and Tertiary times, and the introduction from behind of the modern species, is thus apparently connected with the gradual refrigeration which culminated in the Glacial period.

In Western Canada, in the Rocky Mountains, and in the Queen Charlotte Islands, as well as farther north in Alaska and Greenland, we have a Lower Cretaceous flora characterised by forms approaching those of the Jurassic. One of its characteristic species is a *Dioon*, allied to *D. edule* of Mexico, *D. Columbianus* and *D. borealis*, Dn. Along with this are species of *Zamites* and *Podozamites*, and primitive

species of *Salisburia*, similar to those described by Heer from the Jurassic of Siberia. The lower beds of this (the Kootanie series) are not known to contain any angiosperms, but in beds a little higher (Intermediate and Mill Creek beds) these begin to occur. Newberry has found the same flora farther south in Montana, and it corresponds, in part at least, with the Potomac flora of Fontaine, which occurs over a wide area in the South-Eastern States of the American Union. The geographical distribution of this flora shows an extension of warm climate up to the territory of Alaska. With other geological facts, it also shows that the habitat of the Lower Cretaceous flora was around the margin of a continent not yet elevated into lofty mountain chains, and including to the south a mediterranean sea of warm water; while the conditions in the extreme north must have excluded anything representing the present snow-clad mass of Greenland. This alone is, in my judgment, sufficient to account for the climate of the period, whose warmth extended even to Greenland. I have noticed the nature and correlation of this flora in papers published in the *Transactions of the Royal Society of Canada* (1885, part iv., p. 1, and 1892, part iv., p. 79).

This is succeeded in the north of Canada, at least as far to the south as the latitude of 55°, by the Dunvegan series, holding a warm, temperate flora, containing species of *Magnolia*, *Laurus*, *Ficus*, and *Quercus*, along with such temperate forms as *Fagus* and *Betula*. It also contains Cycads and Sequoias (*Trans. Roy. Soc. Canada*, 1882, part iv., p. 20). It appears to be in the main parallel with the marine Niobrara farther south. This flora is probably continued along the Rocky Mountain region by that of the Mill Creek series, which, however, has more the aspect of that of the Dakota period. (*Op. cit.*, 1885, part iv., p. 11.) It is less rich in cycads and conifers, and has species of *Platanus*, *Macclintockia*, *Cinnamomum*, *Laurus*, *Magnolia*, and *Aralia*. The aggregate of the Dunvegan and Mill Creek series may thus be regarded as Middle Cretaceous or Cenomanian and Senonian, and corresponding in the main to the Atané of Greenland. It belongs to the northern and western sides of the same great Cretaceous mediterranean sea on whose shores the previous Kootanie flora had flourished.

East of the Rocky Mountains this is succeeded by prevalent marine conditions, with the local interposition of the Belly River series, which contains beds of coal, but so far a meagre flora, including *Sequoia Reichenbachii*, *Brasenia antiqua*, *Trapa borealis*, and species of *Acer* and *Populus*.

Passing across the mountains to the Pacific coast, we meet with the abundant and interesting flora of the Cretaceous Coal-measures of Vancouver Island, a truly Upper Cretaceous assemblage. It evinces a still warmer climate than those previously noted, or than the succeeding Laramie; but it is not improbable that already some difference existed in this respect between the Pacific coast and the

interior region of the Continent. It probably coincides with the Patoot flora of Greenland.

Later than this, and in its floral character assimilated rather to the Eocene of other countries, we have the Laramie series proper, indicating a period in which the great interior plateau east of the Rocky Mountains had ceased to be an open sea, and had been reduced to the condition of swamps and lakes, the former holding a rich flora of temperate aspect, even as far north as Alaska and Greenland. The Laramie flora has been recognised locally on the west coast as well, but its greatest areas are in the interior plains, where it undoubtedly overlies the Fox Hill or Danien beds. It is, perhaps, most remarkable for its richness in coniferous trees, *Taxites*, *Sequoia*, *Thuia*, etc., and for the great development of the genus *Platanus*, as well as for its containing some ferns of modern species (*Onoclea sensibilis*, *Davallia tenuifolia*). For details of this flora the reader is referred to *Trans. Roy. Soc. Canada*, 1885, part iv., p. 16; 1886, part iv., p. 19; 1887, part iv., p. 31; 1889, part iv., p. 69.

The Miocene Tertiary is represented on the Canadian plains only by the gravels of the Cypress Hills, holding mammalian bones referred to the White River series; but on the Similkameen River and elsewhere in the interior of British Columbia there are beds holding an interesting insect fauna and a number of fossil plants. Among the latter are several swamp and aquatic species, *Equisetum*, *Azollophyllum*, etc., and conifers of the genera *Pinus*, *Taxodium*, *Glyptostrobus*, and *Salisburia*, along with species of *Myrica*, *Populus*, *Salix*, *Alnites*, *Acerites*, *Carpinus*, *Nelumbium*, etc. The climate evidenced by these plants is still temperate, but probably scarcely, if at all, warmer than that of the coast of British Columbia at present. (*Op. cit.*, 1890, part iv., p. 76.)

It would thus appear that, while we have no evidence of a tropical climate in Northern Canada in the Cretaceous or Cainozoic periods, the successive floras point to equable and warm temperate conditions extending very far northward, and gradually passing in time into those of the colder Miocene and Pliocene. We can also to some extent correlate these climatal conditions with the geographical features of the several periods and with the contemporary animal remains.

I may add that the validity of such deductions does not altogether depend on the accuracy of the reference of particular species to existing genera. In many cases there can be no doubt of this, as in the species of *Liriodendron*, *Sassafras*, *Platanus*, *Sequoia*, and *Salisburia*, and especially in the case of all those forms of which seed or fruit has been procured; but even where the naming may be inaccurate or where the number of species has been unduly multiplied, the deductions as to climate may hold good, though not perhaps to the extent of enabling us to fix a definite thermometrical mean temperature.

As to geological age, the primary requisite is that in some of

the localities of fossil plants their position shall be fixed by stratigraphical evidence. This being done in a few cases, it is not difficult to assign to their approximate position intermediate or allied sub-floras. In Canada, though the collections of fossil plants have not been so large as would be desirable, we are fortunate in having the horizons of the leading floras accurately fixed by the officers of the Geological Survey, and the plants collected carefully referred to the beds to which they belong.

Thus, though the geographical conditions of the Mesozoic and Cainozoic are not of such a character as to enable us to refer sub-floras to their definite geological position throughout the whole Northern Hemisphere in the manner in which this can be done for the plants of the Lower, Middle, and Upper Carboniferous, nevertheless, a satisfactory approximation can be made, and I have no hesitation in affirming that it is possible to define with considerable accuracy the age of any collection of fossil plants from any part of our Cretaceous or Cainozoic districts.

Plants as evidence of geological age have the advantage of wide distribution over the surface of the land, and of long duration in any one place, and slowness of migration when obliged or enabled to spread to new localities. They are also so closely connected with the great movements of subsidence and elevation which mark the lapse of geological time, that they are very certain indices of these, whether they affect plant life directly by elevation and submergence, or indirectly by changing the climatal conditions.

As in the case of animal fossils, we have to allow for differences of station, for possible driftage and intermixture of species belonging to uplands and low levels, and varieties dependent on chances of deposition and preservation. We have also to consider that plants are more permanent and less changeable than the animal inhabitants of the land, so that they may not mark such small portions of time or such minute changes as may be indicated, for example, by mammalian remains.

On the whole there is very good reason to believe that the labours of Palæobotanists have, in the United States and Canada, succeeded in securing for fossil-plants an important place as guides in the determination of geological age. The knowledge we have acquired needs to be collected and arranged in such a manner as to make it more available than it can be when scattered, as at present, through a great number of reports and memoirs.

J. W. DAWSON.

II.

Natural Science in Japan.

II.—PRESENT (*continued from p.* 111).

THE Imperial Museum takes to itself the various functions which with us are distributed between the two sections of the British Museum and the Museum of Science and Art at South Kensington. That is to say, its collections are representative of the Natural History, Antiquities, Art—both ancient and modern, pure and applied,—Resources and technical Industries of the Mikado's Empire. It has, it is true, no immediate connection with the education of the country, since this is amply provided for in other directions and is, for the most part, subordinate to the Imperial University. At the same time, the officers of the Museum have other duties than those of merely conserving and cataloguing the collections committed to their charge. Thus, from the Handbook of the Imperial Museum (*Teikoku Haku-butsu-kwan Yo-ran*) we learn that the officials are instructed to look after the precious objects preserved in temples throughout the country; but perhaps this only refers to those of the state church, which is Shinto, and not to the other purely Buddhist temples. They are also sent out to collect objects, both in Japan and in foreign countries; so that specimens thus obtained are not only cheaper but more authentic than those which pass through the hands of a dealer, while the collector is enabled to obtain that knowledge about the surroundings of a specimen without which the most important collections often lose half their value. It is also considered to be part of a curator's duties to assist students in their investigation of the collections under his care, permitting them to make copies, drawings or photographs of specimens; at the same time he is himself expected to make a scientific study of the specimens and to publish his results. If these instructions, as given in the handbook, are adequately fulfilled, the curators have it in their power to utilise the Museum and their connection with it to far greater advantage than that which can be attained by more tape-trammelled curators nearer home; and we cannot doubt that this will be for the benefit of Science, of the Nation, and of the Museum itself.

The Museum is situated in a prettily arranged garden in Ueno Park. The building, which is two stories high, is of plain semi-

mauresque architecture, with large windows, and appears well adapted for the purposes of exhibition. The offices of the staff are in a separate building at the back. The Natural History collections, with which we are chiefly concerned, are all on the ground floor. It is not of course to be expected that they should contain any specimens of exceptional interest, or be of any great size, since the collections of more scientific importance are at either the University or the Geological Survey office. A walk round the galleries is, however, by no means without interest to the visitor.

The Zoological specimens are arranged systematically and not geographically; still, as the bulk of the collection, at present, comes from Japan, one gets a very good idea of the local fauna. The labels are written in both Japanese and Roman characters, but beyond giving the name and locality they make no attempt to convey information; that is a development which will no doubt come in time. There are, however, a number of old French models of the digestive, respiratory, circulatory and nervous systems in various animals.

As will have been anticipated by those who remember the first part of this account, the arrangement of the Botanical gallery is far superior to that of the Zoological. First are given a series of specimens and models illustrating the different forms assumed by the various organs of a plant: the kinds of roots, kinds of stems, aërial and subterranean, the parts and arrangements of leaves. Then come models for the illustration of the natural classification of plants, and in the next case models to illustrate the Linnean classification. Models of flowers and fruits follow. A selected series of dried plants is exhibited: these are mounted in black frames, hung on screens, and each is accompanied by a coloured plate and an explanation of its position in the system. Thus one is presented with a synopsis of the vegetable kingdom that is both attractive, interesting and readily intelligible. Adjoining this admirable series are additional dried specimens, of local interest, in further illustration thereof. The genera of Japanese ferns are specially represented by a series of dried specimens, with coloured plates and explanations. There are also a collection of the insectivorous plants of Japan and some models of mushrooms. The catalogue of specimens exhibited in this gallery is by Mr. K. Saida, and if he is also responsible for its arrangement, I should like to offer him my congratulations on having interested one so ignorant of botany as I unfortunately am.

The Geological rooms contain nothing of particular interest, except, what seems a useful novelty, a case containing the instruments and reagents employed in mineralogical and petrographical research. The minerals are arranged in three distinct groups, the first illustrating their chemical composition according to the bases, the second according to the acids, and the third the crystalline form. There is a typical collection of rocks from different parts of the world, numbering about 500 examples. The fossils exhibited are mostly

European and largely supplemented by casts, but there are a few Quaternary mammalia of native origin.

Catalogues, printed in English and Japanese, are issued for these three departments, but they are nothing more than lists of names and localities. That to the geological collections is by Mr. M. Nishi, while the zoological catalogue is drawn up by Messrs. Ch. Ishikawa and T. Yuakawa.

The archæological collections of the Museum, though of considerable interest, must not now detain us; nor can one do more than mention the exhibition of native and foreign manufactures, designed to promote the industries of the country, or the various models of native and foreign engineering works. These, as well as the beautiful exhibits of pure and decorative art, are beyond our province. The curators of all departments seem to be doing their work with as much zeal as intelligence; but of course a foreigner is not skilled to judge of the merits of the Japanese labels. It was, by the way, curious that, when I visited the Museum, I could find no official with whom to converse to more than a very limited extent in any European language. In this respect the Museum officials contrasted with those at the University and the Geological Survey. I must not however omit to recognise the courtesy of the Director of the Museum, Mr. Kanaye Kubota, who gave me every information in his power.

We have now considered the chief government establishments connected with natural science in Japan. A few lines only need be devoted to the Learned Societies and minor institutions.

Of Societies the names are legion. As Professor Chamberlain says:—"The Japanese of our day have taken kindly to societies and associations of all sorts. They doubtless feel that their nation has to make up now for the long abstinence from such co-operative activity which was enforced during the Tokugawa *régime*, when it was penal for more than five persons to club together for any purpose." The Geographical Society, or Chigaku Kai, is a private society with about 30 members. The Anthropological Society numbers about 100, and publishes a bulletin which is in Japanese but contains illustrations showing that a vast amount of interesting matter is treated of in its pages. The office of the society is at No. 5, Roku-chome, Hongo, Tokio. The Botanical Society publishes in both Japanese and English. The Zoological, which has about 75 members, publishes in Japanese, but the diagnoses of new species are usually given in some language more intelligible to the world at large. Besides these, there are a Seismological, a Chemical, a Physico-Mathematical, a Geographical, a Philosophical, an Engineering and an Electrical Society, with others more or less connected with science, some of which also publish transactions. The members of these societies are few compared with those of societies with more practical objects. Thus, the Sanitary Society has a membership of over 6,000, the Educational Society over 4,000,

while the Society for the Promotion of Commerce and Industry and the Agricultural Society are equally influential.

In addition to the publications of the scientific societies there is the *To-yo-gak-ge-zasshi*, which literally means the East Ocean Science Art Journal. This was started about 1880, is published at Tokio, and the yearly subscription is one yen, at present about two shillings and sixpence in our money. It is conducted chiefly by University people, and serves to relate the excursions of the students and other details of local interest. It also contains articles by the Professors, and reports of the monthly lectures which they deliver to the general public.

In describing the scientific course at the University, allusion has already been made to the science teaching in the public schools. In the Primary Schools, science is taught by means of object-lessons and talks. In the Middle Schools, which include boys from 14 to 18 years of age, science is taught in all its branches, and such a school is required to have a set of chemical and physical apparatus. Each Middle School has two or three masters who divide between them kindred branches of science. Science is compulsory in these and also in the Higher Middle Schools; but in his second year's attendance at the latter the student confines himself more to his own special subjects. These schools have better laboratories and more science masters, who are thus enabled to devote themselves to more special branches. All science teachers are either graduates of the University or licensed after examination by the Educational Department.

There is of course a large amount of scientific instruction given at the Engineering College in Tokio and the Agricultural Colleges in Tokio and Sapporo, of which the latter was the first founded. These, however, need not detain us now. One may also occasionally come across something of scientific value in the local museums, as at Osaka and Gifu; but their chief intention is to assist commerce and to promote the industries of the country. On the whole, the scientific movement in Japan is concentrated at the capital, and must be studied in connection with those institutions that I have endeavoured to describe in the preceding pages.

III.—FUTURE.

Let not this heading lead anyone to suppose that I am about to assume the prophet's mantle. Zadkiel and the Reverend Samuel Baxter may rest unrivalled. All I intend is to consider the facts already mentioned, as well as certain others that have not yet come within our view, and to suggest from them a few inferences as to the probable development of natural science in Japan, and the relations of that science to the world at large.

There is a tendency to underestimate the solid and original elements of the Japanese character, and to regard the Japanese themselves as a nation of mimics. It is pointed out that the art, the religion, the literature and the government of Japan have, at different times, been transplanted from foreign countries; and there are those who say that these various elements of national life have not greatly improved in the transplantation. At the present day we see the Japanese somewhat too rashly casting aside their ancient traditions and embracing the customs, constitutions and even the clothes of Europe with shamelessly open arms. Science comes with the rest, and we have noted the considerable pitch to which its cultivation is now carried. But, it is asked, will all this endure? When the foreign teachers have been dismissed and when the Japanese endeavour to teach themselves and to advance by their own efforts, will they not sink back into a state of semi-barbarism, from which their present veneer of western civilisation will gradually be worn away? And, what is after all of more importance to Japan, will the nation be able unaided to stand its ground against the plundering hordes of Europe? These sneering doubts come, it must be admitted, with rather a bad grace from a people so indebted to others as we ourselves are. England of the sixteenth century surely owed to Greek philosophy, Roman sagacity, Hebrew religion and Moorish science as much as ever Japan could place to the credit of China and Corea. But, apart from this very obvious analogy, there is no reason why the foregoing questions should be answered unfavourably to Japan. The Japanese are neither parrots nor monkeys. Compare their decorative art with that of China! In all qualities, except perhaps that of technique, it is admittedly superior. As for the Japanese and Chinese theatres, any comparison would be simply absurd: the former is as far above the latter as a "Haymarket" comedy is above a mediæval miracle-play. The fact that there was for two and a half centuries a period of comparative stagnation proves nothing: Europe has had its periods of stagnation too. Now that the incentive has come, there is no reason why we should not behold in Japan a Renaissance with effects as far-reaching as those which were seen in Europe when the re-discovery of the ancient world was followed by the finding of the new.

Such a conclusion will hardly be assented to by another class of objectors. There are people who find in the Japanese nothing but food for mild amusement, and a subject for the cheaper sorts of witticism. As a rule these are the people who see small merit in anything outside England, and it would be waste of time to discuss their opinions. But when we find a writer so in love with his subject as Sir Edwin Arnold uttering sentiments that differ from those of the minor scribblers only in the delicacy with which they are expressed, we feel obliged to give these opinions more careful consideration. Sir Edwin says of the Japanese, "They have the nature rather of birds or butterflies than of

ordinary human beings. . . . They will not and cannot take life *au grand sérieux*." The exaggeration of these statements betrays the practised journalist. Did we not detect here the lurking desire for effect that vitiates professedly ephemeral writing, we should have to suppose that the ascription of a nature so superficial could be based only on superficial observation. Heaven forbid that light-heartedness and abundance of laughter should ever disqualify a people from paying a serious attention to the tasks of life, or make them any the less human beings! But in truth, if the Japanese do not take life so seriously as Sir Edwin Arnold thinks right, they at least take themselves seriously, perhaps a little too seriously, so that we older nations looking on at their solemn performances are sometimes reminded of children that play at being grown up. To compare the Japanese to children is, however, an injustice, and evinces a want of sympathetic insight. They have entered the lists with other nations, and they, if not the others, have the sense to see that the struggle is no child's play. The sincerity of their endeavours was shown only the other day by an instance not wholly foreign to our present purpose. At the Columbian Exhibition almost all the prizes for agricultural improvements were carried off by Canada, largely owing to the admirable system of instruction obtaining in that colony. No sooner did Japan hear of this than she appointed a commission to study on the spot the secret of the Canadian success. The precocity of this child should shame its critics.

No! Grant to the Japanese, by all means, the childlike qualities of sincerity, enthusiasm and imitative power; but recognise that these qualities are at all times admirable, and that in the present instance their value is not impaired by either levity or superficiality. On the contrary, as the description of only one small department of the interests of modern Japan should have shown, the open-eyed observer will note in this people an almost German thoroughness. So far as science is concerned, this may possibly have been, to some extent, derived from their German teachers; but it can hardly be denied that the quality is innate, and there can certainly be no doubt but that, left to themselves, the Japanese better the instruction.

Many, it is probable, will find in the Japanese somewhat too great an appreciation of practical ends, and will remind us gravely that man does not live by bread alone. In the present state of the nation's development the reproach is undoubtedly merited; but what nation that calls itself civilised does not merit it to an equal extent? To those, however, who have the smallest knowledge of Japanese history, to those acquainted (as who is not?) with the artistic skill, the traditional mysteries, the literary enthusiasm and the exquisite taste of old Japan, it will seem absurd to brand these children of the rising sun as devotees of a cold utilitarian creed. The truth is merely the trite one, that if the body be unsatisfied it avails not to satisfy the soul. Japan is struggling for her place

among the nations, for her life itself; that once secured, we have no reason to doubt that she will return to her old allegiance, though perhaps in a modified form. The conditions of modern civilisation do not make for such art as was the soul and the saving of old Japan; but there are other subjects for intellectual activity, and among these one of the noblest and one that may well commend itself more and more to the Japanese in the days that are to come, is the search after knowledge for its own sake.

We may remove from the Japanese the reproach of utilitarianism, but they must retain that of materialism. To a certain extent, however, and certainly so far as it forms a philosophic system, this opposes no serious bar to scientific progress. It is when a man's materialism makes him nothing more than matter-of-fact, when it deprives him of the faculty of imagination, it is then that it impedes his intellectual activity. This is the failing one is said to meet with among the Japanese; and if the accusation be true, those that understand the true value of the imagination in scientific work will see under what disadvantage their students labour. There are many causes for this defect, partly, as we shall see, the language, partly the long period of stagnation already referred to, but chiefly the influence of the Chinese. To know the cause is however, to foresee the cure, and we may well expect that, under the influence of European education, the weakness will gradually disappear.

Another characteristic of the Japanese, upon which everyone is agreed, is their extreme handiness and neatness. How valuable this quality is in scientific work, every scientific worker is well aware.

One should not, perhaps, pass unnoticed here the love of monstrosities to which so much attention was directed in the first part of the present article. This, however, is too obviously a character common to all under-educated persons and nations for us to regard it as in any way a peculiarity of the Japanese. P. T. Barnum did not make his fortune in Japan. Consequently we do not find that the intelligent Japanese zoologists have devoted any abnormal proportion of their time to the study of teratology. In fact the sole instance that comes to mind, if indeed it be an instance, is Watase's paper on the double-tailed gold-fish.

Passing from characteristics such as the preceding, which may be noted by any intelligent traveller, we come to a graver charge, which has been brought against the Japanese by those who have had greater facilities for observation, and longer time for continued study. By many such writers it has been maintained that the Japanese are wanting in philosophic instinct and the kindred intellectual faculties. Kaempfer, for instance, says in his *History of Japan*,—"Now if we proceed to consider the Japanese, with regard to sciences and the embellishments of our mind, Philosophy perhaps will be found wanting. The Japanese indeed are not so far enemies to this Science, as to banish the Country those who cultivate it, but they

think it an amusement proper for monasteries, where the monks leading an idle lazy life, have little else to trouble their heads about. However, this relates chiefly to the speculative part, for as to the moral part, they hold it in great esteem, as being of a higher and divine origin." In our own days, Mr. Walter Dening is said by Professor Chamberlain to write as follows,—"It is well known that one of the most marked characteristics of the Japanese mind is its lack of interest in metaphysical, psychological, and ethical controversy of all kinds. It is seldom you can get them to pay sufficient attention to such questions to admit of their understanding even their main outlines."

Comprehending now some of the tendencies and limitations of the Japanese character, we are better able to see what manner of work their scientific men are best fitted to do. It is clear that their genius leads them in the direction of accurate, detailed investigation; either minute dissections of animal and vegetable structure, careful observation of the habits of living beings, the appreciation of the slight differences so important in the problem of variation, precise chemical analyses and physical measurements, or the construction of elaborate apparatus for determining the slightest changes in surrounding conditions. In the exhaustive and microscopic questionings that the needs of modern science bid us put without cessation to ever-elusive nature, the new Japanese recruits will doubtless prove our most valuable allies.

Turning to the special branches of science with which we have here been dealing, we remember that the Japanese zoologists are privileged to live in a country where the fauna, both of land and sea, is remarkably abundant, while the extraordinary variety of the vegetation will ever render the self-same region a veritable Garden of Eden to the botanist. Both classes of workers may here exercise to the full the nomenclatorial propensities of the old Adam. Many of the native animals and plants have, it is true, been already described by European or American workers; thus Dunker has done the molluscs, Döderlein the sea-urchins, Pryer the butterflies, while besides the botanists previously named we may note Miquel, Maximowicz and Murray, specially instancing the beautiful work "Om Japans Laminariaceer" by Kjellman and Petersen. These and many other active workers have, however, by no means exhausted the treasures of Japan. The Museums already contain large collections of unnamed specimens, now being worked out by native students; while we may expect these collections to be considerably enriched when the northern part of the empire comes to be explored more thoroughly than has hitherto been possible. But it is not merely in systematic biology that we may still expect something new from Japan; this region has already yielded us forms of such far-reaching importance as the king-crab, the giant salamander and the glass-rope sponge, that we may continue to look eastwards for fresh

light to fall on the problems of morphology. As for the geologists, it needs no prophet's tongue to foretell that, for many years to come they will furnish the world with observations of novelty and importance in the branch of their science that deals with volcanoes and earthquakes. Petrology also may be indebted to them; but we hardly dare hope that the greatly altered rocks of Japan will yield the solution of many palæontological enigmas.

There is then a fairly wide field in which the acknowledged powers of the Japanese men of science may well be employed, and their foreign colleagues will await its cultivation with a justifiable hope. One department of science exists, however, in which, if we may place any reliance on the opinions quoted above, the Japanese are not likely to make any conspicuous progress. The world, we may conclude, will never hail a great Japanese philosopher, nor will its line of advance be changed by any new principles emanating from Japan. This very bold conclusion may, however, be rendered invalid by changes similar to those which it has already been suggested will do away with other supposed mental deficiencies of the Japanese. A few such possible changes may now be discussed.

In considering the relations of Japanese science to that of the rest of the world, one of the most patent difficulties, which might almost prevent any relation at all, lies in the language of the country. At the outset there are two obstacles to mutual comprehension: the first is the great difference between the Japanese and European languages in respect of form and construction, the second is the very different mode of writing the two languages. The former obstacle affects the Japanese themselves more than it does the Europeans. There is so much in all European languages that it is almost impossible to express in Japanese; and even when some sort of translation has been effected, the different habits of thought will still render the result largely unintelligible. Thus we may note the absence of all those personal modes of expression and of that constant reference of all surroundings to the speaker as a centre which are of such universal occurrence in our own speaking and writing. Similar to this is the absence of personification of all inanimate and abstract objects. To quote from Professor Chamberlain's fascinating *Handbook of Colloquial Japanese* (p. 272), "Not only does Japanese idiom eschew all such fanciful anthropomorphic expressions as 'the hand of Time' . . . 'Nature's abhorrence of a vacuum,' etc.; but it goes so far as almost to prohibit the use of the name of any inanimate thing as the subject of a transitive verb. For instance, a Japanese will not say 'The rain delayed me,' thus appearing to attribute an action to those inanimate things the drops of rain; but he will turn the phrase intransitively." And again (p. 273), "Thus no language lends itself less to the imaginative and mythopœic faculty than does the Japanese. When, for instance, a European speaks of 'the strife between Religion and Science,' he very likely

spells these names with a capital R and a capital S, and unconsciously slides into regarding them as being, in some sort, actual things, even individualities. . . . Such mythology (for mythology it is, albeit those who have been reared under the exclusive influence of European modes of expression may not recognise it as such) is utterly alien to the matter-of-fact Far-Eastern mind." There are also minor difficulties, such as the apparent want both of a passive voice and of a comparative, all which combine with the others to render the translation of European scientific works into Japanese a matter almost of impossibility. These difficulties are evaded rather than overcome by the necessity the Japanese find themselves under of learning almost all their science through the medium of some foreign language, usually English, and by the growing habit of introducing foreign phrases or at least foreign modes of expression into their own writings.

The second obstacle, that presented by the ideographic writing common to Japan and China, is one that chiefly affects the foreign readers of Japanese, although I am inclined to believe, from conversations with many native friends, that it also places a serious stumbling-block in the way of their own rising generation. It is quite clear that no foreigner, who does not intend to make the Japanese language his life-study, will ever attempt to learn two distinct forms of syllabary and some four thousand ideographs, even for the sake of reading the description of a new species of snail or the account of a novel method of cell-division. Unfortunately one can hardly doubt that the Japanese will not always write their scientific papers in English. Indeed at the present day there is much of value that makes its appearance in the native tongue; and as foreigners come to exert less and less influence in the education of the country the return to the Japanese language is likely to become more general. The prospect is somewhat appalling. Czech is a sufficient hindrance, and Russian daunts most workers: what shall we do when the floodgates of Japanese scientific literature are opened upon us? There is one hope left. The Japanese language does not in itself present insuperable difficulties, and if the Japanese would only consent to write it in the ordinary Roman alphabet, we should hardly have cause for grumbling. This solution of the problem has already presented itself to the Japanese, and in 1885 a society with this object, called the Romaji-Kai, was founded by Professor Toyama and others. Unfortunately the anti-European reaction, which set in in 1889, has made this society somewhat unpopular, and the Japanese students to whom I put the case had plenty of reasons to offer against the proposed change. This is not the place to discuss the question in full; but two facts seem to show that the old methods can hardly survive in the modern struggle for existence. One is that the sender of a telegram is obliged to use either Roman characters or the comparatively simple syllabary known as the Kata-kana. The other is that, even in inland towns such as

Kyoto and Gifu, many of the shopkeepers seem to find it an advantage to write their names up in Roman letters.

A greater hindrance to complete intercommunication of thought than that presented by language may by many be found in the great difference that we have already observed between our own and the Japanese modes of thought. This does not entirely consist in a different attitude of mind or a different temperament, but is to a large extent a result of antecedent conditions. It is hard for us to realise how largely all our Western religion, philosophy and science are the outcome of history, how intimately the present is bound up with the past. But in this past, the common heritage of all European nations, the Japanese have no portion. The allusions, the associations of ideas, the pregnant phrases, the postulated premises: all these inseparable factors of our thought have for the far East no shadow of a meaning. The inevitable to us is the impossible to them.

Of course as the world becomes more of a unit, and as the terms East and West come to have a merely relative significance, these obstacles will gradually disappear; meanwhile it is possible to suggest that they may after all be blessings in disguise. One cannot help seeing that, however much we Westerns gain by our imaginative and mythopœic powers, we are seriously handicapped by this irresistible tendency when we attempt to discuss things as they are. The gross anthropomorphism of all religions that we have at different ages affected finds its parallel in the realms of Science. The Japanese at least are not likely to deify Nature or to make such fetish of a phrase as we have done with "Natural Selection" and "Chemical Affinity." Again, the burden of the past is sometimes as great an incumbrance as ever delayed the progress of the pilgrim Christian. All men know how the introduction of the new scientific methods and the advance of the scientific spirit have perpetually had to struggle with the bonds of ancient belief and to fight to the death with the phantoms of superstition. The bitter feelings thus awakened, and the bigotry thus begotten on both sides, can hardly have advantaged truth or assisted the onward march of the world. Sadly and irresistibly have these events brought home to us the danger of putting new wine into old bottles. Here then is the opportunity of the Japanese, and it is one that they themselves fully recognise. Ignorant of the bonds and unpressed by the burden, they receive from us weapons forged in the fires of controversy and, at last after many a vain experiment, tempered to pierce the dense walls of matter which enshield the secret soul of truth.

F. A. Bather.

III.

Adaptation in Liverworts.

IT is well known among bryologists that the liverworts, unlike their relatives the mosses, are not stereotyped in form, but vary enormously, and in many different directions; they have in consequence been aptly called "one of Nature's experimental grounds." At the outset we have the two great groups of the foliose and thallose —or, according to a more recent and nearly corresponding division, the *acrogynous* and *anacrogynous* forms, with their strikingly different habits of structure and growth, though they are well connected by intermediate forms. Within these divisions, but especially in the acrogynous, there is still an extraordinary variety of form to be seen, and this group will be chiefly dealt with here.

The most important variations are connected with the collection and retention of moisture, as might be expected in plants presenting a small surface, and developing unicellular root-hairs only, which cannot penetrate the substratum to any considerable depth. Results of investigation in this department have been recently published, which show a remarkable power of complex adaptation in these lowly plants (1, 2).

The typical form of the acrogynous Jungermannieæ is a thin, usually more or less prostrate, stem, bearing three rows of leaves, two lateral and one ventral. Those of the ventral row (amphigastria) are usually very much smaller than the others, and are often absent altogether; in the latter case the ventral segment of the three-sided apical cell forms stem tissue only, while the lateral segments produce each a row of leaves. The leaves are commonly bidentate or bilobed, and in the simpler forms, as in the common *Lophocolea bidentata*, the lobes lie in the same plane, so that there is no provision for retaining moisture other than the capillary action among the leaves and stem. Such forms commonly possess well-developed root hairs, sometimes in enormous numbers; while in a few species, *e.g.*, *Lepidozia reptans*, there are whip-like branches bearing only rudimentary leaves, but densely clothed with root hairs, which are apparently modified for this purpose.

Coming now to those forms which possess some kind of water-collecting apparatus, we find, as Goebel has shown, four chief classes,

each with its own type of structure, whereby the same end is attained.

It must, however, be remembered that in the Muscineæ water is absorbed by the leaves, and in those which have a soft stem by the whole surface of the plant.

(1.) First, there are leaves which are simple in shape, but are cut up, and form a more or less spongiose mass. In *Ptilidium ciliare* and others the main part of the leaf is a cellular plate, but the border is divided into a large number of hair-like cell rows. In species of *Trichocolea*, however, there is scarcely any cellular plate at all, the leaf being cut down to its base, and resembling a densely-branched twig (Fig. 1). The effect of this is very well seen when the British species (*T. tomentella*) is gathered in damp weather, as it can then be wrung like a sponge.

The following types depend upon the alteration of the form of the leaf.

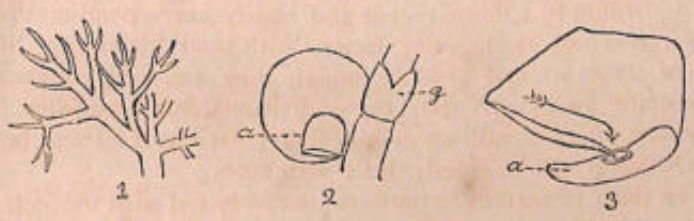


Fig. 1. Part of leaf of *Trichocolea tomentella*.
Fig. 2. *Frullania dilatata*; *a*. auricle or water sac—*g*. amphigastrium.
Fig. 3. Leaf of *Physiotium giganteum*; *a*. auricle.

(2.) In species of *Lejeunia*, *Radula*, etc., the lower lobe of the bilobed leaf does not lie in the plane of the upper lobe, but is turned forward, so that the lobes are apposed. The advantage here lies in the increased capillary surface, while in some species a simple sac is produced by the depression of the base of the leaf.

(3.) In the British species of *Frullania* a considerable advance upon the foregoing is found in the bending and hollowing out of the lower lobe, which forms a cup-shaped sac, with a wide mouth opening posteriorly (Fig. 2). In a foreign species (*F. cornigera*) there are two sacs to each leaf, and the mouth is narrower than the basal part. It is interesting here to note that such sacs are not confined to the foliose forms, since in *Metgeria saccata*, one of the thallose Jungermannieæ, projections are developed at the edge of the thallus, which bend round and form sacs comparable with those of *Frullania*.

(4.) The last type presents a highly-organised water-sac, or auricle, with its mouth towards the middle or base, and protected by a valve. This sac, which is comparable in structure and development with the bladders of *Utricularia*, is found in species of *Colura* and *Physiotium*. In *Physiotium giganteum* (Fig. 3), a plant inhabiting the dry mountain regions of India and Ceylon, the sac is subulate, and has a

circular mouth, bounded by a strong rim or collar, opening on the side next to the stem. Direct entrance is impossible, as the mouth is closed within by the depressed tissue of the circular area of the mouth, and entrance can only be effected by a passage downwards between the two flaps of the valve. One of these flaps is rigid, and exactly like the rest of the foliage in texture, but the other consists of thin-walled, empty, and dead cells; the latter forms the "clapper," and by its easy movement the opening and closing of the valve is effected. The shoots bend outward from the tree to which they are attached and grow vertically upward. The provision for the conduction of water into the auricle is remarkably perfect. "The upper lobe is well adapted, both by its shell-like form and its position on the upright stem, for catching and holding water, which, owing to its oblique position, will naturally flow toward the auricle. It is prevented from running off by the lower edge being strongly incurved, thus forming a channel in which the water may collect, and leading to the mouth of the auricle. The curved edge of the lobe is continued by a projection into the mouth of the auricle, the upper edge of which is raised to prevent the water escaping at the junction of the two members, while a depression below forms a channel which ensures the entrance of the water into the auricle" (2). How effective a water collector this plant is may be readily seen in the epiphytism it engenders, a variety of small plants and animals being commonly found in the sac, while a species of *Frullania* was in some cases so closely applied to the shoots as to be invisible except on close examination.

There is yet another means whereby water is collected and held, viz., the perianth or colesula. This is a more or less cylindrical sac, which develops around the archegonia, and after fertilisation elongates considerably, and encloses the developing sporogonium. As it stands vertically, and is in most cases much larger than is necessary to cover the young sporogonium, it will doubtless catch more or less water, which in those having a contracted mouth will be held with great tenacity. But in a number of species the colesulæ are mostly, and sometimes entirely, barren, the archegonia being in some cases entirely suppressed. These barren colesulæ are occasionally produced in immense numbers, as in *Physiotium acinosum*, where they form the main mass of the plant, the stem and leaves being small and comparatively insignificant; and that they function as water sacs is evident from the fact that water can be wrung from the fresh plant after weeks of rainless weather in a dry locality. There is, however, a disadvantage in this habit, as seeds are enabled to germinate in the vicinity of the liverwort, and the seedlings send their roots into the colesulæ and rob the plant. But the advantage to the "epiphyte" is very great, as the plantlets, which otherwise might not have germinated at all, have now become independent.

Another interesting point which has been recently brought to light is concerned in the moistening of the apex. The mucilaginous papillæ

in the thallose forms, developed immediately behind the apical cell, are well known. It is now known that similar papillæ are commonly formed at the apex of the very young leaf in the foliose forms, and hang over the apical cell. This uses up the apex of the leaf, and the main part of this organ is subsequently formed by intercalary growth. The papillæ usually occur singly or in a pair on each leaf; but in *Physiotium giganteum* there are three to each upper lobe, which hang over the stem apex, so that the leaf in this state presents an appearance irresistibly recalling the jester's cap and bells.

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JESSE REEVES.

IV.

Thermo-metamorphism in the South-Eastern Highlands of Scotland.

IN the north-east of Forfarshire, there is a tract of moorland, much covered with peat and heather, that forms part of the singularly flat table-land of the south-eastern Highlands. It is drained by the North and South Esk, and in the craggy sides of their valleys may be seen exposures of the gneisses and schists of which the area is chiefly composed. Glen Clova, to the south-west, is especially well-known for its picturesque scenery. A detailed study of this area shows that there are several masses of intrusive rock, gneiss, and granite, which are probably connected underground, and that the highly crystalline character of the surrounding schists is the result, mainly, of thermo-metamorphism, or of alteration by heat rather than by pressure and mechanical movements. This conclusion is of some interest, as the special features may, after all, have no necessary connection with great antiquity, but be due to the depth in the earth's crust at which the metamorphism took place. It serves also to strengthen Dr. Barrois' views that regional metamorphism and contact metamorphism are much the same thing, differing not in *kind*, but in *degree*.[1]

The ordinary condition of the intrusive rock is that of a slightly foliated granite, with two micas: muscovite or white mica, and biotite or brown mica. Hence the rock may be termed a Muscovite-biotite Gneiss. Intruded among the schists are a number of pegmatites, or veins of coarse granite, the origin of which was uncertain; but they have now been traced to the parent mass of granite rock—the muscovite-biotite gneiss.

In considering the effect of the forcing of a granite magma, under enormous pressure, into the surrounding rocks, we have practically to deal with a process akin to that by which silver is extracted from lead. Just as the silver crystals are strained off from the lead at a certain temperature, so certain crystals are strained off from the granite-magma during the process of forcing it through every crack

1 General results of paper "On an Intrusion of Muscovite-biotite Gneiss in the South-east Highlands of Scotland, and its accompanying Metamorphism." *Quart. Journ. Geol. Soc.*, vol. xlix., pp. 330-358, pls. xv., xvi., 1893.

and plane of weakness in the surrounding rocks. This results in leaving behind large masses of rock composed of what are known as the "earlier crystals of consolidation," while the residual liquid is forced upwards and outwards, finally consolidating as extremely coarsely crystalline veins and masses known as pegmatites, or "giant granite." A peculiarity of the present instance is the enormous distance to which these veins extend from the parent mass. Perhaps no case has before been clearly established in which pegmatites extend so far as a mile from the surface outcrop of the parent mass of any granite of post-Cambrian age; but in the area under consideration these veins extend as far as 40 miles from any exposure of the muscovite-biotite gneiss.

These pegmatites throw great light on the sequence of the rocks in this intensely-folded area. Obviously, if by a great normal fault, rocks which are penetrated by igneous veins are brought against rocks which these veins have failed to reach, the permeated rocks must be the lower of the two, and we are thus able to show that one group of rocks is higher or lower than another. A starting-point of this nature is invaluable in an area where it is difficult to say if any dip is true or reversed. It may be here noted that the characteristic feature of the pegmatites is the presence of large crystals of muscovite. The original magma consolidates normally as a true granite containing white and brown mica, but the pegmatites never contain large crystals of brown mica. This leads to the natural inference that the pegmatites bearing brown mica, such as may be seen at Cape Wrath, are the upward prolongations of a granitite magma, or one which contained brown mica only and not white mica, and which has been injected under precisely similar conditions to those of the south-eastern Highlands.

Most interesting is the development, over a vast area, of certain minerals, such as sillimanite, cyanite, and staurolite, which have already been described by many authors as the result of the intrusion of large igneous masses into sediments. The development of these minerals is found to be dependent on nearness to or distance from the main intrusion, and it may be further added that the crystalline condition of the entire region may be said to "rise or fall" away from the main intrusion, just as the pegmatites do or do not reach the surface. From these facts it may be inferred that the upward permeation of igneous (often gneissose) material must have taken place over a large part of the southern Highlands. In the ordinary post-Cambrian granites, the development of certain minerals occurs practically within cylindrical rings, one outside another, surrounding the inner cylinder of granite. Here, however, we have a new phenomenon to deal with. The thermo-metamorphism is dependent, not so much or so entirely on the distance away from the parent edge of the igneous mass, but rather upon the fact of the rocks being at a greater or less height above what we may

conveniently term the "upper saturation limit" of igneous material. There is no reason to believe that a similar saturation is not going on now under volcanic areas. Nevertheless, the vast depth required for the rocks to have a sufficiently high temperature for the extremely slow cooling of the giant granites, points to an equally vast time to allow for denudation to expose them at the surface. This at once suggests the great antiquity of areas in which the phenomena referred to are seen. It is, however, well within possibility that, in special areas where denudation has acted powerfully for a long period, this phase of intrusion may be exposed, though it took place in post-Cambrian times; but the proofs of it should be placed beyond reasonable doubt.

GEORGE BARROW.

V.

The Museums of Public Schools.[1]

II.—ETON COLLEGE.

THE present Eton College Museum, which forms part of the block of buildings known as the Queen's Schools, was completed only a few years ago. Before that day there existed, on the present site of the Queen's Schools, a large round building (the Rotunda) and a number of small class-rooms known as the Old Mathematical Schools. After the completion of additional class-rooms for the mathematical work of the school, the Rotunda was used for various purposes until the year 1875, when it was, at the expense of several of the Science Masters, fitted up as a Museum.

At this period a set of British birds, stuffed in separate cases, together with a series of British butterflies, formed the nucleus of the zoological collection, while a large number of mineral specimens and fossils, got together by gift and purchase, formed the nucleus of the geological collection.

About the year 1888 the Rotunda and the Old Mathematical Schools were pulled down, and the present Queen's Schools were erected on the site. The Queen's Schools consist of a block of buildings on three sides of a quadrangle, the centre of which is occupied by a grass plot. The whole of the northern side is occupied by the new Lower Chapel, built by the subscriptions of old Etonians. On the south side is a large lecture-room and a physical laboratory, a large drawing-school, and several class-rooms. The western side of the block is not built upon, and the eastern is almost entirely occupied by the Museum, with a line of class-rooms on the ground floor beneath.

The eastern and southern sides of the quadrangle, with the exception of the Physical Laboratory and Lecture Room, the funds for which were provided by the Head Master, Dr. Edmund Warre, were built by the College out of their endowment.

The architect of the whole block of buildings was Sir Arthur Blomfield.

The Museum consists of a large room (87 ft. by 30 ft.) and a small

[1] The first of this series, on Charterhouse, appeared in this Journal for July, 1893.

adjunct. The principal room is very well lighted by a double row of upright skylights, which extend along the whole length, and by seven large windows.

The window at the northern end of the room is filled with stained glass, and was formerly in the old School Library, where it was put up by old Etonians during the head-mastership of Dr. Hawtrey. It is an heraldic window, containing the arms of Eton and of various personages and institutions connected with the school.

The northern end of the Museum, which is separated to a certain extent from the rest of the room by arches and pillars, forms a library, in which there is an excellent collection of books on Natural History; it is further furnished with tables and chairs for the purposes of reading and study.



THE ETON COLLEGE MUSEUM.

The books comprise text-books, books on the British fauna, natural history, travels, etc., etc., and include a complete collection of the Palæontographical Society's works, and of the scientific reports of the voyage of the "Challenger."

In the three years during which the Museum has been opened its contents have increased rapidly; but before giving an account of what the collections now consist of, it will be perhaps useful to give a sketch of what a museum of this sort should contain; in fact, of an ideal School Museum.

There are two very distinct and separate classes of boys to whom a School Museum will appeal: these may be called the Professional and the Amateur. Of these, the former, though considerably the

smaller in numbers, will make much more use of the museum, while the Amateur class is much more numerous, especially in a Classical School like Eton.

The Professional class of boys are those who are taking up, as part of their school-work, some scientific subject, such as Biology or Geology. For the Biologists, a good typical series of well-named and well-labelled specimens, illustrating the structure and principal forms of the animal and vegetable kingdoms, is required. For the instruction of the Geologists, a good typical series of rocks, minerals, and fossils, arranged in stratigraphical order, is necessary. All the specimens can, of course, be removed when required to illustrate lectures on either subject.

For the Amateurs, by which name I designate those boys whose



INTERIOR OF THE ETON COLLEGE MUSEUM, LOOKING NORTH.

interest in Natural History has been aroused by making such collections as those of butterflies and birds' eggs, a different series of collections is required. For this class, what is mainly wanted is a set of specimens of the British fauna as complete as possible. Mammals, birds and their eggs, and insects are, of course, the groups in which the greatest amount of interest is shown, and it is especially to those groups that attention should be directed. Moreover, as many of the Eton boys come from very different parts of the British kingdom, it seems unwise to limit the faunal collections to the productions of the neighbourhood of Eton.

The local School Museum should, therefore, in my opinion, contain :—

1. Faunal collections limited to the British isles.
2. Small, well-chosen, typical collections to illustrate the forms of animal- and plant-life, together with typical collections of minerals and of fossils, stratigraphically arranged.

A brief account of the present contents of the Eton College Museum may now be given, to show how far it comes up to the ideal, and in what ways it is still deficient.

The north end of the Museum is occupied by three large mahogany table cases, containing a small but useful collection of Invertebrates. The dried specimens are arranged in the table-cases, the bottles containing preparations of the softer-bodied animals are placed on shelves between the two glass lids of the cases. Most of the contents of this collection have been purchased from time to time from the Naples or the Plymouth Zoological Stations.

The collection destined to illustrate the Vertebrate series is much less advanced; as yet it consists only of a few skeletons and preparations placed in a wall case.

With regard to the Faunal collections, the Mammals are not as yet very far advanced, but the Museum possesses a very good collection of stuffed birds, each species being mounted in a separate glass fronted case. This collection is interesting historically, having been formed by Dr. Thackeray, the late Provost of King's College, Cambridge, who was a friend and correspondent of Yarrell. It is said to have been used by Yarrell while writing his celebrated "History of the British Birds"; indeed many of the well-known figures in that work seem to have been drawn from the birds now in the Museum.[2]

Provost Thackeray left his collection to the School, and it formed for a long time the nucleus of the Museum in the Rotunda above mentioned. Although it was made so many years ago, the specimens still remain in a very fair state of preservation, except for the fading of some of them. It is arranged along the walls on both sides of the room.

It has been lately proposed to add to the mounted series a collection of British birds "in skin," which will be much more useful for close examination and handling, and a beginning has already been made in this undertaking.

The collection of birds' eggs is arranged in glass-topped boxes and placed in drawers of a special cabinet presented by Mr. A. C. Benson, one of the Masters; the collection has increased a good deal during the last two years, but is still by no means perfect.

A very good series of British butterflies and moths, arranged in six cabinets, was presented many years ago to the school in memory

[2] *See*, for example, the Alpine Accentor, shot in the garden of King's College, Cambridge, on November, 1822, Yarrell's "History of British Birds," ed. 4, vol i., p. 296.

of Arthur Vernon Jones, an Eton boy, who himself collected the greater part of it.

The Geological collections are arranged in three long table-cases, extending two-thirds of the total length of the Museum. One of these is occupied by fossils arranged stratigraphically, another by a collection of rocks and minerals, and the third by a set of cases illustrating economic processes, such as the ores, varieties and uses of coal, sulphur, iron, tin, lead, and soda.

The collections in these cases have been received from various sources; a considerable number were given by Eton Masters—the late Mr. P. H. Carpenter, Mr. Hale, and the late Mr. Drew and others. A large number of the specimens, especially of the fossils, were purchased from Mr. J. R. Gregory, who also named and arranged most of those already in the Museum.

The Museum now possesses a very excellent collection of minerals, derived from three sources. (1) A collection in a cabinet presented by the Rev. S. A. Donaldson, an Eton Master; (2) a collection made by the late John Murray, of Albemarle Street, and recently presented in memory of him by his son, the present Mr. John Murray, also contained in a cabinet; and (3) a collection specially formed for the Eton Museum at the Natural History Museum at South Kensington.

There is one set of objects in the Museum which has a very close relationship to Eton, and in fact forms almost the only part of the Museum which has a distinctly local interest. This is a series of prehistoric implements of neolithic age, which has been obtained by Mr. Edward Hale from the men who dredge for gravel in the Thames. Besides many examples of the ordinary type of adze, the collection contains flint-chippings, arrow heads, bone-needles, and all the other characteristic remains of the neolithic people, including a remarkable dolichocephalic skull dredged up in the same way.

There is also a small collection of anthropological implements, of which the larger objects such as clubs, weapons, etc., etc., have been mounted on a large wooden shield. This collection was made by the late Sir John and Lady Franklin, and presented to the Museum by Mr. G. B. Austin Lefroy.

A great speciality has been made of photographs in the Eton Museum; a large collection of Gambier Bolton's well-known studies of animals, mounted and framed, with short descriptions and small maps of the geographical distribution, form a prominent object on the south wall of the Museum. We have also got three of Mr. Saville Kent's photographs of the Great Barrier Reef which were specially enlarged for museum purposes, and finally a considerable number of photographs of geological phenomena have been accumulated, all of which are mounted and framed, with descriptions appended and hung on the walls.

The Museum is open every day, except Saturday, between the

hours of 12 and 2, and on whole-school days (*i.e.*, Monday, Tuesday, Wednesday, and Friday) between 4 and 5. Lately the experiment of opening on Sunday afternoons has been tried, and has succeeded admirably. A very large number of boys avail themselves of this opportunity every Sunday, but it must be confessed that the week-day attendance is rather meagre. Whether this can ever be remedied is a difficult question, the present Eton boy having very little leisure during the day. Besides the regular school hours, which, it must be confessed, are not too long, a large amount of time is taken up by the average boy in learning his lessons and also "extras" (*i.e.*, private tuition in extra subjects). When to this is added the time necessarily spent in games, there is very little leisure time left to the ordinary Eton boy for improving his mind, even if he wishes to do so.

The conception and carrying out of the Eton Museum has been almost entirely due to Dr. Warre, the present Head Master, who has always shown a most keen appreciation of the advantages and uses of a School Museum. In these ideas he has been ably seconded by the scientific members of the staff of masters: Mr. Hale and the late Messrs. P. H. Carpenter and J. Drew, who have all largely contributed out of their private means to the support of the Museum. The greater amount of the actual work of planning and setting in order the Museum as it now is, was, however, undertaken by Mr. A. Vaughan Jennings, who, after a year's hard work, had to resign the post of curator on account of ill-health.

W. L. SCLATER.

VI

Instinct and Intelligence in Chicks and Ducklings.

IN my "Animal Life and Intelligence" I quoted some of Spalding's statements as to the intelligence of young birds. I then received a letter from my friend, Mr. T. Mann Jones, informing me of observations of his own which did not accord with those which I quoted, and expressing some scepticism as to the existence of what he termed "the philosopher's chick." I therefore determined to observe for myself, and the following paper contains some account of my observations, which should be compared with those of Douglas Spalding in *Macmillan's Magazine* for February, 1893, and those of Professor Eimer in his "Organic Evolution" (English Translation, p. 245). I desire to express my acknowledgments to Mr. Mann Jones for his suggestions and criticisms.

The eggs were incubated under the hen until about the third day before hatching, when they were transferred to an incubator. After hatching the young birds were left in the drawer of the incubator for from twelve to twenty hours. They were then kept under observation in a small pen surrounded with wire netting in my study. There was thus no influence of adult birds. I was their only foster-mother. I shall describe the observations under the head of the day of chick or duck life—first day, second day, and so on—dating from their removal from the incubator drawer.

FIRST DAY.—*Chicks.*—On opening the drawer of the incubator the newly-hatched birds are often seen to huddle together and to try and burrow under each other. Experiments on the co-ordination of pecking show that any small conspicuous object is struck at. The aim was seldom quite correct, the tendency being apparently to strike somewhat short. Moving the object a little with a long steel pin caused it more readily to catch their eye. It was generally seized at the third or fourth stroke, but a little awkwardly, and was not always successfully swallowed. Flies, from which a portion of their wings had been removed, were followed as they ran, and were seized at from about the seventh to the twelfth stroke. The chicks pecked persistently at their own and each other's toes and at the bright bead-like eyes of their yellow neighbours, also at excrement, shaking their heads and wiping their bills.

Ducklings.—The pecking co-ordination was imperfect. When a piece of white of egg was seized it was mumbled rapidly and shaken out of the bill unswallowed. Towards the close of the day they began to swallow what they seized, but the pecking co-ordination was not quite perfect. They were at first very unsteady on their legs (more so than the chicks) and tilted over backwards on to their tails. One scratched its head, but toppled over, the double co-ordination of standing on one leg and scratching its head was more than it could manage. They walked several times through the water placed in a shallow tin, but took no notice of it. I dipped the beak of one of them in the water; it then drank repeatedly, shovelling up the water with characteristic action. Presently the others imitated the action and drank freely. I dropped, at different times, two ducks into a tepid bath. They kicked vigorously and excitedly, dropping their excrement, but in a minute swam about with easy motion, pecking at marks on the sides of the bath.

SECOND DAY.—*Chicks.*—Several ran repeatedly through the water in a shallow tin, but took no notice of it. Then, after about an hour, one of them standing in the water pecked at its toes. It lifted its head and drank freely with characteristic action. Another subsequently pecked at a bubble near the brim and then drank. The stimulus of water in the bill at once led to the characteristic responsive action. Others came up and pecked at the troubled water; they too then drank. Later on one was running and toddled into the tin; it stopped at once and drank. Wet feet seemed to suggest drinking by association. I placed two winged flies before them. One chick seized a fly at the first stroke. Another followed the second fly and made three pecks at it, but the other chick rushed in and caught it at the first stroke. A large winged fly thrown among other chicks was approached by one bird which gave the danger note (a very characteristic sound). Subsequently the same chick followed it and caught it after several bad shots. They pecked about equally at four kinds of grain, millet, canary, groats, and pari; but swallowed more of the millet. They also pecked at and swallowed sand grains. I took one of the chicks and put it down near a young cat. The bird showed no signs of fear.

Ducklings.—Both ducks made at once for water in shallow tin, drank and squatted down in it. They ate keenly of white of egg, swallowing large morsels, the pecking co-ordination being nearly accurate. Both scratched their heads occasionally and toppled over. They preened the down, especially of the breast, in characteristic fashion; they also applied the bill to the base of the tail and rubbed the sides of their heads along the back in quite approved duck fashion. They stood up stretching out their necks and flapping their wings, sitting down on their tails from imperfect co-ordination. They showed much less accuracy of aim than the chicks in catching running flies. The abortive attempts were numerous.

They ate their own and chicks' excrement freely and showed little sign of disgust. (In South Africa young ostriches are often supplied with the droppings of the old birds, for medicinal purposes. So I was informed).

THIRD DAY.—*Chicks.*—The chicks pecked excitedly at flies placed in an inverted tumbler, but failed to catch them on the wing when the insects were allowed to escape. They still peck at any small objects, especially bright ones, but show more discrimination in swallowing. They run to one's hand when one pecks on the ground with one's finger or a pencil, simulating the action of a hen. One can thus induce them to seize objects which they would otherwise leave untouched. They will always run to nestle in one's hands, poking their heads out between one's fingers prettily. To some chicks (Group A) I threw cinnabar caterpillars. They were seized but at once dropped, with some wiping of the bill. The caterpillars were uninjured, and were seldom touched again. They were removed and thrown in again towards the close of the day. Some chicks tried them once, but they were soon left. I could induce birds to pick them up by "pecking" with a pencil, but they were at once dropped.

Ducklings.—There was nothing special to note.

FOURTH DAY.—*Chicks.*—I threw to the chicks of group A some looper caterpillars and some green caterpillars from gooseberry bushes. They were approached with some suspicion. Presently one chick seized one and ran off, giving rise to a stern chase. Another stole it from the first and ate it. In a few minutes all the caterpillars were cleared off. Later in the day I gave them more of these edible caterpillars, which were eaten freely. Then some cinnabars. One chick ran, but checked itself, and without touching the cinnabar wiped its bill (association). Another seized one and dropped it at once. A third subsequently approached a cinnabar as it walked along, gave the danger note and ran off. Then I threw in more edible caterpillars, which again were eaten freely. The chicks thus discriminate by sight between the nice and the nasty caterpillars. To a second group (B) I threw cinnabars and small worms. Both were seized at first with equal appetence, but discrimination was soon established. The chicks began to scratch the ground (perhaps also the day before, but not markedly). Several of them pecked at the burning end of a cigarette two or three times, but some were stopped by a whiff of the smoke, and then shook their heads and wiped their bills. Subsequently, when the cigarette was out and cold, they came and looked at it; and one, after eyeing it, wiped its bill on the ground. A large *Carabus* beetle, sprawling on its back, was an object of fear; one chick at last pecked at it, uttering the danger note, and threw it on one side. After this none went near it.

Ducklings.—Experiments with cinnabar caterpillars, loopers, and worms gave similar results to those obtained with the chicks.

FIFTH DAY.—*Chicks.*—One of the birds, bolder than the rest, would eat large flies with relish. I threw in a bee. Most of the chicks were afraid, as they were of large flies. The bolder chick, however, snapped it up and ran off with it. Then he dropped it and shook his head, wiping his bill. Probably he tasted the poison and was not stung; in any case, he was quite lively and unconcerned in a few minutes; but he did not touch the bee again. The chicks preened their down early on this day. If they had done so before, I failed to note the fact. Later in the day I put beneath a tumbler a large fly and a small humble bee with a sting. Two of the chicks ran round the tumbler pecking at the insects. I let the bee escape. The bolder chick seized it, dashed it against the ground, and swallowed it without a wink. With another group of chicks I first gave bees, which were seized but soon let alone, and then *Eristalis*. They were left untouched. Their resemblance to the bees was protective. Later I gave *Eristalis* again, and induced one of the chicks to seize it by pecking at it with my pencil. He ran off with it, chased by others. It was taken from him and swallowed. The other *Eristalis* insects were left untouched, but one was subsequently eaten.

Ducklings.—I placed some frog tadpoles in their water. They were soon spied and eaten greedily. The vulgarity of the duckling as a feeder is painful to witness.

SIXTH DAY.—*Chicks.*—I gave them their tin without water. They stood in it and pecked, one lifting its head. They scratched at the bottom vigorously, and pecked again and again. On this day they frequently stood up, stretching out their necks and fluttering their wings. They may, however, have begun to do this earlier. Several of them pecked at a sleepy wasp, but soon let it alone. I made a number of experiments on this and the previous day with regard to their ability to catch flies on the wing, placing the insects under a tumbler. The birds pecked at them as seen through the glass. I then let them, one by one, escape. The chicks made a dash at them, but never succeeded in catching one, though they caught one or two as they crawled out and before they had taken flight. I tried also with tumblers covered with cards. I may add that up to thirteen days I have never yet once seen a fly captured on the wing by either a chick or a duckling, though I have often seen them struck at.

Ducklings.—Each morning, at nine o'clock, I had placed in their pen a large black tray, and on it a flat tin containing water. To this they eagerly ran, drinking and washing in it. On the sixth morning I gave them the tray and tin in the usual way, but without any water. They ran to it, scooped at the bottom, and made all the motions of the beak as if drinking. They squatted in it, dipping their heads and waggling their tails as usual. For some ten minutes they continued to wash in non-existent water (association). I then gave them water. I threw them a bee: one of them seized it and swallowed it. Possibly he was stung. He kept on scratching his beak—first on one

side, then on the other, and seemed uneasy. He was all right again, however, in half-an-hour, but did not seem keen after a bee I offered him; nor would he take any notice of an *Eristalis*.

SEVENTH DAY.—*Chicks* (Group A).—I threw in a number of bits of red-brown worsted, one to two inches long. They were seized with eagerness and eaten with avidity. I could not satisfy them with worsted worms, and desisted in the attempt lest the diet should produce unpleasant effects on their little gizzards. I left, however, one four-inch worsted worm, of which the chicks seemed afraid. Presently the bolder one seized it, ran off with it chased by the others, escaped from the pen, reached a secluded corner of my study, and with great efforts swallowed it to the last half-inch. The same chick pecked repeatedly at something near the corner of the turned-up newspaper which then formed the wall of my pen (I now use wire netting). This I found to be the number of the page. He then transferred his attention to the corner of the paper, which he could just reach. Seizing this he pulled at it, bending it down and thus forming a breach in the wall of my experimental poultry-yard, through which he escaped. I caught him and put him back near the same spot. He went at once to the corner, pulled it down, and escaped. I caught him and put him back on the other side of the pen. Presently he sauntered round to the corner, began pecking again, and escaped. I then pulled it up out of his reach. He pecked at it, but soon desisted. This is a good, simple example of the intelligent utilisation of a chance experience. Group A, including this chick, were near the close of their seventh day returned to the yard from which the eggs were obtained through the kindness of my friend, Mr. John Budgett. They were adopted by a broody hen, and were reported to seem afraid of her.

Very noticeable at this stage is the effect of any sudden noise—a sneeze, clapping one's hands, a sharp chord on the violin; or of suddenly pitching among the chicks a piece of screwed-up paper. They scatter and crouch, or sometimes simply crouch down where they are. The constant piping cheep-cheep ceases, and for a moment there is dead stillness, each bird silent and motionless. In a minute or so, up they get and resume their cheeping notes.

Ducklings.—I repeated the experiment with the dry tin. Again they ran to it, shovelling along the bottom with their beaks and squatting down in it. But they sooner gave up the attempt to find satisfaction in a dry bath.

EIGHTH DAY.—*Chicks.*—On this day I noticed for the first time the chicks crouching down and making all the movements of sand-washing or dusting themselves in the way many birds affect. There was only a little sand strewn over the newspaper and not much good came of the operation. Still it was persisted in for a quarter of an hour at a time. I tried these too (Group B) with worsted worms. They seemed to give complete satisfaction, and there was many a

stern chase after the fortunate possessor of an inch of worsted. I tried them again with cinnabar caterpillars, of which they took scarcely any notice. None were seized. I threw in a lump of sugar. The chicks stood round it, uttering the danger note. Then some ran at it, pecking rapidly and withdrawing in haste. They deal thus with moderate-sized suspicious-looking objects.

Ducklings.—On repeating again the experiment with the empty tin they soon left it, and did not squat down in it at all. But when I poured in water they ran to it at once.

TENTH DAY.—*Chicks.*—I took two of the chicks to the yard from which the eggs were obtained, and opened the basket, in which I had carried them, about two yards from a hen which was clucking to her brood. They took no notice whatever of the sound. They were not in a frightened condition, for they jumped on my hand and ate grain off it, scratching at my fingers. I put them with a hen in a small fowl-house. They did not seem frightened, or, if at all, but little. To those that remained I took back a large humble-bee. One darted at it, giving it a sharp peck, and throwing it disabled to one side.

Ducklings.—One of the ducklings seized the disabled bee, and, after mumbling it for some time in the water, swallowed it.

THIRTEENTH DAY.—I took the remaining chicks to the yard. A hen in a fowl-house was clucking eagerly to her young brood. The chicks were put down outside, out of sight of her. They took no notice whatever of the clucking sounds she made, but scratched about round me. They were then placed among her brood. She seemed inclined at first to drive them away, but afterwards looked more kindly on them. But they did not keep close to her like her own brood. I went over to see them next day. One was at some little distance from the hen. I leant down and held out my hand. The little thing ran to me and nestled in my palm.

The sounds emitted by the chicks are decidedly instinctive, and some of them are fairly differentiated. At least six may be distinguished. First the gentle piping, expressive of contentment. It is heard when one takes the little bird in one's hand. A further low note, a sort of double sound, seems to be associated with extreme pleasure, as when one strokes the chick's back and cuddles it. Very characteristic and distinct is the danger note—a sound difficult to describe,—perhaps somewhat as if a miniature policeman's rattle were sprung inside the chick's head. This is heard on the second or third day. If a large humble-bee or a black-beetle, or a big worm or lump of sugar, or in fact anything largish and strange be thrown to the chicks, the danger note is at once heard. Then there is the cheeping, piping sound, expressive, apparently, of wanting something. It generally ceases when one goes to them and throws some grain or even stands near them. My chicks were accustomed to my presence in the room, and generally were restless when I left them and made this sound. Then there is the sharp squeak when one seizes them

against their inclination. Lastly, there is the shrill cry of distress when, for example, one of them is separated from the rest. I have very little doubt that all of these sounds have, or soon acquire, a suggestive value of emotional import for the other chicks. Certainly the danger note at once places others on the alert. But the suggestive value seems to be the result of association and the product of experience.

The foregoing observations I have presented much in the form, though with many omissions, in which they were noted down at the time; hence much crudity of expression. They appear to me to suggest—

(1.) That there are many truly inherited activities performed with considerable but not perfect exactitude in virtue of an innate automatism of structure.

(2.) That associations are formed rapidly and have a considerable amount of permanence.

(3.) That intelligent utilisation of experience is founded on the associations so formed; such associations being a matter of individual acquisition, and not of inheritance.

(4.) That there is no evidence of instinctive knowledge, even in a h[illegible]se acceptation of this word. This follows from the non-inheritance s[illegible] associations of impressions and ideas. *Co-ordination of activities* is thus apparently inherited, but *not correlation of impressions and ideas.*

(5.) That even the inherited co-ordinations are perfected and rendered more effective by intelligent guidance.

(6.) That imitation is an important factor in the early stages of mental development.

(7.) That the inherited activities on their first performance are not guided by consciousness, though they are probably accompanied by consciousness. The *rôle* of consciousness is that of control and guidance. Only on the first performance of an inherited activity is the chick a conscious automaton. In so far as the activity is subsequently modified and perfected by intelligence the agent exercises conscious control. If we then term it an automaton, we must admit that the automaton has a power of control over its actions in accordance with the conscious concomitants of certain cerebral changes. Into the physiological mechanism of control, as I conceive it, I cannot enter here.

C. Lloyd Morgan.

SOME NEW BOOKS.

THE PSYCHOLOGICAL REVIEW. Vol. I., no. 1, January, 1894. New York and London: Published bi-monthly by Macmillan & Co. Single numbers 75 cents. Annual subscription 4 dollars.

THIS new periodical makes an excellent start, including long contributions from Professor Ladd (his Presidential Address to the New York Meeting of the American Psychological Association), from Josiah Royce on "The Case of John Bunyan." Hugo Münsterberg sends the first of a series of studies from the Harvard Psychological Laboratory. Mr. Francis Galton has a note on "Arithmetic by Smell." Professor James discusses Wundt on "Fe 'ings of Innervation" and the other articles and reviews are well up to the standard implied by the foregoing.

Professor Ladd's Presidential Address is very interesting. He discusses first the burning question of the relation of Laboratory work in physiological psychology to the general subject. He extends a generous welcome to the young science, but deprecates that ardour of its votaries which sees science in no other method. No doubt Professor Ladd holds rightly that scientific psychology must include not only laboratory work but introspection and "reflective study of that artistic delineation of soul-life in which the best novels, poems, and dramas are so wonderfully successful." But it is not good that all truths should be blazoned abroad and in those sciences that have a side open to the inexact and the amateur, it is specially necessary to distinguish between those who write about things and those who work at them. For this reason our sympathies are with those who insist on the laboratory and the methods of the laboratory. For similar reasons our sympathies are again with the experimental school in the endeavour to get at objective conclusions in psychology. No doubt the result of "series" of experiments and generalisations from statistics must be interpreted in terms of consciousness. But that, in a sense, is true of all science, and though the materials of psychology are peculiarly changing, evanescent, complex, personal, laboratory work has shown already that objective truths can be reached. There is no need to insist upon introspection: we all "introspect": perhaps we all must do it, but there is need to insist upon exact methods.

Professor Ladd's treatment of the relation between science and philosophy is very interesting. He traces the events leading to the relation of mutual contempt in which the two subjects stood twenty years ago, and although at the present day "reconciliations" are chiefly effected by "men of second-rate quality," he approves of the spirit. "For philosophy is but wild and mischievous speculation, unless it build itself upon the concrete and particular sciences; and science is but the unsatisfying husk of knowledge, is without rational self-consciousness and highest import and divinest interest, unless it intelligently lend itself to help, and to be helped by, philosophy."

P. C. M.

A Theory of Development and Heredity. By Henry B. Orr, Ph.D., Professor at the Tulane University of Louisiana. New York and London: Macmillan and Co., 1893. Price 6s.

At the present time all theories of Development and Heredity are classified naturally in accordance with the views of the writers about the inheritance of acquired characters. Professor Orr accepts the principle of such inheritance but he is somewhat cavalier in his treatment of the question. He compares one-celled animals with many-celled animals from the "physiological" point of view, and comes to the conclusion that there is no sure foundation for the assumption of a physiological difference between the great groups. In the one case the mass of protoplasm is continuous: in the other the protoplasm is intersected by cell-walls, running in all directions. "In the simpler forms of the metazoa each cell has a nucleus: in the more complex of the protozoa a single-celled animal may have several nuclei. Thus we see that the nucleus affords no basis for the assumption of such a profound distinction between the groups." Cell-walls (primarily, at least) are merely "an indispensable means of support." "None can maintain that the sluggish Hydra is a more highly-developed organism than the graceful Stentor or Paramœcium." In higher animals the complicated nerve-tracts place the organs in a more immediate connection with each other even than the protoplasm in the cells of Hydra. It is true, he admits, that metazoa differ from protozoa in that they have germ-cells. But these are merely part of the protoplasm which retains its original qualities, "only changing its nervous condition to a condition of greater complexity of co-ordinations." The germ-cells are, therefore, able to develop as did their forbears, with the addition of the "nervous" changes impressed on them by the forces of the environment. Professor Orr gives instances showing that the germ-cells are not isolated from the rest of the organism; instances like the effects on the body of castration; instances like changes of season determining the period of maturation. But hear the Professor:—

"Finally, since the germ-cells possess the potentiality of producing all the nervous and mental traits of the body from which they are derived, we cannot suppose them utterly disconnected from the nervous system and inaccessible to its influence. There seems to be a most profound and intricate connection between the two.

"We find, therefore, no essential difference between the development and reproduction of the protozoa and the same process among the metazoa. If inheritance of acquired characters be admitted for unicellular organisms (and I can see no other reasonable interpretation of the fact), then the same must be admitted for multicellular organisms."

Biology, according to the Professor, stands between the science of physics and psychology, and the laws of these cannot be neglected in biological research. The forces of growth and development must be regarded as transformations of the general sum of energy. Starting from this he builds up a theory that is epigenetic in the extremest form. It is the pressure of the outer world that moulds and forms the growing organism. Light and heat, vibrations and impacts, the chemical and physical nature of gases and food-substances, are the active forces at work, and by them protoplasm becomes organism. Some examples will show the extent to which Professor Orr attributes direct mechanical effect to the environment. The fauna of the Gulf Stream are *all* yellow because the seaweed in which the animals live is yellow, and living matter is capable of photographing colours: so

also with the tawny animals of the desert. Nerves are formed because nervous molecular impulses seek the channels of least resistance: and by repeated passing through the same channels, cause a differentiation of the tissue. Cave animals are blind because the eyes of many generations "have never been stimulated to their full growth by light."

Psychology contributes to the theory, the laws of Association and Repetition. Professor Orr believes that the nervous system is merely a specialisation of the nervous activity of cell-protoplasm, and so cheerfully applies to all living matter considerations drawn from psychological observations on human beings. The simplicity of the theory consists in this: if you assume that outward forces are the "cause" of growth and development, and that molecular changes corresponding to memory of growth and development are transferred to the germ-cells, it is quite clear that under similar forces the germ-cells will be changed in the same way: only they will be changed more quickly, because they know what they are bound to do.

P. C. M.

INVESTIGATIONS ON MICROSCOPIC FOAMS AND ON PROTOPLASM: Experiments and Observations directed towards a Solution of the Question of the Physical Conditions of the Phenomena of Life. By O. Bütschli, Professor of Zoology in the University of Heidelberg. Authorised translation by E. A. Minchin, B.A. (Oxon.), Fellow of Merton College, Oxford. 8vo. London: Adam and Charles Black, 1894. Price 18s.

SOME time ago[1] in an account of the original German edition of Professor Bütschli's work, I was able to let readers of NATURAL SCIENCE know that an authorised translation by Mr. Minchin was in preparation. This has now appeared, and the publishers, the translator, and Mr. Minchin are to be congratulated. Only those who have had to grapple with the perplexing German of the original can appreciate how lucidly and how skilfully Mr. Minchin has interpreted his author. In *format* the book is an improvement on the stitched German quarto; the plates have lost nothing in the transference, and the convenience of the reader is consulted by the relegation to smaller type of some discursive controversial matter.

It is needless to describe at length a book which biologists have now no excuse for neglecting. Professor Bütschli, in the course of his well-known investigations on Protozoa, came to see that there are all gradations between scattered vacuoles and a completely alveolar or reticular structure. He was led in this way to interpret all reticular structure in protoplasm as honey-combed structure. Then, after patient effort, he succeeded in manufacturing microscopic foams that, under the microscope, presented the reticulate appearance of protoplasm. In this book he describes these foams minutely and compares them with protoplasm as seen in many organisms. But an unexpected marvel was the appearance of streaming movement and actual progression in his artificial foams. The discussion and physical explanation of these, and the suggestions these physical explanations give for the analogous processes in living material, form the most startling part of Professor Bütschli's contribution to science. These are all matters to be worked out with microscope and materials, rather than to be discussed and criticised in writing. Do not reason with a doubter, using vain words, but lead him to a laboratory and show him the foams streaming and moving.

P. C. M.

[1] NAT. SCIENCE, Jan., 1893, "Artificial Protoplasm," by P. Chalmers Mitchell.

'Mongst Mines and Miners; or Underground Scenes by Flashlight: a series of photographs with explanatory letterpress, illustrating methods of working in Cornish mines. By J. C. Burrows and William Thomas. 4to. Pp. 32, 27 photographs, and vertical plan of workings of the Dolcoath Mine. London: Simpkin, Marshall & Co., 1893. Price 21s.

This book is most interesting. Apart from its peculiar value as a faithful record of the method of working a mine, it gives the reader a good idea of the difficulties to be overcome in winning metals. Mr. Burrows has spent a twelvemonth in producing the twenty-seven excellent photographs contained in the book, and gives a lively picture of the patience necessary to enable one to become a successful photographer of underground mining scenery. The apparatus used was a Zeiss' Anastigmat lens, series iii. (Ross & Co.), and the artist enthusiastically terms it "a perfect gem." Of the many plates used, none equalled the Cadett lightning plates, which proved so satisfactory that in some cases every miner was taken in his working position with his light in its usual place. For illumination, limelights at their maximum intensity had to be employed simultaneously with powerful flash magnesium lamps and ribbons. For close subjects two triple-flash magnesium lamps were generally used, but in large areas more lamps were brought into requisition. The mines chosen for illustration in this work are the Dolcoath, Cook's Kitchen, East Pool, and Blue Hills. The photographs themselves, which are described by Mr. Thomas, include views of the man-engine at Cook's Kitchen and Dolcoath; the balance box and gig at Dolcoath; the 355 and 406 stopes at Cook's Kitchen; the 180 at East Pool; the 375 and the 412 at Dolcoath, and the 66 at Blue Hills; the 70 tram-road and the bridge, East Pool; engine shaft at Cook's Kitchen; the heave at Blue Hills; rock-drilling, milling, overhand and underhand stoping, and a group of miners at "croust," or afternoon meal.

The photographs are excellent, and most detailed; in those cases, too, where the men are holding candles the flame is seen to be perfectly distinctly outlined. We congratulate both artist and author on the results of their patient but enthusiastic labour.

The Oxford Museum. By Henry W. Acland, M.D., and John Ruskin, M.A. 8vo. Pp. xxxvi., 112, two plates, and folding plan. From original edition, 1859. With additions in 1893. London and Orpington: George Allen, 1893.

To all Oxford men, to all scientific men, to all artists, architects and craftsmen, to all who have any sympathy with either nature or art, this book comes fraught with stirring and pathetic interest. The book itself is professedly an address delivered by Sir Henry Acland, in 1858, to certain Architectural Societies, on the building of the Oxford Museum. But admirable though this address is, it is, we venture to think, in that which has grown around it, in the letters of Mr. Ruskin, in the notes and in Sir Henry's preface to the present edition, that the chief interest of the book will now be found. We have here a double story: the story of the revival of Gothic architecture, and the story of the progress of scientific education in an old university; and the two stories are bound together by the impressive personality of John Ruskin. How the Museum gradually came to be, and how it rose beneath the enthusiastic hands of an artistic band of Irish craftsmen, is a tale delightfully told in this volume. Then with wonderful delicacy and tact Sir Henry relates the causes that led to Mr. Ruskin's resignation of his professorship when Convocation voted

£500 a year for carrying on the work in Dr. Burdon Sanderson's Physiological Laboratories. Sir Henry regrets that no living plants or animals are kept in connection with the Museum. "Life in action, with the habits thereto pertaining, is a study as worthy as is the machinery which makes, preserves, and brings it to a close. It is a fault in most museums that only the mechanism of life and not its living actions are displayed."

What follows is of sufficient importance at the present juncture to quote in full. "These general thoughts may seem strange to those in Oxford who, from imperfect knowledge, desire to change the Museum into a so-called 'medical school.' They perhaps have not reflected on the loss that they will inflict on the Profession of Medicine if they succeed. Forty years ago it was hoped to add to the wide Philosophical, Historical, Theological life of the old University the means for similar study of the material Universe considered alike in its Unity and in its special parts. It was felt that this would harmonise with, and supply the missing link in the aims of the old education. The opportunities were to be open to all, for whatever walk in life destined. Adapt it only to one profession such as medicine, you rob all others of the larger opportunity, and—what is even worse—persuade future Oxford graduates that medicine has no relation to science as a whole; that it is a specialism, grounded on itself alone, and that the essence of its education is to prepare by schedules for passing examinations. No greater educational fallacy can exist. To give colour to it is a cruelty to all our youth. Our best students already feel this to be so. The foundation by them of the Robert Boyle Lecture is a proof. Wider views are held by the best thinkers, even for our elementary and Government schools. The conception is a relic of days of ignorance."

Like all works to which Mr. Ruskin's name is attached, this one is beautifully produced. One of the two exquisite plates represents the capital of a pillar in the central court of the Museum, while the other is a charming portrait of the two authors.

NOVITATES ZOOLOGICÆ: A Journal of Zoology. Edited by the Hon. Walter Rothschild, Ernst Hartert, and Dr. K. Jordan. Vol. i., no. 1. Imp. 8vo. Pp. 266 and three plates. Issued January 30, 1894, at the Zoological Museum, Tring.

THIS handsome publication, to which the yearly subscription is one guinea, has been started mainly for the purpose of describing new species, and, we presume, of thereby adding to the type-specimens in the Rothschild Museum. The species-mongering is, however, not to be done rashly, or at the mere caprice of writers; it is to be subject to certain principles which are laid down by the Editors in an introductory note. "The basis of truly scientific, systematic work is the knowledge of the species and their geographical distribution. Therefore we ought to distinguish between the different forms, even if their differences are very '*slight*,' provided they are *constant*. If very closely allied forms are connected by intermediate specimens (as is often the case in the countries where their areas meet or overlap), it is practically not advisable to admit them as distinct species, but they ought to be degraded to the rank of sub-species. Island forms, however slight may be their differences, will, in most cases, be more readily recognised as worthy of specific rank than similarly closely allied forms with a mainland distribution. The term 'variety,' especially among entomologists, has been indiscrimi-

nately used to denote an individual variation within a species as well as climatic or geographical races. We, therefore, to avoid all possible errors, have determined to discard the term variety altogether. To denote individual variations we shall employ the word aberration, and for geographical forms which cannot rank as full species, the term sub-species."

The present number contains communications from the three editors, from Dr. Forsyth Major and Mr. W. F. Kirby, which are written in Latin, German, or English. Nothing is said as to language, but we hope Mr. Rothschild's catholicity will not include Russian or Japanese. Two of the plates are beautiful specimens of the art of J. Smit and J. G. Keulemans.

THE CRINOIDEA OF GOTLAND. Part i. The Crinoidea Inadunata. By F. A. Bather. *Kongl. Svenska Vet. Akad. Handl.* Bd. xxv., no. 2. 4to. Stockholm: 1893.

THE Silurian beds of the Isle of Gotland have long been famous for the wealth of numbers and excellence of preservation of their fossils, ranging from the Upper Llandovery to the Upper Ludlow. Numerous monographs have appeared upon them from time to time, for they have yielded to the Swedish school of palæontology its most profitable material. The Crinoids have been elaborately figured in the great folio "Iconographia Crinoideorum" of Professor Angelin in 1878; and probably no one who has had occasion to use that work but has felt that it is not a fair specimen of Swedish science; the death of the author before its completion probably accounts for many of its errors. The unsatisfactory nature of Angelin's posthumous monograph has been the more regretted owing to the singular wealth and interest of the Gotland Crinoids. Students of Echinodermata will therefore welcome the first of a series of detailed memoirs on these fossils from the pen of Mr. F. A. Bather.

This first part deals only with the Inadunata, the Order in which all the brachials are free and the interradials do not take part in the composition of the dorsal cup except in the posterior interradius. The memoir consists of 200 pages, and 10 admirable plates, crowded with instructive and artistic figures by Mr. Liljevall. With the exception of a few introductory pages, which deal with the terminology, bibliography, and stratigraphical horizons, it consists entirely of a detailed account of the structure of the Crinoids, with discussions of the affinities and synonymy of the various species. The Inadunata in the fauna comprise thirty-nine species sufficiently well known to be described; these are referred to ten genera, and the imperfection of our knowledge of the Silurian Crinoids may be judged from the fact that these ten genera are referred to no less than seven or eight families. Of the species fifteen are new, and there is one new genus appropriately named *Gothocrinus*.

The present, however, is not a work to be judged by the number of species created; it is in its detailed dissections and figures, its precision of nomenclature, the care that has been taken in determining the morphological meaning of the different elements of the skeleton, and the patience with which the nomenclature and synonymy have been discussed, that its real merit lies. Forms such as *Pisocrinus* and *Calceocrinus*, that have been previously turned upside down, wrong side before, or inside out, to suit the varying explanations of their describers, are now finally explained, owing to the recognition of the anal tube. The curiously coiled *Herpetocrinus* is also now first satis-

factorily worked out. One of the most remarkable innovations is the abandonment of the two suborders Larviformia and Fistulata, which have always seemed unsatisfactory on *à priori* grounds, and a return to a classification based on the presence or absence of infra-basals. The author admits that this may seem a retrograde step, but as in this mixed world advance does not always spell progress, it is, probably, none the worse for that.

In a brief notice such as this, one can do but scant justice to this important contribution to palæontological literature. It is unfortunate that it is impossible to get such a work published in England, and that this has had to see the light in a foreign capital. One's patriotism, however, has to get what salve it can from the recognition that this is not a sample of the rule-of-thumb palæontology which foreign workers seem generally to regard as the characteristic product of the English school. The "Crinoidea of Gotland" is worthy, by its conscientious thoroughness of detail, and its grasp of zoological principles, to rank among the best productions of modern palæontology.

THE FLORA OF THE ASSYRIAN MONUMENTS AND ITS OUTCOMES. By E. Bonavia, M.D. 8vo. Pp. xxvi., 215, with 98 figures in the text. Westminster: A. Constable & Co., 1894.

DR. BONAVIA'S books are always worth reading. Even when he puts before us no new facts, he has a way of restating the old ones which is sometimes edifying, sometimes amusing. In the preface to this, his latest work, he has attempted "from the Assyrian real flora, to creep up to their sacred flora, and to interpret certain symbols found woven in with this." He may have succeeded also "in throwing some light on the derivation of other features affiliated to those symbols." He would also have us bear in mind that whenever it may appear that he is too positive in what he states, it is only a mode of diction, and simply means that in his "humble opinion it seems so and so." As regards the Flora of the Assyrian monuments in the British Museum and elsewhere, some plants are unmistakable, as for instance the date-palm, which frequently occurs, and no wonder, considering its immense importance as a food tree, and the reverence paid to it in consequence. The vine, pomegranate, and fig are also easily recognised. Some things looking much like bundles of asparagus are put down for bananas, and we certainly think the doctor makes out a very good case, showing from the geographical and botanical history of the plant that this fruit may well have been known to the Assyrians as an imported and rare article. This also explains why only the fruit and not the plant itself is traceable on the monuments. The pine tree which occurs on several bas-reliefs is identified with *Pinus Brutia*, a conifer, ranging nowadays from Syria to Afghanistan, and most likely to have been the one which was common in the hilly countries of Assyria. "A *young* pine tree, with its symmetrical, candelabrum-like branches . . . makes a very pretty and ornamental object, and such as would attract an artist's attention." The sculptors of those days were also acquainted with a lily, which is identified with the "Madonna Lily" (*Lilium candidum*). A plant with acute radical leaves, and long-stalked, composite flowers, has by the aid of the collection of drawings at the British Museum (Natural History) been run down to *Hieracium pannosum*, a native of the rocky regions of Greece, Taurus, Cilicia, South Armenia, etc. It certainly looks like

a Composite, but he would be a bold man who would accept a specific determination made on such material and in such a very critical genus as *Hieracium*. Another thing, not unlike a sea-weed, "the identification of which appears to be hopeless," Dr. Bonavia suggests as not impossibly intended for the Baobab, a tree found in the Soudan and S.E. Africa, and introduced, perhaps, along with ivory and many other products, which must have come from the Soudan *viâ* the Red Sea.

Out of the study of the Flora arises the question of the origin of the so-called Sacred Trees, of which four or five different kinds are traceable on the monuments, viz., the date, vine, pomegranate, fir, and perhaps the oak. This leads us on to the cone-shaped object, "held in the hand of winged genii, and pointed either at a sacred tree or at the king's person, or at the entrance of a temple, palace, or town." In the other hand the genius invariably holds a bucket of some sort, and in the bucket the worthy doctor finds the key to the situation. After rejecting his own notion of the citron, and also Dr. Tylor's theory, that the cone is the male inflorescence of the date-palm, of which a further supply was contained in the bucket or basket, and that the pointing indicates fertilisation of the sacred tree, the author suggests an explanation to which he does "not see sufficient grounds for not adhering." It is that the bucket contains holy water and the cone-object represents a *cedar*-cone, used as a sprinkler. This will explain the pointing at the king's back hair and other objects besides the date-palm. As we are barely half through the book, we cannot stay to evolve a more satisfactory solution. In the next section the Lotus as a decorative object is discussed with a protest against viewing all ancient ornamentation through "lotus-spectacles." The "Anthemion," it is argued, originated in the sacred date-tree of the Assyrians. Apropos of "the Evil Eye" it is suggested that the Prince of Wales's feathers may have originated from a pair of horns (a protection against the aforesaid eye) and three date-palm leaves.

The last two sections of this very readable and well-produced work are entitled "The Trident" and "Some Notes on Cylinders."

THE GENUS MASDEVALLIA. Issued by the Marquess of Lothian, plates and descriptions by Miss Florence H. Woolward. Folio. Part v., containing ten species and varieties. London: Porter, 1893. Price £1 10s.

WE congratulate the authors of this admirable monograph on the completion of the fifth part of their undertaking, which must bring them about half-way through, as fifty species and varieties have now been figured and described. We are inclined to think the present the most interesting of any of the parts which has yet appeared. It certainly is so from a scientific point of view, as it contains a complete history of *Masdevallia uniflora*, the first Masdevallia made known to science, discovered more than a hundred years ago in the Peruvian Andes by the Spanish botanists Ruiz and Pavon, and named in honour of their fellow-countryman, Dr. Josepho Masdevall, a physician and a patron of botany. It was found only in one locality, of which a description is given by Ruiz in his manuscript diary preserved in the Botanical Department of the British Museum. Huassa-huassi, as it is called, was then a small village of about forty inhabitants, situated in the depths of a steep narrow ravine, on the banks of a mountain torrent of the same name; the slopes around were covered with brilliant flowering plants, among which orchids abounded, their

bulbs crowding the entire surface of the driest and most rocky grounds while the air was filled with the fragrance of their strange and elegantly shaped flowers. In spite of this promising description Huassa-huassi has, so far as we know, never since been visited by any botanist, nor has *M. uniflora* again been gathered. The species has never been in cultivation, and until Miss Woolward made it a subject of study there was no specimen in the country. The graceful drawing of the plant which accompanies the text is taken from the better of two of Ruiz and Pavon's specimens preserved in the Museum at Madrid; a third, consisting of two leaves and a bud from the same collection, was given by the Director, Dr. Colmeiro, to Miss Woolward, and by her to the Botanical Department of the British Museum. The fourth, and the only other specimen known, is now in the Boissier Herbarium at Chambésy. It formed part of a valuable collection of dried plants belonging to Pavon, discovered after his death by Mons. Reuter, hidden away in a garret in Madrid. Reuter purchased the collection for the Boissier Herbarium, of which he was then curator, and the orchids were submitted for examination to the late Professor Reichenbach who redescribed *M. uniflora* in "Bonplandia" (1856). Besides the small specimen from Madrid, Miss Woolward has also given to the British Museum the original sketch of the larger Madrid specimen, and one of the plant in the Boissier Herbarium.

Part V. also contains plates and descriptions of two Brazilian species not hitherto figured, and quite unknown in Europe. They were found by Senhor Barbosa Rodriguez, Director of the Botanic Gardens at Rio Janeiro, to whom the authors are indebted for the originals of the plates and the information in the text.

Several other species are also figured for the first time, and the Part opens with a splendid double plate of the gorgeous *M. chimæra*.

ZITTEL'S HANDBOOK OF PALÆONTOLOGY.

THE recent appearance of the concluding fasciculus of Professor von Zittel's "Handbuch der Palæontologie," coupled with the circumstance that the Council of the Geological Society have, most worthily, just bestowed their highest honour—the Wollaston Medal—on its learned author, affords us a fit opportunity of congratulating both the Professor himself and his enterprising publishers on the conclusion of their labours. As we have from time to time noticed some of the fasciculi which have appeared since this Journal came into existence,[1] while the limits of our space entirely forbid any detailed notice of the earlier volumes, we must content ourselves on this occasion with offering our most unqualified congratulations on the completion of the work.

It has been frequently remarked that Science has had many martyrs, and in our day the chief of these are unquestionably the compilers. Compilation is, indeed, work which, owing to the vast flood of scientific literature now poured forth without any system of publication, and in a confusing Babel of tongues, becomes year by year more necessary and more difficult. If there are few men who are willing to abandon original research, with its bracing stimulus of effort and novelty, for the duller plodding work of preparing summaries, there are still fewer capable of accomplishing this in a satisfactory manner, more especially in such a difficult science as palæontology, where so much is uncertainty and speculation.

[1] *See* NATURAL SCIENCE for March and September, 1893.

Such a compiler must be master of many languages, for most States have their own Geological Surveys, and publish reports in their own dialects, however obscure these may be. He needs great patience and capacity for work to enable him to grapple with the enormous volume of literature, combined with considerable elasticity of mind to avoid treating foraminifera and mammoths alike; he requires literary ability for lucidity of exposition and accuracy of diagnosis; he must be thoroughly well versed in zoological methods of enquiry, and completely *au fait* with the rules of zoological nomenclature; while, above all, he must be a sound geologist, fully in sympathy with and conversant with the needs and work of his fellow-labourers in the field. If any of these qualities be lacking the chances are that he will do as much harm as good, and have thrown himself in vain under the Juggernaut car of periodical literature.

It is doubtful whether any other man could be found possessing these qualities in a higher degree than Karl von Zittel, the Professor of Palæontology in the University of Munich, a brilliant linguist, a masterly teacher, a field-geologist of wide experience in Europe, Africa, and America. Uniting the laborious patience of a North German with the imagination of a Bavarian, he possesses all the qualifications necessary for the compilation of a reliable summary of contemporary palæontology. For the last eighteen years he has been engaged upon the task which he has now brought to a conclusion.

The "Handbuch der Palæontologie" is divided into two parts or sections; the first, in four volumes, dealing with the animals, being by Professor von Zittel himself, while the latter, in one volume, describing the plants, was commenced by the late Professor Schimper of Strassburg, and concluded by Professor Schenk, of Leipsic. The part on the animals alone occupies four thick volumes, containing a total of no less than 3,364 pages, with upwards of 2,976 illustrations. The first part was issued in 1877, and the last at the close of 1893. Although a work which has necessarily been such a long period in progress has some portions standing in need of revision, it will be found to contain a complete and reliable summary of the state of our knowledge of extinct animal and vegetable life at the time when the several parts respectively made their appearance. The first volume contains the general introduction, the Protozoa, Cœlenterata, Echinodermata, Bryozoa, and Brachiopoda, the second describes the remaining groups of the Invertebrates, the third the Fishes, Amphibians, Reptiles, and Birds, while the fourth is devoted solely to the Mammals.

Not only is the work issued in the original German, but, with commendable promptitude, four volumes have already been translated into French by Dr. C. Barrois, with the aid of other specialists: these four volumes respectively made their appearance in 1883, 1887, and 1891, and only the one devoted to Mammals remains to complete the French issue. It cannot fail to be a matter of regret that there is no prospect of any English translation being presented to the public; and we fear that the remark of a late President of the Geological Society to the effect that "no English publisher at present would feel justified in undertaking" the task, is only too likely to prove true.

The description of each separate order commences with a general account of the anatomy, the structure of the skeleton, and a sketch of its distribution in time. The main part of the work consists of more or less detailed systematic description, the whole of such families as

have fossil representatives being included; while short diagnoses are given of all genera of any palæontological importance. The work, therefore, is an almost complete synopsis of the whole range of palæontology.

The "Handbuch" is, however, by no means merely a compilation. Professor von Zittel has always shown himself far too conscientious a worker to be willing to be content to leave some of the groups he has had to treat in the state of hopeless chaos in which he found them. In nearly every group the author's acumen in interpreting the descriptions of others, or his researches on the very extensive collections at Munich, have considerably advanced the classification. Take, for example, the Sponges; before the author came to them in the preparation of his second "Lieferung," they were in absolute chaos, the descriptions being based almost entirely on points of such little scientific value, that it was totally impossible to make any satisfactory comparison of the fossil with the recent faunas. The author accordingly at once commenced a re-investigation of the group and devoted nearly two years to the eighty pages in which the Sponges are described; by the study of thin sections under the microscope the forms of the spicules were determined and our knowledge of the fossil sponges placed on a scientific basis. In other groups there was less room for such revolutionary changes; but there are few, if any, which Professor von Zittel has left where he found them. In the Echinoidea he founded the family Conoclypeidæ after a study of the internal structure of the peristome, and thus removed one stumbling-block from the path of echinologists. The classification of the Bryozoa was extremely advanced considering that it was prepared fourteen years ago, while many very sound advances were made in the complicated synonymy of this group.

In the Vertebrates, which, we believe we are right in saying, are not his speciality, the Professor has in the main followed the classifications of those who have made studies of the various groups. Even here, however, he at times strikes out a line of his own. Thus, as we have had occasion to mention in our previous notices, he divides the Mammals into the Placental and Eplacental groups, of which the latter includes both Monotremes and Marsupials; while in the Ungulates many modifications have been made (whether wisely or unwisely we do not care to discuss on this occasion) in the limits of family groups.

Perhaps the most valuable contribution which Professor von Zittel has made to this part of his subject is the masterly essay on the geological distribution and evolution of the Mammalia with which the last volume closes.[2] In the two preceding numbers of NATURAL SCIENCE certain remarks from the pen of an independent observer have been made in regard to the improbability of the extinct mammals of Patagonia being of lower Eocene age; and it is interesting to note how Professor von Zittel's observations support the view there taken. Thus, the Professor remarks how difficult it is to believe that mammals with such specialised dentition as those in question could by any possibility be correlated with those of the European lower or middle Eocene, and how much more closely they resemble in their grade of evolution those of the upper Eocene or Oligocene; more especially as they are accompanied by at least one

[2] Dr. G. J. Hinde translated this essay into English and published it in *Geol. Mag.*, Sept., Oct., and Nov., 1893.

existing genus.[3] If he had had the opportunity of knowing that the alleged commingling of Dinosaurian and Mammalian remains does not appear to be founded on facts the author's remarks might possibly have been still more decisive.

To proceed further in pointing out the merits of this unrivalled Compendium of Palæontological Science would, as we have said, far exceed the limits of our space, and we can, therefore, only conclude by stating that, in our opinion, Professor von Zittel and his two colleagues have succeeded in carrying out and completing their arduous task in a manner worthy of the highest admiration, and have produced a work of inestimable value not only to the palæontologist, but likewise to the zoologist generally.

An important paper on fossil vertebrates comes to us from France. In the *Annales des Sciences Naturelles* (*Zoologie*), Mr. Filhol describes remains from the Quercy Phosphorites, which he considers to be those of true Edentates. They include (1) a form, *Necromanis*, related to the Pangolins of the Oriental and Ethiopian regions, and with a skull approaching in some respects that of the South American genus *Myrmecophaga*; (2) *Palæorycteropus*, allied to *Orycteropus*, which is now found only in South Africa, though during the Pliocene period one species occurred in Samos and Persia; (3) *Necrodasypus*, related to the Neotropical Armadillos, and represented by a portion of a carapace said to resemble that of the Glyptodonts. These discoveries and determinations tend to show that the Edentates must have had a northern centre of distribution; consequently they are of great importance, if they are correct.

In the last number of the *Quarterly Journal* of the Geological Society (1 Feb., 1894), Dr. J. W. Evans gives an account of a recent journey through the Matto Grosso district of Brazil. The ground covered by the author, which was practically the region drained by the Upper Paraguay, consisted of an undulating tableland, rising in some places to 2,600 feet above sea-level. The formations exposed are referred to Quaternary, Cretaceous?, Trias?, Carboniferous?, Devonian, and pre-Devonian. The pre-Devonian beds consist of foliated and schistose rocks, highly-cleaved clay-slates, limestones, sandstones, and red argillaceous shales. The Devonian beds are sandstones, which extend over the greater part of the Chapada Plateau, from which they take the name; and are fossiliferous near Sant' Anna de Chapada, as shown in Dr. O. A. Derby's paper (*Arch. Mus. Nac. Rio de Janeiro*, vol. ix. (1890), p. 59). The Carboniferous age of other beds is inferred from the evidence of fossil ferns found at Miranda, the authority for which evidence is quoted by Dr. Evans. The Cretaceous contains vertebrate remains, as turtles and *Mosasaurus*, the nature of which decides Dr. Derby to regard them as Cretaceous rather than Triassic. A geological sketch-map accompanies the paper.

A few days before the publication of Dr. Evans' paper, appeared part 5 of volume xxviii. of the *Zeitschrift der Gesellschaft für Erdkunde zu Berlin*, containing the conclusion of Dr. Vogel's journey through the same district in 1887–88. This part of his paper consists mainly of topographical details, measurements of heights, and magnetic and meteorological observations taken during the journey. To this

[3] We may venture to point out (vol. iii., p. 738) that *Chlamydophorus*, and not *Chlamydotherium*, is an existing genus.

account is appended a paper by Dr. Ludwig v. Ammon on Devonian Fossils from Lagoinha in Matto Grosso. The specimens came from the Chapada Plateau, and consist of, among others, the trilobite *Phacops*, the gasteropod *Bellerophon* (*Bucanella*), a *Tentaculites* (*T. bellulus*, Hall), a fragment of a *Nucula*, and five Brachiopods. Of these Brachiopods one is said to be identical with *Discina baini*, Sharpe, and another with *Chonetes falklandica* (Morris & Sharpe) from the Falkland Islands, while a third is identified as *Leptocœlia flabellites* (Conrad), a North American species. Figures of these fossils are given, and Dr. Ammon notes that the beds containing them correspond to those of Ereré on the Amazonas, and to the Hamilton and Upper Helderberg series of the United States, a statement agreeing with one made by Dr. Derby in the paper referred to above.

IN the last issue of the *Quart. Journ. Micr. Sci.* (vol. xxxv., pp. 407–432), Mr. E. S. Goodrich gives an exhaustive account of all the known remains of mammals from the well-known Stonesfield Slate of Oxfordshire. From having carefully "developed" several of the specimens, the author has not only succeeded in revealing the existence of teeth supposed to be missing, but has likewise determined in others the true arrangement of their cusps. Perhaps his most interesting result is to show that *Amphitherium* had true "tritubercular-sectorial" lower molars; that is to say, these teeth had three cusps arranged in a triangle anteriorly, followed by a keel, as in the modern opossums and bandicoots. This leads him to conclude that *Amphitherium* cannot be included in the same family with *Phascolotherium* and *Amphilestes*, in which the three cusps are linear. May we ask him if he would, on the same grounds, propose to separate the existing Thylacine from the Dasyuridæ, as the difference is nearly the same in the one case as in the other? We are glad to see that the author supports, in the main, the doctrine of "tuberculism," even going so far as to derive the primitive tritubercular molar from the multitubercular type—a derivation which we are by no means sure that we are inclined to support. He totally rejects, however, the view that the so-called triconodont type of molar (that is to say, the one in which the three cusps are ranged in a linear series) is the primitive type from which the tritubercular molar was evolved. On the contrary, he considers that the triconodont is the derivative from the tritubercular. He, however, propounds this view of the specialisation of the former type as if it were a new one, instead of dating from 1887. Thus in that year Mr. Lydekker (*Cat. Foss. Mamm. Brit. Mus.*, pt. v., p. 257) wrote of *Triconodon*, "it appears to be a highly specialised primitive form, of which the lower true molars come nearest to those of *Thylacinus*, and bear the same relationship to those of *Dasyurus* as is presented by the inferior carnassial tooth of *Ictilyon* to that of *Canis*." We will not take leave of this valuable piece of work without remarking how very refreshing it is to see palæontological investigation finding place in a journal popularly supposed to be given up to the section-cutter and embryologist. We hope that a new era is being entered upon in British Palæontology, and that this publication marks a closer alliance between students of the living and students of the extinct.

WE are glad to hear that a fresh start has been made with the long-suspended and much-desired "Flora of Tropical Africa." The work was commenced by Professor Oliver, the late keeper of the

Kew Herbarium, more than five-and-twenty years ago, and three volumes were issued: vol. i. in 1868, vol. ii. in 1871, and vol. iii. in 1877. In the production of these volumes Professor Oliver had much assistance from other botanists; in fact, the last volume, which ends in the middle of the Gamopetalæ, is largely the work of Mr. Hiern. Since 1877 nothing has appeared, owing, says the *Kew Bulletin*, to the Herbarium staff being fully occupied in "keeping fresh accessions determined scientifically and incorporated," while "such extraneous aid as was available has been mostly absorbed in assisting Sir Joseph Hooker in his 'Flora of British India,' . . . and in other undertakings." Why so important a work as the "Flora of Tropical Africa" has had to await the pleasure of these other undertakings is not explained. Recent exploration and the development of commercial enterprise have at last suggested to the Government the desirability of pushing forward the work (in proof of which some three-year-old letters are quoted), a fresh start has been made, and vol. iv. is now "in active preparation." As German botanists are meanwhile rapidly describing all the novelties they can secure, it has been thought advisable to publish brief diagnoses of two new species in the *Bulletin* during the progress of the work, and the January and February numbers contain those of the orders Apocynaceæ, described by Dr. Stapf, and Gentianeæ, Borragineæ, Bignoniaceæ and Convolvulaceæ, by Mr. J. G. Baker, Professor Oliver's worthy successor.

OBITUARY.

SAMUEL WHITE BAKER.

BORN JUNE 8, 1821. DIED JANUARY 20, 1894.

SAMUEL WHITE BAKER was born in London, and was the eldest son of Samuel Baker of Lypiatt, Gloucestershire. He was educated at a private school, and afterwards went to Germany, where he studied engineering. As early as 1845 Baker went to Ceylon, chiefly for the purpose of elephant hunting. The reader of his books, however, will find that he had already taken an interest in geography, and before he left the island (1854) he had founded an agricultural settlement at Novera Elia, which he had peopled with English emigrants, and stocked with cattle mainly at his own cost.

From 1855 he superintended the construction of the Danube and Black Sea railways, and in 1861 made his first exploration in Africa, taking with him his second wife, Florence Finnian von Sass, a Hungarian lady, who has since shared all her husband's labours and enterprises. His journey had for object mainly the determination of the sources of the Nile, but partly the relief of Speke and Grant, who had left Zanzibar in 1860 with the same intent. Diverting his course in search of sport, he explored the Abyssinian rivers, and learned Arabic. Reaching Khartoum, he proceeded in 1862 southward to Gondokoro, where he met Speke and Grant in February 1863. Fired by their accounts, Baker and his wife determined to push still further, and on March 14, 1864, they reached the east side of the Albert Nyanza, and though unable to prove the exit of the White Nile, he crossed the river on his return journey about sixty miles to the north of the lake. For this adventurous journey, and its brilliant results the Geographical Society awarded Baker a gold medal. He was further created M.A. of Cambridge, and received the honour of knighthood. The story of his wanderings will be found in "The Albert Nyanza," 1866, and "The Nile Tributaries," 1867.

After a year's rest in England Baker and his wife returned in 1867 to Egypt, having in view the extinction of the slave trade. In 1868, the Khedive took Baker into his council, and it was decided that he should go, with a strong force, and absolute and supreme authority "to suppress the slave trade, to introduce a system of regular commerce, to open to navigation the great lakes of the Equator, and to establish a chain of military stations and commercial depôts distant at intervals of three days' march throughout Central Africa, accepting Gondokoro as the basis of operations." But nature and the natives were against him, and from June, 1870, to April, 1871, the expedition

suffered great hardships, and numbers died from malaria and other diseases. After reaching Gondokoro, the Bakers pushed on south, and towards the end of 1871 established a fortified camp on the Nile at Fatiko, from which he waged war unceasingly on the slave hunters, and for a time checked the trade. But so soon as he left the country the trade revived and the crusade was continued by Baker's successors. Baker and his wife returned to England in 1873, and told the story of their expedition in "Ismailïa" 1874.

Sir Samuel settled down for a while at Newton Abbot, but the old sportsman and traveller could not rest. He and his wife visited Cyprus in 1879, Syria, India, Japan, and America in later years.

His chief works, beside those referred to above, are:—"With Rifle and Hound in Ceylon," 1854; "Eight Years' Wanderings in Ceylon," 1855; "Cyprus as I saw it in 1879"; "Wild Beasts and their ways," 1890.

The cost of African exploration has been exceptionally heavy during the past month. Mr. Ingham has been killed by an elephant owing to the jambing of his rifle on the Congo, where he had done much good work. M. Parmentier, who for several years had been one of the Belgian officials on the Upper Congo, and who added greatly to our knowledge of its geography, has died at Nice; on the very day of his death the news arrived that three missionaries in Equatorial Africa had fallen victims to the climate. The unhealthiness of this region has been impressed on the public mind by the difficulties it is placing in the settlement of Uganda. By Sir Gerald Portal's death, the malarial dysentery of the Sana has robbed the nation of a public servant of exceptional promise; Mr. Rennell Rodd, who took his place at Zanzibar, has been invalided home and cannot return to Africa. Captain Besant who had started to fill the vacancy left by the death of Sir Gerald Portal's elder brother in Uganda, has collapsed on the way, and has been brought back to the coast by Dr. Charters of the mission station at Kibwezi. The news, however, that the daring young American explorer, Mr. Astor Chanler, has succeeded in reaching Mombasa is the one bright spot on the page of disaster. He started in September, 1892, on an expedition to Fort Kenia and Lake Rudolph. His colleague, Lieutenant von Höhnel, was wounded by a rhinoceros in the thigh and had to be sent back. Shortly afterwards eighty porters who had been sent up as reinforcements with extra stores left him and returned to the coast, and this was followed by a still heavier blow as over one hundred of his original force deserted and left him in Daicho with only eighteen men.

ALEXANDER THEODOR VON MIDDENDORFF.

Born 1815. Died 28 January, 1894.

Middendorff was born at St. Petersburg; he studied medicine at Dorpat, where he received his doctor's degree in 1837. He completed his studies in Germany, and in 1839 was appointed

Professor at the Vladimir University, Kieff. The following year he undertook, with von Baer, a voyage in the Arctic Seas and Lapland. Charged shortly after by the Academy of Sciences of St. Petersburg, he explored Northern Siberia, and made, between 1842–1845, a difficult and perilous journey. The results of his work were given in "Voyage dans l'extrême nord et dans l'est de la Siberie" (St. Petersburg, 1848). He added to this work another dealing with the natural history and physical conditions of Russia in Asia. Returning to St. Petersburg in 1845, he became privy councillor, and was made secretary of the Academy of Sciences from 1855–57. In 1859 he was President of the Society of Economics but retired soon after, on account of ill-health to Livonia.

JUSTUS KARL HASSKARL.

HASSKARL, the botanist, died on January 5 at Cleve in the Rhine Province. He was formerly co-director of the botanic garden at Buitenzorg, Java, where he introduced the Cinchona and for some time actively superintended its cultivation. The *Münchener Allgemeine Zeitung* in announcing his death, remarks that he died in moderate circumstances, although he had been instrumental in conferring on his fellow countrymen so great a boon. He was also the author of numerous papers dealing with the Flora of India and the Malay Archipelago.

THE McGill University, Montreal, has lost a valuable benefactor in the person of Mr. PETER REDPATH, who died last month at Chislehurst, Kent, at the age of 73. Until his retirement, Mr. Redpath followed the business of a sugar-refiner at Montreal. In 1880, he founded the Peter Redpath Museum, in the grounds of McGill University, the finest Natural History Museum in Canada; and last year he presented to the University a new spacious library, capable of holding 130,000 volumes.

WE are also informed of the death of Mr. CHARLES OTLEY GROOM, who later assumed the style of His Most Serene Highness the Prince of Mantua and Montferrat. Mr. Groom left a large and valuable collection of Natural History specimens, and we understand that the cabinets of fossils and minerals have been purchased by Mr. R. F. Damon, of Weymouth.

THE following deaths are also announced: Sir HARRY VERNEY, the Father of the Agricultural Society; General Sir C. P. BEAUCHAMP WALKER, Foreign Secretary to the Geographical Society; and EDMOND FRÉMY, Director of the Museum of Natural History at Paris.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

HERR HOLST, a German botanist, has been appointed to the German station at Kilima-njaro in East Tropical Africa.

Dr. W. MIGULA, docent in Botany and Bacteriology at the technical high-school at Karlsruhe, has been raised to the rank of Professor.

PROFESSOR ENGLER, the well-known director of the Berlin Botanic Garden, has been raised to the dignity of Geheim Regierungsrath.

GEHEIMRATH PROFESSOR PRINGSHEIM, another famous German botanist, has been appointed a Knight of the Order of Maximilian and an honorary member of the Petersburg Academy of Science.

DR. W. SAPOSCHNIKOFF becomes Professor of Botany at the University of Tomsk, in Siberia.

DR. G. F. STONE has been appointed assistant Professor of Botany at the Massachusetts Agricultural College at Amherst.

MR. ALBERT F. WOODS has been appointed assistant Pathologist in the Section of Vegetable Pathology of the Department of Agriculture in Washington.

MR. W. W. CLENDENIN, formerly an Assistant in the University of Missouri, has been appointed Professor of Geology and Botany at the University of Louisiana.

AT the University of Cagliari, in Sardinia, Dr. Dominico Lovisato has been appointed Professor of Botany and Keeper of the Botanical Garden, while Dr. Francisco Sanfelice has been nominated Professor of Hygiene.

DR. GIACOMO CATTERINA has been made Assistant in Zoology and Comparative Anatomy at the Naples University.

Dr. Zacharias, late Professor-extraordinarius in Strassburg, whose recent paper on the cells of the Cyanophyceæ is of so much interest, has been appointed Director of the Botanical Gardens at Hamburg. Dr. Zacharias is a native of Hamburg, and has now returned after 17 years' service in the famous Strassburg school under De Bary and Graf zu Solms-Laubach.

MR. E. CHAPMAN, who has for many years been science tutor at Magdalen College, Oxford, is leaving Oxford at the end of the summer term. Mr. R. T. Günther has been appointed his successor.

THE Linacre Professor of Comparative Anatomy in the University of Oxford, E. Ray Lankester, is republishing in one volume various studies made in the Morphological Laboratory since his election to the Professorship. We sincerely trust that the original place of publication and the original pagination of these papers will be fully quoted; otherwise the volume will please few people besides the Professor and his pupils. Professor Lankester has recently been removing a quantity of worthless stuffed specimens from the court of the Oxford Museum, with the intention of replacing them by decent specimens and anatomical preparations so arranged and labelled as to afford as much information as possible. This is only a continuation of many valuable improvements that the Linacre Professor has carried out during the last three years. When are similar improvements to be continued in the zoological galleries of our national museum?

THERE is now a Psycho-physical laboratory at Cambridge, and Dr. W. H. Rivers of St. John's College, is conducting practical work this term.

AT Cambridge University an important proposal has been made by the Council of the Senate, to establish two new degrees, Bachelor of Science and Bachelor of Letters, which may be conferred upon graduates of any recognised University. The conditions suggested are—matriculation; residence for three terms, the prosecution of advanced study in Cambridge; an original dissertation on some subject, literary or scientific, coming under the cognisance of one of the Special Boards of Studies. This lead is pretty certain to be followed by Oxford, where similar ideas have been in the air for some time. The scheme, if confirmed, will prove of great advantage to the Universities themselves, and to the advanced students who will then be attracted thither.

THE Library of the late Professor Milnes Marshall has been generously presented to Owens College, by his friends and executors. A committee has been appointed to establish some memorial of his work, and it is thought likely that some definite scheme for the keeping up of the library will be proposed.

THE Professorship of Zoology at the Owens College, Manchester, vacant through the death of Professor Marshall, has now been advertised. The income is calculated to amount to £750 per annum, and consists of a fixed stipend of £300, a fee of £50 for services rendered to the Museum, and various capitation fees. The Professor is required to instruct in Biology (Physiological and Zoological portion) and Zoology, including Comparative Embryology; to take charge of the Laboratory; to conduct certain special classes for teachers; to give scientific aid in the arrangement of the Zoological part of the Museum; and to deliver annually a short course of museum lectures or public demonstrations. Further, as he becomes a member of the Senate, he has certain duties to discharge in the College and in the Victoria University. He has two demonstrators to assist him in his work. Applications must be made to the Registrar, not later than April 3.

PROFESSOR ALEXANDER, who has succeeded Professor Adamson in the chair of Mental Philosophy at Owens College, Manchester, intends to offer opportunities for laboratory work in physiological psychology. Professor Alexander has been engaged in experimental work in Dr. Burdon Sanderson's laboratory at Oxford for a considerable time.

EDINBURGH University is about to add to its possessions M'Ewan Hall, built at a cost of more than £50,000, for graduation and other ceremonies; University Hall, erected through the efforts of Professor Patrick Geddes, as a hall of residence for men students; Masson Hall, about to be erected as a hall of residence for women students; a field near Corstorphine, purchased for £9,000, and fitted out at a cost of

£3,000 more, for athletic sports; and a chair of Public Health, for endowing which the late Mr. A. L. Bruce has left £5,000.

THE Milroy Lecturer to the Royal College of Physicians this year was Dr. J. Berry Haycraft. His lectures on "Weismannism, Disease, and Race Progress" were given in the Examination Hall at the Savoy on February 15, 20, and 22.

AT the annual meeting of the Board of Regents of the Smithsonian Institution, the secretary presented his annual report with a few remarks upon the enlarged activities of the Institution, referring especially to the work in physical science and to the increased number of correspondents of the Institution, now numbering about 24,000, more than half of them being without the United States, and scattered over the whole globe.

THE Ohio Academy of Science is organising a Natural History Survey of the State. Professors Claypole, Kellerman, and Kellicott have been appointed directors, their duty being to enlist and organise volunteers, to direct them in the choice of fields, and to aid them in the prosecution of their labours. Each worker will, however, independently present his results to the Academy, and be himself responsible for them.

A FREE course of lectures on General Archæology, entitled "Man before History," has been delivered by Dr. Daniel G. Brinton, at the Lecture Hall of the Academy of Natural Sciences of Philadelphia, during the winter session. "The Aims and Methods of Archæology" formed the subject of the first lecture; those of the second, third, fourth and fifth respectively were Africa, Asia, Europe, and America in the semi-historic and pre-historic periods; that of the sixth and final lecture, to be given on March 8, "The Island World in the Semi-historic and Pre-historic Periods," including geographical distribution. The detailed results of this suggestive series will, we trust, be eventually published in the same form as Dr. Brinton's previous course on "Races and Peoples."

THE Biological Station at Plon, which was started about a year ago for the special investigation of Fresh-water Biology, seems to be doing well. In the second part of its *Forschungsberichte* (Berlin, 1894), Dr. Zacharias gives a map of the East-Holstein fresh-water areas in the Plon district. Dr. W. Ule contributes a sketch of the geology of the district, and Dr. Krause a sketch of the botany. Graf Castracane and Professor Brun describe the diatomaceæ of the Plöner See, and Dr. Zacharias figures and describes ten new infusoria.

WE understand that "The British Institute of Preventive Medicine," to which we referred in our February number of last year, has acquired land for the erection of buildings close to Chelsea Bridge, from the Duke of Westminster. The committee have received £120,000 to enable them to carry out their scheme.

THE Woodwardian Museum, Cambridge, has recently been enriched by a large collection of fossils, mostly Cretaceous, the gift of the Rev. Professor T. Wiltshire.

THE Royal Botanic Society (London) is appealing to Government for a grant towards the support of their gardens in Regent's Park. The Society has done good work in the past in affording help, in the form of living specimens, to botanical students in the medical, pharmaceutical, and veterinary schools of the metropolis. We can ill afford to lose such institutions as the Regent's Park Garden or the Apothecaries' Garden at Chelsea; at the same time, they should be made as available as possible for the use of the interested public. There has been no doubt

a certain exclusiveness towards the public on the part of the Fellows of the Society now in question; and a Society which asks for public money and occupies a large space in a public park must afford simpler means of entrance to their domain than an order from a Fellow. A small fee for admission, following the example of their neighbours at the Zoological Gardens, would give people generally the opportunity they have a right to ask, and at the same time help to make up the £8,000 deficiency which the Fellows of the Society have now to meet.

THE Scottish Geographical Society has appointed a committee, consisting of Dr. John Murray, Professor James Geikie, Dr. Buchan, and Mr. J. G. Bartholomew, together with delegates from other scientific societies, to take steps towards the promotion of Antarctic exploration, and the *Scottish Geographical Magazine* for February contains two communications on the subject. In the first Mr. William S. Bruce, naturalist to the s.s. "Balæna," gives a short account of previous explorations; in the second Dr. C. W. Donald describes the Dundee Whaling Expedition of the summer of 1892-3. A landing was effected on the beach in the north-west of the Erebus and Terror Gulf, and the authorities all agreed that a party could spend a comfortable winter in such a spot as this. Dr. Donald ascertained also that it would be comparatively easy to reach the surface of the ice-cap from many of the landing-places in sight, on Louis Philippe Land as well as on Joinville Island. The finding of a practicable wintering place from which the surface of the ice-sheet would be accessible is an important advance in our knowledge, for abrupt cliffs of rock or ice, bound most other parts of the Antarctic continent. There are numerous notes on the character of the ice, and some illustrations; but little could be added during an expedition of this sort to our knowledge of the fauna and flora of the parts visited, and the notes made naturally refer mainly to the whales and seals. Among the birds only one not aquatic, the common hooded crow, was seen. The plants were all mosses and seaweeds. Some fossils are mentioned as having been obtained from Seymour Island, but it is not stated what they are. We believe, however, that land or fresh-water species are still quite unknown from the Antarctic regions, either living or in a fossil state, except the few cryptogams found on different occasions. If the Scottish Geographical Society continue their efforts, we may expect before long to learn something more about this mysterious land, and may ascertain whether it ever supported a fauna and flora as peculiar as that of the similarly isolated continent of Australia.

DR. HENRY WOODWARD, F.R.S., is the new President of the Geological Society of London, Mr. Richard Lydekker becomes a Vice-President, and Dr. J. Walter Gregory forms the new blood in the Council. The election took place on February 16, when the retiring president, Mr. W. H. Hudleston, F.R.S., read his address "On some recent work of the Geological Society," being a continuation of the subject dealt with by him on the occasion of the last annual meeting. The fellows and their friends dined together at the Criterion the same evening. The more reasonable charge of half-a-guinea for dinner tickets, initiated by Dr. Woodward, resulted in a better and more representative attendance. Speeches were made by Sir Douglas Galton, Sir Charles Wilson, Sir W. Flower, Sir Archibald Geikie, Dr. Hinde, Dr. Hicks, Dr. Woodward, Lieutenant-General McMahon, Mr. J. E. Marr, and others.

LIEUTENANT-GENERAL C. A. MCMAHON was elected President of the Geologists' Association (London) at the Annual General Meeting held on February 2. The retiring President, Mr. Horace B. Woodward, read an address "On Geology in the Field and in the Study," which will be printed in the "Proceedings."

A FEW months ago Mr. James H. Veitch exhibited before the fellows of the Linnean Society a number of interesting objects collected during his recent travels in Japan. He has now presented his extensive and varied collection to the Museum

of the Royal Gardens at Kew, and the January number of the *Kew Bulletin* supplies a few notes on some of the products, many of which are quite new to the Museum.

THE Eastbourne Natural History Society has issued its *Transactions* for the Session 1892–3. Among the various papers on subjects of such world-wide importance as are usually affected by our local societies, we note a list of the Marine Polyzoa of Eastbourne, by Dr. H. S. Gabbett, and some interesting notes by Mr. H. Michell-Whitley, the president, on recent archæological finds in the Eastbourne district. The Society numbers 62 members, including the Right Hon. T. H. Huxley.

THE Royal Zoological Society of Dublin has recently acquired by purchase a jaguar, a hairy tapir, a golden agouti, three tree ducks, two curassows, four crested quails, and two black parrots.

THE Geological Society of America held its sixth annual meeting at the end of December last, in Boston and Cambridge, Mass., under the Presidency of Sir J. W. Dawson. In his presidential address, Sir William discussed recent geological problems, especially those connected with pre-Cambrian rocks, mountain formation, the Glacial period, and the Deluge. Abstracts of many of the papers read will be found in the February number of *The American Geologist*. The next winter meeting is to be at Baltimore, when Professor T. C. Chamberlin will preside. We are informed that at the annual dinner "there was a continual flow of wit and good humour." This is probably to be accounted for by the fact that the remarks made by the fellows present were "brief."

AT the annual meeting of the Biological Society of Washington, the President, Professor C. V. Riley, entomologist of the U.S. Department of Agriculture, delivered an address on the Social Insects. "The instinctive and inevitable actions of insects are," he said, "associated with many others, which result from the possession of intelligence, of conscious reasoning, and reflective powers."

PARTICULARS of the sixth Geological Congress, which will be held at Zürich from August 20 to September 2, are to hand. The subscription will be twenty-five francs (£1), and should be sent to M. C. Escher-Hess, Bahnhofstrasse, Zürich. The Excursions are numerous and comprehensive, and for the convenience of our readers we append a brief list.

Those arranged before the session commences are as follow:—August 22 to 27, under the direction of M. Schardt—Geneva to Bellegarde; Châtillon-de-Michaille to Nantua; Charrix to Chesery; to Thoiry; to Gex; to Geneva (cost 50 frs.). August 23 to 27, under M. Jaccard—Portarlier to Vallorbes; to Yverdon; to Fleurier; to Neuchâtel; to Locle (cost 50 frs.). August 22 to 27, under M. Rollier—Porrentruy to Delemont (3 days in this district); to Saignelégier; to St. Imier; to Bienne (cost 60 frs.). August 22 to 26, under M Schmidt—Wiesenthal to Bâle (2 days); to Laufenberg; to Brugg; to Zürich (cost 60 frs.). August 24 to 28, under M. Mühlberg —Staffelegg to Olten; to Kirchzimmer; to Reigoldswil; to Aarau; to Zürich (cost 50 frs.).

Those arranged after the session:—September 3 to 13, under M. Heim—Zürich to Weissbad; Säntis; Amden; Obstalden; Linthal; Elm; Flims; Tenigerbad; Olivone; Bernardino; Lugano (cost 200 frs.). September 3 to 14, under M. Schmidt —Zürich to Schwytz; Wasen; Hospenthal; Ulrichen; Airolo; Fusio; Faido; Lugano (250 frs.). September 3 to 10, under M. Baltzer—Zürich to Innertkirchen; Gstellihorn; Guttannen; Hospice Grimsel; Hospenthal; Airolo; Lugano. September 3 to 15, under M. Schardt—Bulle to Albeuve; Rougemont; Châlet du Gros Jable; Ormont; Bex; Champéry; Sanlanfe; Tête-Noire; Champex; Bagnes Brigue; Domo d'Ossola; Lugano (150 frs.). Several other excursions have been

arranged, but full particulars of each and every excursion can be obtained from M. le Prof. E. Renevier, Lausanne, or MM. Ruffieux et Ruchonnet, Agence Suisse de Voyages, Lausanne.

THE Oxford University Junior Scientific Club announce that they will hold a conversazione at the University Museum on May 22, and that Professor Macalister of Cambridge will deliver the third "Robert Boyle Lecture" on 4th or 8th May, his subject being "Some Morphological Lessons taught by Human Variations." At the meeting of the Club on February 16, papers were read by Mr. M. S. Pembrey, on "Histology of Organs after Extirpation of the Thyroid Gland"; and Mr. F. Druce, on "Some Characteristics of Cyclonic Storms."

A LARGE quantity of natural history material, which was exhibited at Chicago, has been purchased on behalf of the city of Philadelphia, and it is proposed to erect a fresh building for its accommodation, instead of incorporating it with collections already in existence.

THE Crystal Palace has fallen from its high estate, and is associated in the minds of most people with no other science than that of pyrotechny. Still, however, it does something for the cause of natural history, and we learn from the report of the directors of the company that £300 *per annum* is spent on the natural history department, while £800 goes to the attendants in the Aquarium, and as food for fishes.

THE HAECKEL CELEBRATION (Special Account).

ON the 16th of February Ernst Haeckel completed the sixtieth year of his life. On the 17th, the little town of Jena, in whose University Haeckel is Professor of Zoology, was thronged by a great crowd of his friends, pupils and admirers, among whom may be specially mentioned the Hertwigs (Oscar and Richard), Waldeyer, Arnold Lang and Hermann Credner, besides many well known professors of Jena itself. The chief ceremony of the day was the uncovering of the marble bust of the great scientific worker and writer, from the chisel of the eminent sculptor, Professor Kopf, of Rome. At noon the lecture-theatre of the Zoological Institute, in which the greater part of Haeckel's life-work has been carried on, was crammed from floor to ceiling, and Professor R. Hertwig, of Munich, the pupil, friend and colleague of Haeckel, was called upon to unveil the bust. In an admirably-worded speech he alluded to the main facts of Haeckel's life, and especially to his labours in the cause of science and scientific freedom; of these facts we give a short account below. The unveiling of the striking bust was the signal for a great outburst of applause, and when this had subsided, a deputation from some societies, the Medicinish-Naturwissenschaftliche Gesellschaft of Jena and the Geographische Gesellschaft of Thüringen, offered to Professor Haeckel their honorary membership. They were followed by a deputation from the students, who expressed in enthusiastic terms their admiration and respect for the Professor of Zoology. Professor Max Fürbringer of Jena followed with details concerning the subscription to the bust, informing us that there had been nearly 700 subscribers, who sent their tokens of appreciation from all parts of the world; he especially alluded to the gratifying fact that many subscriptions had come from France. As a consequence of this, the total amount exceeded the cost of the bust by at least £300, and this sum he had pleasure in placing in the hands of Professor Haeckel, for him to devote to such purpose as he might think best in the interests of science.

After the ceremony, and after Professor Haeckel had, not without emotion, acknowledged the honours showered upon him, the elect among the visitors adjourned to a banquet in the Hotel zum Bären, where covers were laid for about 120 of both sexes. The day concluded with that characteristic German institution, a "Commers," in which almost all the students of Jena seemed to be taking part. Cheers for the Professor, songs and speeches in his honour, mingled with the clinking of glasses, enlivened the old University till a late hour of the night.

Ernst Haeckel, the second son of Ober-regierungsrath Carl Haeckel, was born in Potsdam on February 16, 1834. He was early led to the study of nature, especially of Botany, an inclination nourished by his mother, who came of the old legal family of Sethe, and by his father, who himself took a keen interest in Geography, and numbered among his friends the geographer Ritter and the African traveller Barth. He went to school at Merseburg, whither his father had removed, and continued his studies in natural science. At his father's instance, however, he decided on medicine rather than pure science, as affording a surer means of livelihood; at the same time he hoped to travel as a ship's doctor, and so to see distant lands. At the age of eighteen, then, he began his medical studies in Berlin, but seems to have been mostly attracted by the lectures of the botanist, Alexander Braun. From the autumn of 1852 till Easter of 1854 he attended the anatomical lectures of Kölliker and Leydig in Würzburg, where he made friends with Gegenbaur and the present Berlin physician, Gerhardt. Returning to Berlin, he went to the lectures of Johannes Müller, which greatly influenced him and determined the future course of his life. In his enthusiasm he spent all his spare time in natural history excursions in the neighbourhood, holding entirely aloof from the beer-drinking entertainments of his fellow-students. Autumn of 1854 saw him with Johannes Müller in Heligoland, where he obtained his first impression of the sea and of the pelagic fauna, from which sprung his predilection for researches on the "plankton" or floating tribes of the sea. While at Berlin, Haeckel made friends with Max Schultze, Claparède, von Richthofen, von Martens and others who have since attained to eminence. At Easter, 1855, he returned to the then famous medical school of Würzburg, where, in the summer of the following year he became assistant to Virchow, in whose *Archiv* he published one of his first independent works,—"Ueber die Plexus chorioides." In the autumn of 1856, together with Müller and Kölliker he visited the sea-fisheries of Nice, where he made his first acquaintance with the Radiolaria, and discovered the movement of cells in the connective tissue of Tunicates. His medical studies in Berlin were then resumed, and in March, 1857, he took his degree of Doctor of Medicine, presenting before Ehrenberg his Thesis "On the Tissues of the Crayfish." A summer in Vienna was divided between medical work and excursions in company with the botanist Focke from Bremen and the zoologist Krabbe from Copenhagen. Although in the following winter he passed the State Medical examination in Berlin, yet, at the instance of Johannes Müller, he determined henceforward to devote himself to Zoology. Unfortunately, his intention of working under Müller was thwarted by the sudden death of the latter at Easter, 1858. In 1859 he went to Italy and again studied the pelagic fauna of the Mediterranean, collecting materials for his Monograph on the Radiolaria, which appeared in 1862. At the suggestion of Gegenbaur, in 1861, he settled in Jena, where he was soon elected Professor of Zoology. In this quiet little town, amid beautiful and natural surroundings, he has stayed ever since, resisting pressing invitations to go to the larger Universities of Würzburg, Bonn, Vienna, and Strassburg.

A change now came. After a year and a half of happy married life, his wife, who was his cousin, Anna Sethe, died, and Haeckel drowned his sorrow in the most intense work. Darwin's "Origin of Species," which had appeared a few years before, had made a great impression on him. This he had already acknowledged in his Radiolarian monograph, and now he set about his important work "Generelle Morphologie," which appeared in 1866, and was the first book to view the animal kingdom in its entirety from the standpoint of the dual doctrine of descent and natural selection. This work completed, Haeckel went on a holiday to the Canary Islands, of which we have an account in his "Besteigung des Pic von Teneriffa." On his return through London he met Huxley, Lyell, and Darwin himself.

In 1868 appeared another book inspired by Darwinism, his "Natürliche Schöpfungsgeschichte," which in 1889 had reached its eighth edition, and been translated into twelve languages, including Malay and Japanese. This result was not, however, attained without gaining for the author many enemies, and converting several warm admirers into hot opponents. From these general works he turned again to mono-

graphs on particular groups, the Development of Siphonophora (1869), Studies on Monera and other Protista (1870), Monograph of the Calcareous Sponges (1872). The material for the latter work had been collected on excursions to Norway and Dalmatia, and in it is first found the Gastræa Theory, subsequently elaborated by him; according to which all many-celled animals are derived from and pass through in their development a primitive sac-like form, the Gastrula. In 1874 was published his "Anthropogenie," a popular exposition of the evolution of animals leading up to man. About this time Haeckel travelled a good deal; in 1873 to the Red Sea in 1875 to Corsica, and in 1877 to Corfu. In 1876 appeared his important paper on the Perigenesis of the Plastidule. In 1877 he delivered his address before the gathering of Naturalists in Munich, on the modern theory of development in its relation to general science. This produced a reply from Virchow which Haeckel answered in his pamphlet "Free Knowledge and Free Teaching," where he adopted as his motto, "Impavidi progrediamus." In 1881 appeared his Monograph of the Medusæ, and this was followed by a journey to Ceylon, which gave rise to his charming "Indische Reise-briefe." After this Haeckel came into closer connection with English scientific men, by the publication of his magnificent "Challenger" Reports on the Medusæ (1881); on the Radiolaria (1887); on the Siphonophora (1888), and on the Horny Sponges of the Deep Sea (1889). In 1890 he visited Algiers and Tunis, and in this year was published his "Plankton-Studien," which summed up his pelagic researches and stated his views as to the distribution of marine life in opposition to the views of Hensen and others. A continuation of these researches was made in 1892, around the Hebrides, in company with John Murray of the "Challenger," and in 1893, in the neighbourhood of Messina. Last year also he published his "Monism as a Link between Religion and Science."

It would be impossible here to enumerate the many eminent scientists who have been pupils of Haeckel, and who have raised the fame of the University of Jena. By his own fame, too, the University has been enriched, as in the case of the endowment of a Professorship and a travelling studentship by Dr. Paul von Ritter, and the bequest of the Countess Bose. Indeed his lucid and flowing style, his orderly and philosophic grasp of facts, have attracted many to his works who would otherwise have been repelled by their detail and technicality. In the broader aspects of his scientific work he has been as it were the complement of Darwin in the direction of Morphology and Embryology, somewhat as Herbert Spencer has been in Psychology and Sociology. His works are adorned by drawings from his own pencil, for he is an excellent draughtsman and water-colourist. Although sixty years of age, he has the strength, energy, and freshness of a much younger man, having been in earlier days a noted gymnast. In 1867 he took as his second wife the daughter of the anatomist Huschke, by whom he has one son and two daughters. Haeckel has never plunged far into the troublous sea of politics, but has reserved his energies for fighting his own battles, and they have not been few, and for advancing the standard of science, which he has done to no small extent.

IN connection with the P.N.E.U., the A.W.P.L. have arranged two courses of lectures on natural history and geology, which are being given at the British Museum (Natural History). The first course, on the Domesticated Animal, from which men were excluded, was delivered by Miss Goodrich Freer on Saturday afternoons during February. The place chosen for the lecture was that very popular gallery of the Geological Department of the British Museum in which the fossil Ammonites are exhibited.

The next course will be given by Miss Whitley on the mornings of March 2, 9, 14, and 16. The subject is Field Work in relation to Practical Geology, and the lecture hall is to be the adjoining public gallery of the same department (Members meet near the Mastodon). After one hour's talking, the lecturer will give demonstrations in the Museum, which, it is well known, contains a large number of exhibits relating to practical geology (or, are we confusing it with the other establishment at Jermyn Street?); anyway there are some fine blocks of fossiliferous stone on which the members can practise field work.

CORRESPONDENCE.

Scientific Volapük.

The following, written nearly seventy-five years ago by Thomas Jefferson, one of the signers of the Declaration of American Independence, and sometime President of the United States, seems rather to the point now that new words are so much to the front:—"I am a friend to *neology*. It is the only way to give to a language copiousness and euphony. Without it we should still be held to the vocabulary of Alfred or of Ulphilas, and held to their state of science also, for I am sure they had no words which could have conveyed the ideas of oxigen, cotyledons, zoophytes, magnetism, electricity, hyaline, and thousands of others expressing ideas not then existing, nor of possible communication in the state of their language. . . . . Dictionaries are but the depositaries of words already legitimated by usage. Society is the workshop in which new ones are elaborated. When an individual uses a new word, if ill formed, it is rejected in society; if well formed, adopted, and after due time laid up in the depositary of dictionaries. And if, in this process of sound neologisation, our trans-Atlantic brethren shall not choose to accompany us, we may furnish, after the Ionians, a second example of a colonial dialect improving on its primitive." — ("Correspondence of Thomas Jefferson," vol. iv., pp. 339, 340. Monticello, Aug. 15, 1820. Letter to John Adams.)

Nearly three-quarters of a century afterwards *our* American brethren still hold the lead. But words must not be condemned because they are new. Some of the new words, like "topotype" in your list, supply a decided want. New words again, are certainly preferable to old ones distorted. It must be easier for a beginner to learn a new word than to remember that "period" and "age" in a geological paper each mean very different things, according to whether they are in inverted commas or not, and in some cases they occur in the same page, each with their different meanings. "Age" has been strongly condemned by a President of the Geological Society in his address (W. T. Blanford, 1889, p. 37).

Another advantage of new words is that they often emphasise a distinction which is apt to be otherwise overlooked. It ought to be impossible for a warm discussion to arise betwen consentients, as I have seen, over a remark—"the older shells of —— are more plicate"; because one took "older" in a geological sense, another as referring to the age of the individual, a third to the age of the race. It is a climax of absurdity that the "older" shells geologically are the "younger" biologically. It suggests that geological strata should be called "earlier" and "later," never "older" and "younger"; but, again, "earlier" and "later" are used in an "evolutionary" (!) sense, and they are not necessarily equal to "primitive" and "more specialised."

S. S. Buckman.

Climatic Conditions and Life.

With reference to the remarks made on p. 89, no. 24, of Natural Science (Feb., 1893), regarding a suggestion of mine as to former climatic conditions—

I.—May I be permitted to point out that so far as I know there is no *limit* to the extent of the areas over which these phenomena (fumaroles or hot-springs) might be manifested.

A whole continent might, in former times, have been covered with them and the

character of its vegetation and animal life determined by them—though it was set itself in a frozen sea. Such an extreme case is, I think, a possibility.

In India where we have flows of trap or lava, altogether covering an area of 200,000 square miles, we learn to look at such questions from a somewhat liberal scale of measurement.

II.—Chemical deposits are by no means invariably formed by hot springs. And even in those cases where they are formed they are not always of so stable a character as to leave permanent traces.

Still it may be the case that beds of limestone occurring in association with such fossils as I have referred to may really have been derived from the calcareous deposits (travertine) of hot springs.

III.—It is scarcely needful to add that fossil localities often occupy very narrow and restricted areas.

Science and Art Museum, Dublin, V. BALL.
February 12, 1894.

THE GEOLOGICAL SOCIETY'S JOURNAL.

IN the "News of Universities," NAT. SCI., vol. iii., p. 470, reference is made to the printing of the date of issue of scientific publications side by side with the signatures. Allow me to point out that the editor of the *Quarterly Journal of the Geological Society* has this year initiated an improvement even on this, the date of issue being printed at the top of every open two pages, half on each side of the fold. C. D. S.

Cephalaspis IN THE CAITHNESS FLAGS.

IN reference to the recent announcement of the discovery of the "typical Lower Devonian genus *Cephalaspis*" near Thurso (NAT. SCI., vol. iii., p. 255), it should be remembered that this fish occurs in the Upper Devonian of Scaumenac Bay, Canada (see *Geol. Mag.*, 1890, p. 15). We must not, therefore, be surprised to find it in the allied fauna of "Lake Orcady."

Junagadh, Kathiawad, India. J. W. EVANS.

TO CORRESPONDENTS.

REV. S. BARBER.—We are obliged for your communication. Meteorology does not especially concern us; but you will find a mention of the storm of July 8, 1893, on p. 395 of our third volume.

RECORDER.—The investments of the Geological Society of London for the past four years are:—£420 (1890); £506 (1891); £528 (1892); £502 (1893). The total value of the invested funds is, according to the latest balance-sheet of the Society, £10,729 11s.

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NATURAL SCIENCE:

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NOTES AND COMMENTS.

Independence in Scientific Journalism.

MR. ROBERT LOUIS STEVENSON, in his delightful "Letter to a Young Gentleman who proposes to embrace the Career of Art," insists that it is "far more necessary that a man should support his family than that he should attain to—or preserve—distinction in the arts." So also in journalism, it is the first business of a journal to prove the utility of its existence by paying its way. The splendid memoirs of endowed institutions are necessary and honourable, the periodical caprices of the wealthy are amiable follies, but neither are journalism. On either side the path of a scientific journal are many pitfalls. Mere popularity may be lucrative but is a descent from science, mere technical science for the most part requires external financing. But there are pitfalls more perilous because less apparent. Suppose the necessary condition of a journal be attained—that it can pay its way—the next thing is for it to take a strong, fearless, independent line on all questions affecting its subject-matter. It must distribute praise and blame; it must criticise freely where criticism may lead to improvement; it must attempt in every way to serve the best interest of science. For these purposes, especially in matters of blame, the most competent and complete information is necessary.

Now, in England the number of those who are competent to deal efficiently with all matters concerning the advancement of science is strictly limited. Necessarily, most of them are connected with the Universities, great societies, and institutions of this country—connected, in fact, with precisely those social mechanisms the working of which must be passed in review. Natural Science, therefore, believes that in the best interests of science and of individuals a certain part of its columns must be open to anonymous criticism. It is the editorial business to distinguish between anonymity and irresponsibility. In many matters this distinction does not exist: fortunately, in scientific matters the degree of reasonableness of any criticisms is

patent to all whom they concern. We shall continue to welcome all comment that is useful, and to publish it in whatever way seems most useful. We are concerned with science and not with individuals; it is our business to distinguish between merely personal criticism and criticism for the advantage of science. So far as we succeed in this we are confident of securing the approbation of all workers: if ever we fail, or seem to fail, we shall receive gladly advice and correction.

New Journals.

The stream of new periodicals is this month increased in volume by two dealing with Natural Science. The first of these is not strictly a new journal, though practically it is so. *Science Gossip* is reappearing under "entirely new management," and seemingly with somewhat different aims. The present month has also witnessed the birth of *Science Progress*, a "monthly review of current scientific investigation." It is conducted by Mr. H. C. Burdett, of Stock Exchange and hospital fame, and it may be purchased for half-a-crown. Mr. Burdett is assisted by Professor Farmer, who is editor-in-chief, and a number of other gentlemen, who form an editorial committee. He has also secured as "contributors to the earlier numbers" Professors Burdon Sanderson, Dunstan, Ray Lankester, and others. The committee states that "in no case will the articles be of a popular character, although it is intended that they shall be intelligible to persons possessing an ordinary scientific training." The first number contains 104 pages, distributed among seven articles, dealing with "Physical Science and its Connections," "The New Theory of Solutions," "Insular Floras," "Fossil Plants," "Bacterial Poisons," "Vertebrate Morphology," "Chemical Physiology." These appear to be excellent *résumés* of current research, and cover a wide field.

Memorial Volumes.

The multiplication of periodicals not directly containing the results of individual research leads us to the consideration of a type of publication which is both interesting and annoying. Journals, popular or otherwise, which do not affect to contain actually new work, have a notable and valuable place in the hierarchy of scientific literature. But the publication of actual research concerns very practically all those who are engaged on research. The confusion of tongues and the laudable enthusiasm of governments and institutions already render patience a necessary equipment of the Zoologist or Palæontologist. With care, access to many libraries, and perusal of Records, it is nearly possible for him to keep abreast with what appears in the ordinary publications. But there have of late been published a good many volumes, we cannot call them books, of a strangely composite nature, their subjects merely united by the desire to do honour to

some teacher or school, or to glorify some particular professor. As instances we may mention the Macleay Memorial volume (NATURAL SCIENCE, vol. iv., p. 141), the volume just issued by the Linacre Professor of Comparative Anatomy (NATURAL SCIENCE, vol. iv., p. 232), and the Wilder Quarter-Century Book, which latter is a collection of papers dedicated to Professor Burt Green Wilder, at the close of his twenty-fifth year of service in Cornell University, by some of his former students. In some cases the papers contained in the volume have been previously published; in others they appear to be something that the authors wished to get out at the moment and for which they have availed themselves of the excellent opportunity presented under a laudable appearance of hero-worship. But in all cases the connection between the papers is of the slenderest. Now it is not our wish to detract from the honour that should be paid to great men, nor to do anything but praise the papers themselves; but we do submit that this mode of publication is one of the very worst that could possibly be devised, and is by consequence no excellent way of showing admiration. Take the case when the matter is new. The volume is probably too expensive for any but rich libraries or institutions to buy; it therefore does not readily fall in the way of the ordinary student; it is cumbrous and not easily broken up, so that one cannot buy the part that one wants; and, in fact, it combines all the disadvantages of a serial with none of its advantages. It would be more to the advantage of readers, and therefore of writers, that these papers should be issued through some of the ordinary channels of publication, and thus find their way readily into the hands of those who take an interest in the various subjects. When the matter is merely republished, of course, these objections do not apply to so great an extent. But another danger presents itself; namely, that the original pagination is often ignored and even the first place of publication not quoted. Whether this is done from ignorance or vanity the result is to cause much vexation and unnecessary labour to all who have to refer to these volumes, and it is probable that the language used about the person the volume is supposed to honour will not always be of a benedictory nature. Hence we wish to suggest that some other means, more consonant with the needs of workers, should be found for doing honour to those who are thought to require it.

A NEW LEMUR.

TURNING from the consideration of publications to the consideration of their contents, we must call attention to a new form of Lemur, named by Dr. Forsyth Major *Megaladapis*, recently (*Phil. Trans.*, 1894) described by him from a marsh at Amboulisatra in Madagascar. The animal appears from its skull—the only part known—to have been a giant among Lemurs; it had in all probability the bulk of four cats. The size, however, did not indicate anything but a comparatively low position among Lemurs.

It seems that this genus does not bring us anywhere near to the ancestral Lemuroid. Dr. Forsyth Major is of opinion that it was a highly-specialised type, mainly by reason of its tritubercular molars and its small brain. The tritubercular molar has been held by Osborn and by others to be the mark of the primitive Mammalia; but the author of the paper before us thinks differently. In Madagascar this kind of molar occurs in the Viverridæ (Carnivora), Centetidæ (Insectivora), and Lemuridæ. This of itself points to convergence, for the most bigoted adherent of "trituberculism" would hardly insist upon a very close resemblance between these three families. Moreover, each of these three families shows members in which the reduction of the cusps has gone further. The genera in which this occurs show other evidence of degeneration. It is an interesting question whether this ancient and extinct Lemur was contemporaneous with man, as was possible in the case of the Æpyornis found associated with it. The skull examined by Forsyth Major was scratched, as are the bones of the bird, but whether this scratching is due to a sharp-pointed instrument of human manufacture or to the teeth of a carnivorous neighbour appears to be uncertain.

Carboniferous Insects.

We learn from a letter in the *American Journal of Science* (February, 1894), written by Dr. Samuel H. Scudder from Paris, that we may shortly expect Mr. C. Brongniart's work on the fossil insects of the Commentry coal. Glimpses of this extremely rich fauna have from time to time been given to the student, and considerable interest naturally attaches to the complete record. The fact that mining at Commentry is carried on in the open air offers the most favourable opportunity for recognising and preserving the fossils. Dr. Scudder writes: "I have had the opportunity of seeing not only a considerable part of the collection, but also the illustrations prepared by M. Brongniart himself from the choicest specimens; illustrations made with a care and exactitude which leave nothing to be revised. . . . Leaving the cockroaches out of account, to which M. Brongniart will give his attention later, the number of these illustrations, their variety, the extraordinary character of the insects themselves and their rare perfection, leave not the least room for doubt that when his work appears, our knowledge of palæozoic insects will have been increased three- or four-fold at a single stroke, and an entirely new point of departure for the future opened. No former contribution in this field can in any way compare with it, nor even all former contributions taken together. Besides it will offer such a striking series of strange forms as cannot fail to awaken the attention of the least incurious. One may not enter into details, but mention may simply be made of one species, regarded by M. Brongniart as one of the forerunners of the dragon-flies, in which the wings have an expanse of

considerably more than two feet (or about 70 centimetres), and of which several specimens are preserved. It is a veritable giant among insects."

Pedigree Wheat.

A well-known artist, on being asked by an amateur how he mixed his colours, replied that he mixed them with brains. Fortunately for the languishing pursuit of agriculture, there is some prospect of a larger admixture of brains than was necessary when farm produce was protected by duties on importations. The live-stock of England has become famous all over the world because of careful selection by breeders. As Mr. Rendle shows, in his article on the Cross-fertilisation of Food Plants in this issue of Natural Science, as much may be done for food plants as has been done for domesticated animals. Unfortunately, many plants, like fruit trees and potatoes, have been propagated so entirely from cuttings that the necessity of growing from seeds has been lost sight of. But it seems a general law of the organic world that sexual reproduction, securing as it does the advantages of cross-fertilisation and the admixture of strains, is necessary, at least occasionally, to secure the best and hardiest offspring. But in all breeding from seeds careful selection is absolutely necessary. The most expensive tillage, the most lavish manuring, and the blandest of seasons are as naught compared with the selection of seeds from the best plants, and the skilful choice of plants for cross-fertilisation. Animals and plants are born differently, and by careful attention to these natural variations almost anything can be done in a few generations. England is a country well adapted by its range of climate and of natural conditions, and by the high state of cultivation of its soil, for experimental seed-growing, and there is no reason why the pedigree wheats of England should not command the markets of the world just as the pedigree horses of England are sought far and near. The range of our land is not wide enough to yield the enormous crops of Russia, America, and Canada, but we might well provide the best seeds for these larger areas. For this change of industry scientific knowledge is requisite. The councils of many English counties are providing agricultural scholarships to be held at scientific institutions. We hope that the councils will see that their scholars are chosen from among those likely to engage in the practice of agriculture, and that they receive their technical training at institutions where scientific knowledge is combined with enlightened practice. There is still a future for England, but it is a future depending more on the brains of her sons than on the natural fertility of her soil.

An Ammonite Problem Solved.

It is a well-known fact that the various species of Ammonites have a very wide distribution over the earth's surface, and that they

succeed one another in the rocks in the same order though in regions far apart. This fact, though extremely important, is in itself not particularly remarkable, since there are other groups of animals whose species have a similarly wide extension in space and a narrow limit in time. The truly remarkable thing is that the same species often occurs in rocks of a very different character, being found in limestones, clays, or sandstones. This may be expressed by saying that the lithological sequence bears no relation to the faunal sequence; and the apparent conclusion is that the evolution of Ammonites was in no way affected by the environment. Taking their stand on some such facts as these, some have even gone so far as to suppose that the rise and fall of any genetic series was an independent phenomenon, comparable to the life of an individual. This may be, but is it so certain that the facts of Ammonite distribution have any bearing on the question?

This question has recently been discussed by Dr. Johannes Walther in the second volume of his "Einleitung in die Geologie als historische Wissenschaft," a volume which deals with the mode of life of marine animals, especially such as are of geological importance. Without offering any opinion as to the zoological affinities of the Ammonites, Dr. Walther points out that their shells, which are practically the only portion with which the geologist has to deal, are paralleled by the shells of only two genera among recent Cephalopods, namely, *Nautilus* and *Spirula*, which, though differing in every feature of morphological import, yet resemble one another as well as the Ammonites in the possession of a coiled, chambered shell. Now the living *Nautilus* and *Spirula* both live at the bottom of the sea and are very restricted in their distribution; yet their dead shells are found over an enormous area. "In the mangrove swamps of Java, on the sandy shores of Ramesveram, among the volcanic ashes of the Canaries, and on the coral-reefs of the tropical zone; everywhere are the shells of *Spirula* found scattered. No coastal deposit in the whole of the Indo-Pacific province is free from *Nautilus*." In these respects *Nautilus* and *Spirula* differ from all other living Cephalopods, even from the female Argonauta, whose shell, though outwardly similar, is devoid of chambers; and just in these respects do they resemble the extinct Ammonites.

Dr. Walther comes, therefore, to the following conclusions: The distribution of Cephalopod shells provided with air chambers bears no relation to the habits of the living animal. The richness of a deposit in chambered Cephalopod shells does not depend on the distribution or habits of the living animal. The form of a chambered Cephalopod-shell allows us to frame no safe conclusion as to the organisation of the animal. The distribution of chambered Cephalopod-shells bears no relation to the changing character of the containing rock or to the depth of the sea in which it was deposited. Applying these conclusions to the particular case of the Ammonites,

we see that their shells may safely be taken as zonal indices, since they mark truly contemporary divisions of the earth's crust wheresoever they occur; while, since their distribution must have taken place after the death of the animals, they afford us no evidence as to the environing conditions that may or may not have governed their evolution.

Molluscan Mimics.

Our ignorance as to the mode of life of extinct mollusca often lands us in great difficulties, and problems continually occur of which the solution can never be more than guessed at, although, could we have but a glimpse of the once living animals, our questions might receive an obvious and immediate answer. The curious resemblances between species of totally different genera, may be explained in more than one way, perhaps by action of similar environment, perhaps in obedience to dimly-perceived laws of growth; but it is not often that we apply to them the explanation afforded by the biological phenomenon known as "Mimicry," not at least when we are dealing with fossils. A paper recently read by Mr. A. H. Cooke before the Cambridge Philosophical Society seems to show that we might occasionally do worse than venture this answer to the riddle. He reminded us that the shells of *Strombus mauritianus* and *S. luhuanus* differed from those of all other *Strombi* in their close resemblance to the shell of *Conus*, a genus with which they were known to live. He pointed out that *Strombus* was a vegetable-eater, with small weak teeth, while *Conus* was a flesh-eater, with large barbed teeth, provided with a poison bag and duct. *Conus*, then, would naturally be avoided by predatory fish, and any resemblance to so dangerous a genus would be of great advantage to *Strombus*. In the case of extinct mollusca, a similar explanation might occasionally be possible, but, of course, only when the species in question occur in the same locality and bed.

Chaos in Science Teaching in London.

Some time ago (Natural Science, vol. iii., p. 297), in referring a second time to the clever anonymous pamphlet on the Organisation of Science, we urged that the existence of competitive journals and rival societies secured freedom and prevented novel views being suppressed, merely on account of their unorthodoxy. To a certain extent the competition and rivalry among teaching bodies in London secures flexibility and range in science teaching, and the present chaos of institutions, which has come about by natural growth, has its good side. On the other hand, much money and time is wasted by unnecessary multiplication of teaching plant; there is no coördination by which various institutions can as a right utilise each other's advantages. The great libraries, museums, and teaching institutions,

are totally unconnected with each other; only by private courtesy or as members of the general public can those working at one institution gain access to another.

From the point of view of a student the case is still harder. He must throw in his lot with one definite institution, and though his education is at least as expensive, his opportunities, considered all round, compare most unfavourably with the opportunities in the great provincial towns, and at the Universities of Scotland and Ireland. In the case of medical students the disadvantages of present arrangements are peculiarly great. The foremost men in different branches of the profession are scattered among the hospitals, and a student at one must be content with mediocre lectures on many of his subjects, although a few streets off the foremost authorities in Europe may be discoursing on them. Moreover, two students at different institutions pursue nearly the same course, and at the end of their career receive the one, an University degree, the other, a mere qualification.

Report of the Gresham University Commissioners.

We hope that the report of this second Commission will prove a great step towards the removal of some of these anomalies. We do not propose to discuss it in a controversial spirit, but to note its main provisions.

The Commissioners recommend first that there should be one University in London; that its teaching functions should be confined to the metropolis, but that it should continue to be an examining body for students presenting themselves from all parts of the British Empire. To prevent a repetition of the old delays, it is recommended that the necessary modifications of the existing University should be made by legislation, and not by charter, as that would entail the action of the present University itself.

In the matter of examinations for degrees, the Commissioners propose that the certificates of institutions other than the University shall be accepted in lieu of the first examination for degrees, but that the final examinations for degrees shall be the same for internal and external candidates. This they propose, having in mind the necessity of the same standard for the same degree; but as in the reconstituted University the teachers will be a very large body with very great powers, they believe that the present unsatisfactory system, by which teaching is wholly subservient to examinations, will be remedied.

The University is to include six faculties—arts, science, medicine, law, theology, and music. The faculty of science is to include pure science and applied science under different boards of study. The latter is to include engineering, architecture, agriculture, and other subjects of technology.

A University Board is to encourage University extension, and if it be satisfied that any portion of its work is of equal educational value, is to accept it as a portion of the University course.

University College, King's College, the Royal College of Science, the Medical Schools of the Great Hospitals, the London School of Medicine for Women, the City and Guilds of London Institute, and Bedford College are to be among the Schools of the University, and their teachers, when individually approved by the University, are to be members of its faculties.

Great additional facilities for research are to be provided, and these, though not definitely associated with the teaching institutions, are to be open to all teachers of the University.

A senate consisting of a Chancellor and sixty-five other members, of which eighteen will be elected by the faculties, is to be the supreme governing body of the University. Then in order of authority follow an academic council elected by the faculties; the faculties, consisting of teachers appointed by, or recognised by, the Universities; the boards of studies; and convocation.

The Commission finally recommend that a statutory commission be appointed with power (subject to the approval of Parliament) to carry out and give conclusive authority to the recommendations of the report.

We must welcome this report as a solid, fair, and practicable attempt to evolve out of the chaos of London institutions an University worthy of the metropolis.

THE NEW BIOLOGICAL SYLLABUS OF THE CONJOINT BOARD.

THE medical schools of London form one of the most important sets of institutions affected by the Gresham University. For some time, biological teaching at them was in abeyance, save at those large enough to have a fair annual number of students going up for a London degree. Three years ago, the Conjoint Board of the Royal Colleges of Physicians and Surgeons, the Board which gives their qualification to the largest number of medical students in England, wisely instituted an elementary course of Biology, as part of the necessary curriculum. The first schedule was cumbrous and peculiar. It included elementary botany, the differences between plants and animals, a number of types of parasitic worms, the characters of the vertebrates, the difference between Man and the Mammalia generally. The syllabus was neither educational, easy to teach, nor harmonious with the existing courses of biology. A revised syllabus of a much better kind has been issued recently. The theoretical work is associated with a definite series of types, which serve to illustrate the main features of ascending grades of structure. It can be taught along with the course for the London "preliminary scientific," although in certain respects the London syllabus is more advanced.

But a great deal has to be done to prevent waste of energy in the

teaching of science at the London schools. The new university will have, as one of its most difficult tasks, the concentration of teaching in a smaller number of institutions without prejudice to the independence and friendly rivalry of the various hospitals. We suggest that the solution will be found on lines similar to the combined lecture courses in successful operation among groups of colleges at Oxford and Cambridge.

Science Teaching at Secondary Schools—The New Commission.

Those who are accustomed to teach at medical schools, or at colleges and universities, must be struck with the lamentable ignorance and want of method of the ordinary pupils from secondary schools. At some of the larger public schools science teaching is admirably conducted, and those at least who give themselves up to the modern and scientific sides are well prepared for advanced science teaching or for the technical training of the medical schools; but in scientific training the majority of endowed and intermediate schools are no whit better than, as Sir Philip Magnus pointed out in 1876, they were at the time of the Commission of that year. Some students have a scrappy acquaintance with elementary facts, but they are ignorant of the methods of studying nature, and they are totally untrained in observation and incapable of describing on paper the simplest object set before them. In this respect, as in many others, endowed and secondary schools stand in marked contrast to the public elementary schools of this country. Anyone who has the slightest acquaintance with pupils from the two classes must have been impressed by this. Now that the movement in favour of technical instruction has become an actual force, the distinction between the education of the lower classes and the education of the upper and middle classes will become still more greatly in favour of the lower classes. Most of the County Councils of England are now offering higher scholarships for various purposes which will enable a greater number of elementary scholars to reach the higher scientific professions; and, unless great improvements are made rapidly in secondary and endowed schools, not only will the path of the most promising children from the largest section of the community pass by these institutions, but, in the competition of life, pupils of the higher schools will be beaten hopelessly. Boys from elementary schools will pass by scholarships direct to technical institutions, and from these by higher scholarships to the universities, medical schools, and engineering institutes. Going by these routes, they will not be subjected to that tone and leisurely culture which is the boast of endowed schools; but none the less will they succeed in the battle of life, and the total result to the community will be, taking for granted the pretensions of the endowed schools, an absence of culture and refinement from many of the most useful and successful members.

of the higher professions. If for these reasons only, we welcome the new Commission on Secondary Education. But although it contains many good names, it is specially weak on the side of the natural sciences. Science is represented on it by Mrs. Bryant, D.Sc., Dr. R. Wormell, Sir Henry Roscoe. We should like to have seen some representatives of the scientific teaching of Agriculture, of Medicine; some representatives of pure sciences like Biology and Geology.

Lord Playfair on University Extension.

Addressing students connected with the London Society for the Extension of University Teaching (on March 10), Lord Playfair paid a wise and eloquent tribute to the activity and utility of University extension. He regarded the hostile evidence given before the Royal Commission on the University of London as having arisen from a misunderstanding of the origin and purposes of extension. Up to the last quarter of the eighteenth century the working classes were separated by an impassable barrier from the learned class, for these spoke in a language the others could not understand. For about two thousand years Latin was the learned tongue, and for about two centuries Greek was raised as a second wall of separation between the learned and the people. University extension was an attempt to extend the higher knowledge to the people who are unable to attend courses during the day. Its main purpose was not to educate the masses, but to permeate them with the desire for intellectual improvement, and to show them methods by which they could attain this desire.

So far as extension limits itself to these great objects it must receive the approval of fair-minded people. But extension covers a multitude of sins, although it is only fair to say that the London Association is less a sinner than either Oxford or Cambridge. In the vast population of London it is possible to get together audiences who will prefer the competent expert to the mere lecturer. But in the provinces it is not so, and the lecturers of the Oxford and Cambridge centres are for the most part successful and popular in the inverse ratio of their special knowledge of their subjects. With this in itself we have no quarrel. An evangelist of the Gospel requires no Hebrew or even Greek. A lecturer whose business is to arouse the people, not to educate them, need be no profound thinker or learned scholar. But, at least on its present lines, the University Extension system is unadapted to the teaching of those who are themselves to teach, and against its pretensions to recognition from Government, or from bodies granting degrees, an emphatic protest must be made. For, unfortunately, on every side we hear it urged by the official representatives of the movement that a great educational work among teachers is being done. Teachers, like others, need stimulation and awakening, but stimulation and awakening are quite other than professional education. The theatrical methods of

the public lecturer are the worst example for the trainers of youth. The London Association has some courses specially adapted for teachers: the Cambridge sequences are useful for teachers occasionally: the Oxford courses are least useful. Courses adapted to the mixed audiences of the provinces have no *direct* educational value, and should carry with them no certificates of any value as part of the professional training of teachers. The best training for these must come from those associated with the actual acquisition of new knowledge rather than with the popularising of the results of new knowledge. The advantage of knowledge to the community is not that it is interesting, but that it is useful: useful in the objective sense, that it is the means by which the forces of nature are bound to the service of man: useful in the subjective sense, that it is the means by which the human mind is deepened and widened. The adult population of the community who have found their places in the ranks of workers have earned a right to be interested and brightened, and the great task of humanising their leisure is the task of the University Extension. But children have to find their useful place in the world, and it is above all things necessary that their teachers should be taught by those who actually have to do with fresh acquisitions and fresh applications of knowledge.

Summer Meetings and the Training of Teachers.

On the other hand, University Extension Associations have arranged, in connection with their summer meetings, courses of the highest value for teachers. In the dog-days the great laboratories stand for the most part idle: no teaching is going on, and the devotees of research are taking holiday. At Oxford, at Cambridge, and at Edinburgh, practical courses for teachers, sometimes in connection with County Councils, sometimes not, have been arranged. These courses, where most successful, have been limited in sphere. An amount of ground that can be covered in four hours' practical work, preceded by an hour's demonstration-lecture each day for three weeks, gives teachers accustomed to mere book-work an insight altogether new into the meaning of scientific work. The afternoons can be occupied by excursions either purely for pleasure or bearing on geology, natural history, and agriculture.

We think that much might be done in this respect in London. We have here many laboratories that stand idle for considerable times during summer; there is no difficulty in getting competent teachers of practical work, and in the elementary schools of London and of the neighbouring districts there are hundreds of masters and mistresses who would rejoice to make use of any opportunities provided them. We commend this suggestion to the London Association and to the Technical Instruction Committee of the London County Council.

Closely connected in purpose with University Extension Associations are two bodies which, so far as we know, did not put forward a claim to be considered in connection with the all-embracing Gresham University.

The P.N.E.U.

A note in our last number (p. 238) has, we regret to find, mystified several of our readers, who thought we were starting an Agony Column, and who have been racking their brains to discover the hidden meaning of P.N.E.U. and A.W.P.L. We hasten to dispel their deplorable ignorance.

The P.N.E.U., we beg to inform them, stands for the Parents' National Educational Union, an association intended to educate parents how to educate their children. Parentship is not, we believe, a necessary qualification for membership; still, many of the members appear to be parents, and are presumably married. Remembering, however, an old saying, we are not surprised to see that many of the lecturers on other people's children have not, apparently, entered either of those blissful states. We ourselves have lately pointed out how much education our educators need (Natural Science, vol. iv., 181), and from what we have learned of the work of this association, we are inclined to welcome its endeavours. We understand that there are agencies of the Union in various towns of England, but the leading branch appears to be that of Belgravia, which has for its honorary secretary the Lady Isabel Margesson. This is no impertinent patronising philanthropy that forces its way into the homes of the poor with a tract in one hand and a scented handkerchief in the other; on the contrary, it attacks our great under-educated classes themselves in the gilded ignorance of their own drawing-rooms. Possibly even these parents will resent the intrusion, and ring for the footman to show the P.N.E.U. downstairs; for these parents bring their children up by second-hand. First the nurses (of various denominations), then the governess, then the schoolmaster; these are all, as Professor J. R. Seeley has called them, "professional parents," usurping all the more important functions of both father and mother. They do their best, let us admit; but under this system true education, as opposed to mere instruction, languishes, parents fall into disrepute, sons go to the devil, and daughters revolt. The Parents' Union would remedy this. "It strives," to quote its own leaflet, "to show parents that they cannot, by money payments, divest themselves of their responsibilities. It endeavours to supply them with knowledge and training for their task, to impress on them the absolute necessity for giving careful thought and study to the subject of education, and to show them that without wisdom and knowledge parental love will be maimed, and unable to rise to its true perfection."

The Union uses many means to attain this admirable end. A

few parents in any district take counsel together, and associating themselves with those who can assist in the study of child-nature and training, discuss and invite lectures on various problems and difficulties that present themselves. Libraries are started, training lessons are given, natural history clubs are formed. Many other agencies will arise as time goes on.

The educational advantages to be gained from the study of natural history are recognised by the parents who belong to the Union; who also recognise that children will not, as a rule, become keen about natural history, unless their parents and teachers themselves show an interest in it. But the poor parents were not taught how to study natural science in their youth, and now they do not know how to set about learning or how to be enthusiasts in an unfamiliar subject. Fearful of being found out, the poor papas and dear mammas of the Belgravia branch started a Natural History Club not quite a year ago. This Club is distinctly practical in its working, and those who join are required to do some field-work every summer. An Exhibition of the collections made during the first summer was held some time ago, and was very favourably reported on by Mr. E. Sykes, the Secretary of the Malacological Society. The Club also sold to its members a very suggestive Handbook, drawn up by the Lady Isab[illegible] Margesson and Miss Vintner. Encouraged by their success, th[illegible] propose to issue shortly a more extensive Handbook, to which chapte[illegible] are contributed by some dozen writers well-known in their own lines. This work will be published by Messrs. George Philip & Son, 32 Fleet Street, and should prove useful to others than the benighted parent.

THE A.W.P.L.

THESE letters stand for the Association of Women Pioneer Lecturers. The objects of this Association beautifully combine egoism and altruism. First, it endeavours to open up a new field of work for women and to organise women. Secondly, this work is the extension of science and culture to places and people of all classes not reached by the University Extension lecturers, and the pioneering the way for those lecturers. A representative of NATURAL SCIENCE, who attended the annual meeting of the Association at University Hall, Gordon Square, on March 7, gathered the following information. The Association was founded by Miss Edith Bradley about a year and a half ago, and as it has not yet succeeded in paying its way, is kept going largely by the exertions of that lady. A woman who wishes to be a Pioneer Lecturer is required to prepare a syllabus of ten lectures and then to deliver one of them as a test lecture. Great attention is paid to proper delivery, and those who do not come up to the standard have to attend elocution classes. The lecturer is also desired to "cultivate a graceful deportment and to appear in a suitable and becoming costume." The staff of lecturers at present numbers twenty-four, of whom eleven are University women that have

obtained honours. The number is increasing, but unfortunately the supply already appears to exceed the demand. If only 300 kind people would subscribe a guinea for three years, they would be allowed to attend *all* the lectures and the Association would be greatly benefited. But would the subscribers?

Our readers will be interested to hear that no less than nine ladies are anxious to pioneer them through the difficult paths of science, teaching them science—moral and otherwise, physiology with or without hygiene; in fact, everything from primitive man to all the duties of housekeeper and mother. Some of these scientific ladies have names not unknown to us; but we confess we should tremble to place ourselves in the hands of one who would lecture us from Greek literature, through the Poetry of the Century (Browning, of course, to be treated as an *extra*), down to the Vertebrated Mammal. Clearly "you pays your money and you takes your choice"; or you don't do either. The science lectures do not seem to have been very popular; at certain lectures given at South Kensington the average attendance was as follows: Geology, six; Animals, six; Botany, ten. The lectures in connection with the P.N.E.U., to which we alluded in our last number, were, however, better attended, and it is intended to follow them up with a course of lectures on Zoology for children.

We wish the A.W.P.L. all success; but we must point out one very serious omission from their lecture list. They give no instruction in the elements of humour. But possibly they think example is better than precept.

Science at the Free Libraries.

A remarkable suggestion which bears upon the instruction of the masses is made by Mr. John T. Carrington in the excellent first number of *Science Gossip*. As a result of a recent tour through the metropolitan libraries, he points out striking deficiencies in them. Illustrated books on popular natural history are to be found in many, but these, for the most part, are out of date and of no educational value. In most cases the income of the library goes in the purchase of fiction, or in general expenses, and the librarian depends on donations for the science section of his catalogue, and must accept whatever comes to hand. He makes the valuable suggestion, that some competent authority should invite the councils of the various learned societies to draw up and revise periodically a list of text-books and authorities dealing with their especial subjects. Although we consider this suggestion most valuable, we do not think it practicable. Members of the councils of learned bodies are not, we fear, likely, as such, to give the necessary advice periodically and satisfactorily. For one thing, most of them are not specially familiar with text-books. Proper advice could be attained more easily by appeal to the teachers of different subjects at the various scientific institutions of London. But Mr. Carrington has made out so excel-

lent a case for the establishment of some kind of guidance, that we hope he will succeed, through the medium of *Science Gossip*, in arousing the necessary attention of authorities.

The Darwin Memorial.

Last month we commented on the praiseworthy efforts of Shrewsbury to do honour to Charles Darwin, perhaps the greatest name associated with that beautiful city. Apparently dogmatic folly is still raising its bray against science. A correspondent writes to us:—

"A prophet has lifted up his voice against the iniquity of the people of Shrewsbury in venturing to propose a memorial to Darwin in his native town. A sign has been sent from heaven, and the spire of St. Mary's Church has been blown down. The righteous then are punished for the sins of the ungodly. The punishment would have been more *ad rem* had it fallen on the Shrewsbury Museum or the mayor's house; but 'the wind bloweth where it listeth.' It is not for wicked people like ourselves to deny the logic of the Reverend N. Poyntz, but he seems to have a marvellous short memory. Are we not right in saying that about a year ago some stones of this same spire fell down, a fact which might have suggested even to the vicar the advisability of initiating some repairs? Really the supposition most charitable to the common sense, if not to the ingenuousness, of Mr. Poyntz is that, knowing himself to be in the wrong, he endeavours to distract attention by casting the blame on those who could have had least to do with this regrettable disaster."

I.

Plateau Man in Kent.

THE further back we push our studies of man, the plainer it becomes that a more comprehensive and at the same time more simple definition of the word "anthropology" must be allowed than is sometimes given. Anthropology is not a science of structure and function simply, but of man as an object of natural history, so that man's ontology and phylogeny must be studied like that of any other animal. Unfortunately, we are so prejudiced by preconceived ideas as to his dropping from the clouds in a highly civilised state, that anything claiming more than a very limited amount of development for him is ill-received. Anthropology has thus been but grudgingly allowed to embrace even Palæolithic man; and all sorts of theories have been invented, in the face of hard facts, to minimise the number of centuries that have passed since Palæolithic times. We have still with us the venerable champion of Palæolithic man—Professor Prestwich, who almost single-handed fought for our progenitors in the valley of the Somme. Since those days, however, this science has made remarkable strides: not only has it made its strongholds impregnable, but it has found it necessary in many parts to extend the dominion of the genus *Homo* away into the geologic past to a degree that was previously undreamed of. Nor has this been done at the expense of the State or by official observers. Month by month, year by year, a body of workers has been plodding along in various parts, gaining here a little and there a little, to which a new importance has occasionally been given by the more detailed work of the Geological Survey. In our own Thames area we have had workers like General Pitt Rivers, Messrs. Spurrell, Worthington Smith, Allen Brown, Greenhill, and Shrubsole, to whose names may be added the less known but none the less deserving one of H. Lewis. Then we have had collectors and philosophers in wider fields, such as Sir John Evans, Sir John Lubbock, and several others. During this time it has been conceded that a division might be made in the science, under the head of Prehistoric Anthropology or Prehistoric Archæology. So long as the study is confined to Neolithic, or even later Palæolithic man, these terms might be allowed to stand; for, so long as our discoveries are confined to river-drifts some hundred feet above present water-levels, it is just possible that no great changes in the surface have

taken place, such as we should require any great amount of geological knowledge to understand. But recent discoveries show that the science does not begin with the archæologist, but with the geologist and the palæontologist: when we come to deposits upon existing watersheds, or others which bear no relation to them, or yet others 400 or 500 feet above these, it is obvious that the subject has entered the domain of Geology. For the principal of these latter discoveries, especially in Kent, we are indebted to the undaunted energy of Benjamin Harrison, a tradesman in the little village of Ightham, between Maidstone and Sevenoaks.

It is now over thirty years since this indefatigable observer began making a collection. From time to time the echoes of the great thought-movements of the day reached this sequestered little village, but they were to Harrison as handbills dropped from a balloon; he was entirely shut out from the scientific world. His struggles and his perseverance; his fighting against want of encouragement and sleepless nights; his early risings and tramps to some spot four or five miles away, so as to be there at sunrise, and to hunt before opening his shop: all these are matters to be read with a relish only when the hero is no more. But, however Harrison's labours might have suffered from want of sympathy, they were soon rewarded by interesting finds. The house of his ancestors happened to stand in the old Valley of the Shode; relics of Palæolithic man found not far from his own shop soon whetted his appetite, and stimulated his researches. He carefully searched almost every inch of the adjoining country in the Holmesdale Valley, adding, as he went along, not only to his finds, but bit by bit to his store of knowledge, until he had managed to pull himself abreast of many of the scientific opinions of the day. Then he chanced to come upon a very beautiful palæolith (Fig. 1), some 70 ft. above the bed of the Shode, which set his mind at work in another direction, and one in which it must be conceded he received but little encouragement for a long time. From the first Harrison had kept a strict account of his finds, numbered them, sketched them into books, marked the position of their occurrence on the 6-inch map of the district, and made notes upon them as they presented themselves to his mind. As he contemplated the skilful work of this weapon, the question suggested itself—"Is it possible that this represents man's first essays at flint working? If so, what a dexterous creature he must have been born; in fact, he must have dropped from the clouds nearly perfect. But is it likely? Is it at all probable that man's evolution began with so highly-worked a flint weapon?" To which his reason answered: "No! If, then, this is not one of the first, where must I look for his earlier attempts, but to the more unwasted conditions, to the gravel patches lying above the present watersheds?" Nor were his labours in these places in vain, for he soon found a number of implements, which, in point of workmanship, were inferior to the majority of those of the valley, and, therefore,—from an evolu-

tionist's point of view—might be considered to be older. There was one thing, however, that hardly fell in with his expectations, and that was, that in the valley there were implements of a type identical with those on the hills. But when he came to compare these two groups, he found that those of the hills were fresh and somewhat sharp, while those of the same type in the valley were, without exception, more or less water-worn. Here, then, was the explanation. The implements of this character found in the valleys were derived from the hills; and this idea also explained the existence in the valleys of several other things, such as erratics foreign to the locality, and types of implements still rougher than those of the Hill-men.

But now the physical features of the country perplexed him. He had evidently got to something older than the oldest existing vestige on the Counterscarp; and the question was, where to look for the earlier home of man, in a period before the genesis of this last physical feature, and when the land-surface was probably 700 feet higher than at present. This surface, said he, must have been continuous with the chalk plateau; and upon the elevated ground of the latter, in its most unwasted parts, we must look to find the earliest traces of man in our district. Then commenced an inch-by-inch survey of the whole plateau for a distance of eight or ten miles; and here his labours were rewarded by considerable finds, at first, perhaps, a little disappointing. He soon discovered that here were to be found some implements exactly similar to the later palæoliths of the Holmesdale valley. His ruder forms were also represented, especially in the deep valleys which cut into the plateau, while the "old brown" deeply-stained flints occasionally found in the valley below were now found in great profusion all over the surface, especially in some places where the land was higher and had been curiously less denuded. Unfortunately, however, not a single section was to be seen upon the whole of the interesting area. Careful searching of the surface, of slight excavations for mangold trenches, of holes for trees and posts, and the deepenings of dew-ponds, and here and there of a well-section, not only showed the existence of the "old brown" flints, quartzites, cherts, and other erratics, but revealed the remarkable fact that the former had been picked up and worked perhaps on one edge, used, sharp edges being abraded in the using, then thrown down again; and further that all this had taken place before the flint entered into the remarkable deposit which so altered the surface of the stone, and changed its colour to that characteristic dark brown.

Here Harrison was sure he had evidence of an earlier form of culture or even of intelligence. The tools were *used* tools rather than *shaped implements*. But from the want of sections and invisibility of beds in juxtaposition it is at present impossible to state positively the exact age of the deposit in which these tools received their colouring. Sometimes they are Eocene pebble flints split by frost,

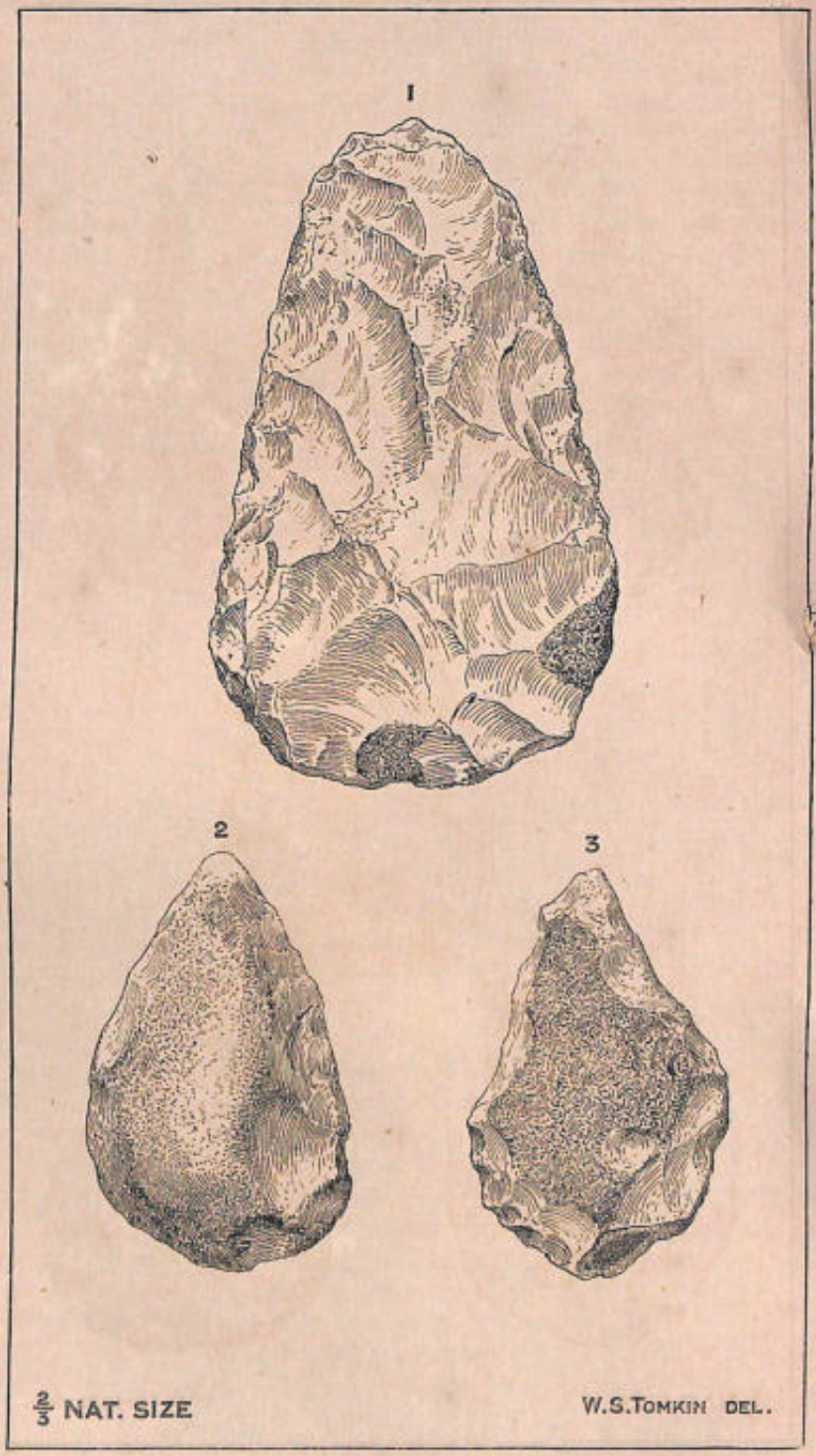
and worked from one side only, at others they are nearly whole flints picked up promiscuously, the working appearing always on the opposite side to a good hand grip. There are no oval hammer-stones such as were used in Palæolithic times, and consequently bulbs of percussion on flakes are very rare. Frequently the flints are striated exactly like some of those in the glacial beds of East Anglia. From these rude used flints numerous groups of implements diverge towards the well-known types of the Palæolithic and Neolithic ages. Just as our palæontologists saw the progenitors of our present ungulates in very dissimilar Eocene forms before the connecting links were discovered, so Harrison saw in his early finds the prototypes of later implements. For years he sought to make his finds known to the scientific world, but his converts were few. However, as time went by, spurred on by his own assurance and the incredulity of others, he increased his collection amazingly. He also distinguished groups of different forms, some of which were most curious. Form, however, was nothing to Plateau man; two objects alone presented themselves to his intelligence, a hand-grip, and a usable edge. Nor was he as yet the victim of one-handedness, left-handed forms being almost or quite as numerous as right. In all his early workings he worked from *one* side of the flint, which he did sometimes with one hand and sometimes with the other, as is evinced by the reverse workings.

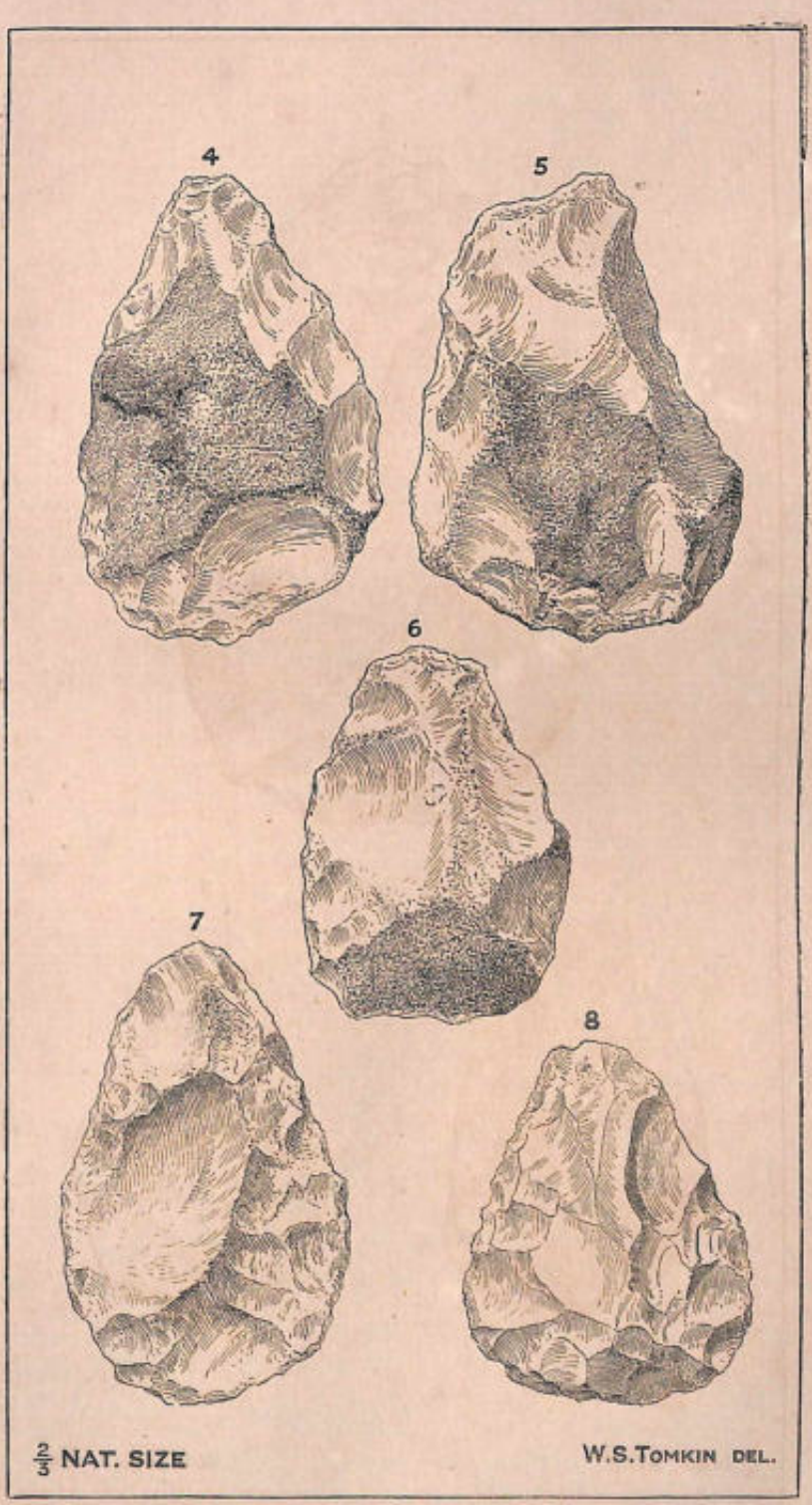
While collecting this material Harrison was brought into contact with many well-known scientific men. On one occasion he was introduced to a celebrated archæologist bearing a name classic in geology, who, as Harrison began to speak of Palæolithic man, blurted out in his characteristic way, "Of no interest; prove nothing; in fact prehistoric archæology is played out; my old friend Boucher de Perthes proved all that could be proved." Rather disheartening this, but perhaps better than the action of another set of critics, who, while admitting some of the better finished weapons when shown separately, assumed a position of open hostility to the subject both in season and out of season. Among the various scientists who encouraged him mention might be made of Messrs. Montgomery Bell, F. C. J. Spurrell, and Grant Allen, Dr. A. R. Wallace, and Dr. James Geikie.

In the meantime Harrison had been continually in touch with Professor Prestwich for several years; and as soon as the Professor's leisure would allow he traversed the whole or principal parts of the scene of Harrison's discoveries under his guidance, and from time to time studied these new evidences of a greater antiquity of man. To those who know Professor Prestwich's conservative tendencies and his predisposition to close in the period during which man has been on earth, it will at once be clear how strong must have been the evidence for him to turn round and become the champion of the cause. We well remember the eventful evenings at the Geological Society and the Anthropological Institute on the occasions of the reading of the

Professor's papers on this subject, when the old adage "history repeats itself" was once more verified; for assuredly the Somme finds were never scouted with such contempt as were the poor plateau tools. It is, perhaps, only right to say that we were among the majority. We remember well the impression made upon us by a specimen, which the Professor afterwards figured (*Quart. Journ. Geol. Soc.*, vol. xlvii., pl. viii., fig. 4); but even from this we admit we were obliged to withhold our assent, and we should think none the less of any who did the same in the absence of seeing a series of these things. Fig. 3 shows this to be simply a naturally broken piece of flint with the bark on one side, the right top edge being also a natural break, while any of the flakes struck from the other part are such as might, under exceptional circumstances, be produced in the vicissitudes of gravel making. But, upon closer examination, we notice that the whole of the left edge is worked from the flat side, and that it was formed by some score of blows. Moreover, we see that the resulting edge, till it reaches the butt, lies all in the same plane, and is at a constant angle to the flat side, which implies that every blow must have been administered at the same angle to the surface struck. Anyone who has had a few years' practice in the working of flint would readily admit that this flint was held in the right hand and the blows administered with the left. Further, when we come to the butt, we find that a slight twist exists in the flint, and, accommodating it to the grip of the hand, changes the angle at which the blows were received; consequently the rest of the flakes came off at a lower angle. Now arises the question of mathematical probabilities and the possibilities of "Nature" working in this manner. Could natural forces have administered all these blows on one side, at each time in the right place, when the whole surface of the flint was exposed, and on each occasion at the right angle although there were 180 degrees to choose from? Could natural forces have maintained the blows at a constant angle from the point of the implement till the butt was reached, and then have maintained them at another angle all round the butt? Would all the blows have been struck from the flat side, except when a prominence occurred, and have then been struck from the other side? Finally, would they have attacked the projecting ridge on the top right edge, with the result of turning out a usable implement, however rough? The improbability of such a coincidence of natural blows is so extreme, that though we may regard the shape of this stone as three-parts the work of Nature, we are obliged to ascribe the remaining part to the agency of man.

That Nature suggested this outline to man, or, in other words, that man discovered it nearly or quite at hand, we have not the slightest doubt. In Fig. 2 we have an example with even less work, but when placed with the others, its position in the series is evident. We have but little faith as a rule in illustrations of these rude forms; the specimens must be *seen*, and the physical properties of flint, the laws


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⅔ NAT. SIZE
W.S.TOMKIN DEL.


4
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⅔ NAT. SIZE
W.S.TOMKIN DEL.

of force, and the vicissitudes of gravel making, thoroughly understood. In this tool a half pebble is again employed, and from the flat side the whole of the right edge is worked, being struck at a very low angle and made to correspond with the opposite edge. And here again, as in the preceding, the flakes from the point to the commencement of the butt are in the same plane, or at the same angle; and with the change of grip to operate upon the butt, the angle becomes correspondingly changed. A few flakes removed from the left edge, extending about half of its entire length, complete the bilateral symmetry. In Fig. 4 we have got a step further, or, judging by the specimens before us, several steps. Nature, however, still claims the greater part of the work. The large flake from the base is due to the action of frost, but antedates the work on the edge, so that it is practically worked on one side for seven-eighths of the edge, the blows being delivered in different zones and at different angles. On the other side Nature claims a greater share, about one-quarter of the surface being occupied by the natural bark, and about another third by frost-bite; but in this we are introduced to workings on both sides of the edges, which mark a decided advance upon the early workings from one side only. Fig. 5 shows a still further advance; a great deal of the natural bark is still left, but there are decided attempts towards a thickening of the butt, and a tapering from above and below towards the point. Unfortunately, the ancient artisan found he had got too much material to finish with ease and did not quite understand the angle at which flint breaks; but he had the right idea in his head, and probably was far more successful than in this case before he gave up implement making. In Fig. 6 we have a greater advance; the bark is still retained for the base, but the latter is well thickened. Most of the blows, however, were delivered from one side, giving rise to a plano-convexity. This is about the quality of skill achieved by the Hill-men. Fig. 7 marks a still further advance, being chipped all over, and is only one of many specimens which are found in the hill group and occasionally in the valleys.

In selecting from Harrison's collection a half-dozen specimens to illustrate the whole evolution of a type, we are aware of the great gaps that exist between each specimen figured, and of this we are more conscious from the large number of specimens presenting those intermediate characteristics now before us. We cannot expect to prove the evolution, but merely to show the relation of one form to the other. Study of the specimens, however, shows clearly enough that we have here an unbroken sequence of development. In some cases, such as hollow scrapers and bone-splitters, quality of work and condition of material alone separate the Plateau from the Neolithic forms. There are numerous other extensive groups into which the Plateau tools can be divided, in which the archæan character is only surpassed by their constancy of form and recurring numbers. What the uses of some of these tools could have been is as great a mystery

as would have been a fossil boomerang had its use died out with Palæolithic man.

Coming now to the reality of Plateau man, the question, to our mind, is a very simple one. It matters not to us whether many of the cherished treasures of Benjamin Harrison are the work of Nature. We are certain that very many hundreds of them are not. Even if of the 2,625 collected, the odd five can be shown to be the work of man, and their stratigraphical position established, then Plateau man becomes a real being, to whom the modern world was first introduced by the Ightham shopkeeper. At the time of Professor Prestwich's paper the horizon of these implements was not susceptible of exact definition; since then a number of important finds have been made and facts discovered which will go a very long way to settle the question of age. But we dare not enter upon this question now, although we hope to do so shortly, and to prove Plateau man to have been Pliocene.

There is also another great work which Harrison has accomplished at the suggestion of Professor Prestwich since the papers of the latter on this subject. At the date of these papers, the Hill-men were not clearly defined, and implements found at certain heights were associated with them. This group of implements was first noticed by Mr. F. C. J. Spurrell, and he called them the Cave Group ("Palæolithic Implements found in Kent," *Archæologia Cantiana*, 1883). But, so far as we remember, Mr. Allen Brown was the only one to notice that they were of far more modern facies than those of the Hill-men. These have since been proved to have been the work of the Rock-shelter men, and for the purpose of making the necessary excavations the British Association have made grants of money to Mr. Harrison. Anyone who visits this spot will admire the skill displayed in his mode of procedure. Since the occupation of these shelters, the configuration of the country has been so altered that the ground just in front of them, which one would naturally expect to prove fruitful, was not worth working; but, seeing an unwasted ridge about two hundred yards off, Harrison successfully excavated it, obtaining some fifty implements and six hundred flakes. On this ground, and round about the shelters, he had found remains for many years. The implements of this period constitute a distinct group in which the work is of the highest quality ever reached before Neolithic times, indeed the skill displayed in one of the specimens is so great that, when first submitted to Professor Prestwich, despite its shape and altered surface, he was doubtful as to its claim to be called Palæolithic. Many years ago a superb weapon was found in a garden near Sevenoaks Station, beautifully polished all over, which in outline was exactly similar to one of Harrison's finds drawn out half as long again. One of Harrison's specimens, of the Rock-shelter group, was illustrated by Mr. Spurrell (*loc. cit.*). Especial reference may be made to this, as it helps to

bridge the gap between Palæolithic and Neolithic implements. It has usually been considered characteristic of Palæolithic implements that they were used on the point as thrusting tools, and that the broad cutting edge was an essentially Neolithic feature. But in this implement there is a soft spot near the point, and through this a hole has been somewhat irregularly drilled. A point intended for use would hardly have been so weakened; the hole, however, might have served to fasten the flint into a haft of some kind, thus leaving the broad end for a cutting edge, as in Neolithic weapons.

These indefatigable labours of Harrison have thus established the existence in the neighbourhood of five distinct stone periods, although not necessarily separated by a great hiatus, each characterised by special groups of implements. They are as follows:—(*a*) The usual neoliths on the surface; (*b*) The superior late palæoliths of the Rock-shelter or Cave-men; (*c*) The ordinary river drift types of existing valleys and watersheds; (*d*) The Hill group, above existing watersheds; and (*e*) The Plateau group which antedate the present structure of the Weald. Of course, it is only natural to expect to find the implements of all the later periods in various positions in the valleys of the plateau. One thing, however, of extreme importance remains, and that is, that there are large tracts of plateau drift from which all traces of deposits of Palæolithic age have been entirely removed, if indeed they ever existed on them; and in these patches the old Plateau forms are found, to the total exclusion of the Palæolithic types, although the most diligent search has been made for the latter, both by Mr. Harrison and other workers. This disposes of the weightiest argument against the separation of the two races, namely, the idea that the old rude Plateau specimens were the rough work of Palæolithic man dropped upon the surface. It is greatly to be regretted that the necessary funds are not forthcoming to carry out a thorough system of excavations upon the plateau, and to trace the relations of this drift to the undoubted Pliocene beds of the neighbourhood, which we are quite sure would confirm Mr. Harrison's discoveries.

W. J. Lewis Abbott.

II.

Characters in Biology.

IN a recent controversy as to the Factors of Organic Evolution, as well as in other biological writings, many difficulties have been raised, and many phenomena have remained inexplicable, because of fallacious reasoning that occupied itself with terms which have been inaccurately defined, and which mean much less than they are taken to mean. There are several prominent instances of this fallacy, notably in connection with the words *adaptation*, *purposeful* or *zweckmässig*, *variation*, *acquired*, and others; but by far the most remarkable case of the uncritical use of an ill-defined term may be observed in most [illegible] the speculative writings where the word *character* is prominent, and ev[illegible]e same is true of the German *Eigenschaft*, or *Merkmal*. The frequent term *hereditary tendency*, used as it is in a sense not greatly differing from the word *character*, is also a great source of confusion, and needs the same examination as does the latter.

"*Character*" appears on every other page of our biological works, and is a term we could not well do without. It has always had so obvious a meaning, and is now from long use so familiar to the ear, that it is difficult always to keep in mind what the limitations of the term should be. This character is, or is not, inherited; that other tends to vary: these are plain and intelligible phrases, and are apparently incapable of introducing serious error when used in the study of organisms. But as a matter of fact the term is extremely wide; it may, indeed, include almost anything, yet it is often used as if it had a definite meaning, or answered to real distinctions of parts or relations of the organism, or as if a *character* could justly be regarded as a unit in the study of heredity. The characters of an organism may be new or atavistic or neither; acquired or congenital; individual, specific, or generic; quantitative or qualitative; of a part, of an organ, of a function, or of a relation; or even the negation of any of these; and, yet, utterly incoördinate as they are in kind and amount, utterly arbitrary and changing as are their limitations, they are constantly regarded as units, not only rightly, to the observer, but wrongly, to the organism; are talked of as being present or absent, or as varying in one way or another without affecting the rest of the body, and as only occasionally being connected with one another so as to show a relation of interdependence. Even where

the idea is not so definitely formulated as in de Vries' chapter on "the mutual independence of the hereditary characters," we are constantly met with the assumption that the aspects of the organism, or of its parts, looked at from a hundred different points of view, and selected for consideration in a rich variety of ways, have a special significance as units for the study of inheritance, being treated of as independent of one another in heredity and variation, and often, for that reason chiefly, being regarded as represented in the germ by independent and special material vehicles. A dog, for instance, according to the view of that writer, inherits some of his characters from his mother, some from his father. Some come from a remote ancestor, and others are quite new. One of the parents may have had more to do with him than the other, for in the list of characters it may be that there are ten from the father and only seven from the mother. In most cases the characters are not the mean between those of his parents, they do not even vary all in one direction. There is no obvious interdependence between them. They are, therefore, according to de Vries, to be referred to different material foundations in the germ, derived from various ancestors, which *Anlagen* may vary independently of one another, each working out the character it represents. There are material vehicles for each of his characters, as, among others, for the length of his legs, for his swiftness, for his bark, for his love of sugar and his fear of snakes, as well as for the absence of all that he is not. All these are hereditary characters, varying to all outward seeming independently of one another, and all due to the ultimate *Anlagen*. Now, while few go so far in isolating characters as de Vries does, yet their independence of one another and their unity in themselves is so generally accepted that it is worth while studying what is the principle on which phenomena are grouped together and called a character, thereafter to be treated of as unit, and on what principle they are grouped to form two or more characters, varying independently or in correlation with one another.

The term may, in the first place, be used generally, of the whole creature as individual, with or without any special reference as of size, colour, habit, etc.; and this is generally Weismann's use of the term. Here there is no artificial division of the organism into parts that have no answering physiological divisions. In agreement with this point of view, Weismann's determining foundations in the germ answer each of them to the complete character of one individual ancestor, which is impressed on the whole of the new organism by the little piece of matter in charge of that character and by the little pieces derived from it, whereas those of de Vries answer each of them to one artificially limited character of many ancestors, which aspect of the new organism they look after, neglecting the rest of the body. As Weismann often, however, uses the term character in its narrower sense, he is led into the common fallacies at times. Indeed, an

animal is generally thought of and observed in some special direction, with the result that some appearances soon eclipse the others and come to be regarded as the characters *par excellence* of the form. Anything particularly striking to the eye or fancy at once assumes undue importance and becomes marked off as a unit. And still more often, those characters which are of the highest definitive significance, and consequently, as a rule, of the lowest physiological importance, those which are most "characteristic," take a more prominent position in our minds than others, quite rightly for some purposes, but wrongly for the study of inheritance. For thus one aspect of the organism comes to stand for all.

But, as a rule, characters are quite freely regarded as separate from one another and as independent of the rest of the organism, and are formed arbitrarily and used singly in the observation of the facts of inheritance. The animal is made up of, or at least capable of definite analysis into, separate incoördinate individual appearances, the presence or absence of which, in various proportion, determines its specific and individual relations. And in marking off characters from one another, one principle is always present, though loosely adhered to; that is—that those appearances which usually vary without any obvious accompanying change in the other relations of the organism, are included in one term. Thus a patch of colour is one character, being present or absent from animals otherwise not different. Any relation of paired parts is always called one character, not two, because such always bear a fixed relation to one another. An instinct or habit is a character, being for us irreducible to its complex causes. But any phenomenon for which we can find a cause, or even a distant causal relation in another, is grouped with the latter as one character. Wherever our knowledge of physiological relations between parts breaks down, there we too readily assume a breach in organic relations, calling all that lies between such breaches a unit that varies independently. So, to modern biology, a seeming independence in quantity or quality of one or many structures or functions or both, from the rest of the organism, together with a real and obvious causal relation between the former, are enough to give them the status of a unit for heredity, having a set of little determining demon particles all to themselves. And yet we are asked how it is that characters can vary independently of one another; and ingenious theories are built to account for so strange a fact and to fit an apparent union on to this disunion. Why, but for the very simple reason that just those phenomena which, to superficial observation, range themselves in independent groups are chosen for the dignity of being styled separate characters. Why A = B, is a question that needs little discussion if higher up the page you have defined A solely on the ground of that equation. If this be not the principle of definition of a character, why consider the length of right and left legs not two relations but one? If they ever differed seriously in

length they would be distinguished as two. Now, if this is really the principle on which characters are to be defined, it is superfluous to ask whence the independence in variations. That independence rests on ignorance of causes, and is restricted, step by step, with the growth of physiology. Were all the relations of function of the parts of an organism known, it is simply inconceivable that the quantity or quality of any part you like could be regarded as without effect on the rest, or as having come to the condition in question without the co-operation of the rest. And even if such a hiatus in the relations of parts were thinkable, we have ample evidence that it does not occur, in the cases of obscure correlation of phenomena of various kinds in various parts apparently unconnected with one another in any special manner. In every living thing the islands of connected changes which rise here and there above our sea of ignorance are united on a common bed, perhaps not so deep down as is usually taken for granted.

This false isolation of characters in the adult, and with regard to inheritance and variation, is the chief cause of the assumption of demon particles; and in this connection it may be said that in the expression *latent characters*, which are carried through one generation or sex to appear in another, whether in cases of dimorphism, polymorphism, or atavism, we have a metaphor that often causes error, though it would be pedantic to object to the convenient phrase. The character simply does not exist in the intermediate generation, and no carriers need be invented for it. To say that an appearance is latent is to say that it is not. Though there were waves yesterday and will be waves to-morrow, we do not think of saying when the sea is calm that they are now latent in the sea; we simply say that there are no waves, though we know that when the conditions arise they will appear. A character is simply the appearance caused by the reactions of circumstance and the mass of cells derived from the germ, and in the absence of certain conditions certain characters do not appear. This has been well shown in respect to one class of characters by the valuable essay of Geddes and Thomson. The action of circumstance must come into our account of the rise of alternative structures and functions somewhere; it is surely more reasonable to regard it as directly answerable for the changes in question, and not simply indirectly as awakening the demon carriers of one set of structures which thereafter supersede the demons of the alternative set which now become latent. For thus we have the support of experiment, and substitute a physiological account for what is at best only a bad formal one. One might as well say that men have within them the carriers or determinants of a latent black eye, which are called into action by a blow, as that the difference in form and function of the sexes is due to the action of alternative sets of "determinant vehicles of hereditary characters" one of which triumphs over the other. If one alternative form differs from the other in one respect,

it differs in all; it is against all physiological and pathological experience to think of, say, the sexual characters as independent structures and functions superimposed on a common neutral basis.

If characters are not regarded as varying independently of one another, or as being present in some mystical way in the forms which "transmit" them without "showing" them: if they are not looked at as being any definite unities for the organism, but are recognised as arbitrarily-defined groups of phenomena; if, in short, we make it clear to ourselves that they are only appearances of the individual, which cannot with justice be divided into parts in the consideration of heredity, and which if it varies in one respect varies in its whole constitution, then the result for our theories of heredity is obvious. The questions become at once more complex and more simple. More complex because, being deprived of our false units, we have to leave tabulating characters, and have to go to school to physiology that we may trace the many aspects of the individual to the functional peculiarity of the germ, the many differences to the one, just as the above-mentioned authors have done for the complex phenomena of sex; and because a character ceases for us to be transmitted or carried latent as such by the demon particles, and has instead to be referred in its rise to the reactions of every part with every other and with the world. They become more simple, because we shall cease to be hampered by the difference in the habits of authors as to the way in which they form characters, or by the difficulty of reducing positive and negative relations of quantity and quality, utterly incoördinate and incomparable as they are, to a common formal expression; and most of all because we shall cease to be confronted with the question as to how the demon particles do their strange work.

Surely it is an unprecedented thing that men going about to explain the shapes of animals and plants should leave on one side what are clearly the causes of these shapes, namely, the processes of development which lie to their hand in physiology and embryology, and should rest content with an antique and fantastic scheme of growth which makes all intermediate processes of no value; so that all the changes known as inheritance and variation can be studied in relation to only two moments in the whole of life, namely, the adult, as pure *morphe*, and the germ, fixed and stained. And since this has been to so great an extent our method, it is not surprising that Natural Selection is loaded with more than it can bear, or that the expression which the great principle of use-inheritance receives is artificial and exaggerated.

GEORGE SANDEMAN.

III.

The Cross-Fertilisation of Food-Plants.

SUCH a Society as the Royal Agricultural, which boasts the motto, "Practice with Science," could not better carry out its principles than by encouraging experiments in the cross-fertilisation of food-plants. Darwin, we know, put forward conclusive evidence in favour of cross-fertilisation as a general rule for the production of the strongest and most fertile offspring, and insisted on the advantage or necessity in all cases of an occasional cross. These are points which scientists universally accept; yet so great is the gulf still between practice and science, that the vegetative method is to-day often the only one used in the propagation of our most necessary crops; *i.e.* this year's plants are produced, not from the seed, but from cuttings or some detached portion of last year's plants.

The potato plant, for instance, has been taken from a light soil in the cool, dry, even climate of the mountains of Chili and grown for years in all soils in the warm, damp, changeable climate of the British Isles, and, to make matters worse, instead of starting again and again with new individuals produced from seed, the life of single individuals has been indefinitely protracted by purely vegetative reproduction. Part of an older plant, namely the tuber, has been divided up into new plants, and this process has been repeated till, under the artificial conditions of growth and multiplication, the plant has lost all stamina. What wonder, then, that the last fifty years tells a tale of continued struggle between the weakened plant and disease? It is to cross-fertilisation that we are now looking for help in the fight with the fungoid and other pests which have done, and are doing, so much damage. During the last few years Messrs. Sutton have raised seedlings by crossing a variety of the common potato, *Solanum tuberosum*, with another species, *Solanum Maglia*, or, as it is sometimes called, Darwin's potato; and, though it is too early for definite statement, we may hope that in a few years some of the seedlings will prove the precursors of varieties yielding crops good in themselves and also less liable to disease. Not only is there the strengthening influence of the new blood introduced by the cross, but the advantageous fact that *Solanum Maglia* is a native of a damper, warmer climate, more like our own than is that to which the common potato is indigenous.

The recent discovery of seedling sugar-canes may exercise an important influence on the prosperity of our West Indian colonies. As with the potato so with the sugar-cane, vegetative propagation has been exclusively followed: new individuals have been produced, not from seeds, but from cuttings of the joints of older plants. Indeed, so long has the sugar-cane been cultivated, that, like the wheat, its native country is to-day a matter of dispute, and it is a question if it is now ever found in the wild state. In consequence, perhaps, of the many centuries of cultivation, the plant has almost lost the habit of producing seeds, and we may search the "arrows," as the long, feathery flower-heads are called, and find not a single fertile flower. It is obvious, that if the ovary has become permanently sterile, or the pollen lost the power of fertilisation, the benefits of cross-fertilisation in the form of invigorated offspring or new varieties, are precluded. Until about three years ago there was a very general opinion, and one, too, of long standing, that the sugar-cane did not seed. In 1889, however, as the result of observations in Java, Dr. Benecke published a description and drawings of the ripe fruit and the germination of the seed; while, almost at the same time, Messrs. Harrison and Bovell, working at the Botanical Station at Barbados, proved that some varieties on the island also produced seed. From even seedling plants were raised, while some seed sent to England was successfully germinated at Kew, and also by Messrs. Veitch at Chelsea. Fertile flowers on the "arrow" are few and far between, and the size is very small; but in spite of these difficulties the discovery opens up the prospect of seminal reproduction, and the advantages of cross-fertilisation.

The object of the cross-breeder is to secure in one variety the valuable characteristics of two or more; to get, in fact, the largest possible crop of the best quality, and ripening at the proper season. In no case is this of more importance than with wheat and other cereals, the cultivation of which in Great Britain is now at so great a discount. In 1839, according to a statement in the *Journal of the Royal Agricultural Society*, the annual growth of wheat in England and Wales was estimated at 12,350,000 qrs., worth, at 50s. per qr., nearly 31 million pounds sterling. Fifty years later, in the same journal, we read: "The present annual growth in the United Kingdom does not exceed 9 million quarters." As the crop is still held to be indispensable in the corn-growing districts, the introduction of improved varieties for the sake of adding to the yield is of great importance. An account by Mr. Carruthers of some experiments on the cross-fertilisation of cereals, published in the latest volume of the same journal, is consequently of great interest, and no apology need be made for a brief re-description in NATURAL SCIENCE.

In the great majority of grasses the male and female organs, stamens and pistil, are contained in the same flower, not surrounded by an attractive corolla of petals but more or less enclosed in

brownish scales called glumes, the number of which varies in different genera. Each flower is solitary, or more often several are crowded together forming a spikelet, while the spikelets may be arranged closely forming the so-called "spike" as in wheat and barley, or loosely as in the "panicle" of the oat.

In each flower the stamens and pistil are generally ripe at the same time and thus self-fertilisation is favoured. The barley is an extreme case, as here the fertilising pollen escapes from the stamen and falls on the moist receptive stigma of the pistil before the flower opens and while the ear is still within the sheath. When the stigmas are finally pushed out beyond the scales of the flower they are dead, and cross-fertilisation is therefore precluded. In the wheat the anthers are already burst and some of the pollen-grains discharged and adhering to the divisions of the ripe, feathery stigma before the flower naturally opens, showing that self-fertilisation is at any rate possible. The flower opens for a short time, about a minute, the opening being effected through the pressure of the stamens, stigmas, and the two small scales, or lodicules, supposed by some to represent the petals; the filaments pushing the anthers before them extend to three or four times their former length, and the feathery stigmas spread to the air. The pendulous anthers are shaken by the lea breath of air and rapidly emptied of their remaining pollen-grains that the transference of pollen from one plant to the stigmas of th flowers of another is also rendered possible. Observations in the field show, however, that self-fertilisation is the rule, for in experimental grounds where different varieties of wheat are grown in close proximity in parallel rows, no accidental cross-fertilisation due to wind-carried pollen-grains has been noticed.

Artificial cross-fertilisation of wheat or barley is a delicate operation. The parts of the flower are small, and the slender stamens must be removed before the pollen is ripe, while the stigmas must be protected from the access of pollen from outside. The application of the selected pollen to the feathery stigma is best accomplished by applying the ripe grains direct from the anthers to the stigma by gently moving them over its surface. Moreover, this has all to be done in the open field on a flower supported on a long, slender, and easily-moved straw.

The offspring obtained by cross-breeding vary in fertility with the closeness of affinity between the two parents. By fertilisation with pollen from the flower of a similar plant of the same species, or even of a plant with characters distinct and permanent enough to be recognised as a variety, though still of the same species, we obtain offspring producing fertile seeds from generation to generation. When the pollen applied is taken from an allied species of the same genus, perfect seeds can generally be obtained, but the hybrid plant of the next generation is almost always sterile, though the vegetative organs, such as leaf and stem, are often finer. In the case of widely-

separated species of a genus the difficulties are greater, and the chances of procuring fertile offspring less; while hybrids between distinct genera are rare. Out of four hundred experiments in fertilisation of plants of the four different genera—wheat, rye, barley, and oats, with pollen from each other, in only two cases were seeds produced. The two seeds, hybrids between rye and wheat, germinated and produced plants intermediate between the parents, but the pollen did not ripen, and consequently the flowers set no seed.

In the case of plants which are not cultivated for the sake of their seed, but merely for the flower as by the nurseryman, or for some portion of the vegetative structure like the sugar-cane or the potato, the absence of seed is no disadvantage, while the increased vigour of the vegetative organs may be an advantage. It is obvious, however, that with cereals we must keep well within the limits which will ensure production of seed; attempts to produce improved forms must be limited to the crossing of individuals of a single species, including its varieties. Since they have been so long under culture and have given rise to so many cultivated varieties, it is not only hard to determine which variety most nearly represents the primitive form, but also with how many species we are dealing. The following seven types of wheat are easily distinguished:—

1. *Triticum monococcum*, L., with one grain in each spikelet, and the innermost scale (or pale) dividing into two when ripe.

2. *T. Spelta*, L. (spelt), with a loose four-sided ear, and grains which do not fall out in threshing.

3. *T. dicoccum*, Schrank, with dense two-sided ears, and grains as in the spelt.

4. *T. vulgare*, Vill., with compact ears, and grains falling when threshed.

5. *T. turgidum*, L., with dense four-sided ears, and short, thick, blunt grains, which fall out in the threshing.

6. *T. durum*, Desf., with dense long-awned ears, and long, narrow, very hard, and flinty grains, which fall when threshed.

7. *T. polonicum*, L., with long compressed ears, long papery glumes, and long narrow grains.

Botanists hold different opinions as to the number of species comprised by these seven types. Blomeyer unites them under one, *T. sativum*, L., while Hackel, the greatest living authority on grasses, thinks *T. monococcum* must be separated as a distinct species. The second view is supported by the fact that fertile hybrids have not been obtained between *T. monococcum* and any of the varieties of *T. sativum*. Hackel considers *T. polonicum* as a species which has, perhaps, arisen under cultivation. Our illustrious countryman Bentham recognised three species, *T. monococcum*, *T. Spelta*, and *T. sativum*, the last being represented by several varieties. Hence, if we wish to obtain improved varieties by means of cross-fertilisation,

T. monococcum must be tabooed, while *T. polonicum* had also better be avoided.

The ideal wheat for cultivation in Britain should give plenty of straw of good quality, and strong enough to carry the ear till ripe without being laid, should tiller—*i.e.*, produce new shoots from the base—freely and ripen early, yielding a good weight of fine well-filled seeds. "In selecting parents it should be remembered that the male appears to exercise a special influence on the seed, while the female affects the character of the vegetative parts of the plant." In the *Royal Agricultural Society's Journal* for 1889, Mr. Henry Evershed gave an account of experiments on the cross-fertilisation of wheat carried out by Messrs. Carter; the cross-bred seedlings yielded a variety of forms, and careful selection was required to fix permanently the best of these. In his recent paper, Mr. Carruthers describes some results of experiments in cross-breeding, extending over thirty years, made by Messrs. R. & J. Garton, at Newton-le-Willows, Lancashire. These gentlemen have collected numerous varieties from which to select suitable parents, not only from Britain, but from all parts of the world. Parents have been chosen both from the established varieties with which they began and also from the improved ones obtained in the course of their experiments. Whenever a good point in quality or quantity of straw or grain was detected, an endeavour was made to maintain or increase it by using the plant in turn as parent. Thus the varieties with which they are now working are the products of many previous crosses, and may be termed composite crosses. A serious difficulty exists in the increased tendency to variation with the increase in the number of parents, and careful selection of desirable forms and their continued cultivation is necessary, with persistent elimination of sports and defective plants, before a fixed variety can be established.

Our first figure represents a few of the parents of the new varieties raised by Messrs. Garton. Hardcastle and Mainstay are in general cultivation. The spelt was used in the hope of getting a form which would inherit a seed so enclosed in the glumes as not to fall out during harvesting, while the flinty seeds of the Hard wheat and the abundant cropping of the Grey wheat offered qualities which might be combined with advantage. After several years' crossing, a selection was made of the most hopeful varieties, and these have been under careful cultivation for three years. The second figure represents eleven of these composite crosses grown in 1893, "all of which are the progeny of a selected plant of the harvest of 1890." The remarkable tendency to sport is well shown; it results from the great diversity of ancestors represented in the direct parent, the 1890 plant, and gives some idea of the difficulty experienced in getting a pure and fixed variety. An examination of the individual figures and comparison with the figures in our first illustration will show the effect of the different original parents.

Thus the first figure recalls both Hardcastle and Mainstay, but the ear has more spikelets, each of which contains an increased number of larger grains. The spelt comes out very strongly in Nos. 3, 4, and 5; the last approaches it most nearly, but has larger spikelets more compactly arranged on the main axis. Nos. 3 and 4 contain characters derived from the commonly cultivated varieties. No. 6 is somewhat similar, but branched. In this case the axis of the spikelets, after producing two normal seeds, has, instead of bearing two or three additional flowers as usual, become extraordinarily lengthened, forming a secondary axis which in turn bears smaller spikelets. A similar

FIG. 1.—SOME OF THE VARIETIES USED AS PARENTS.

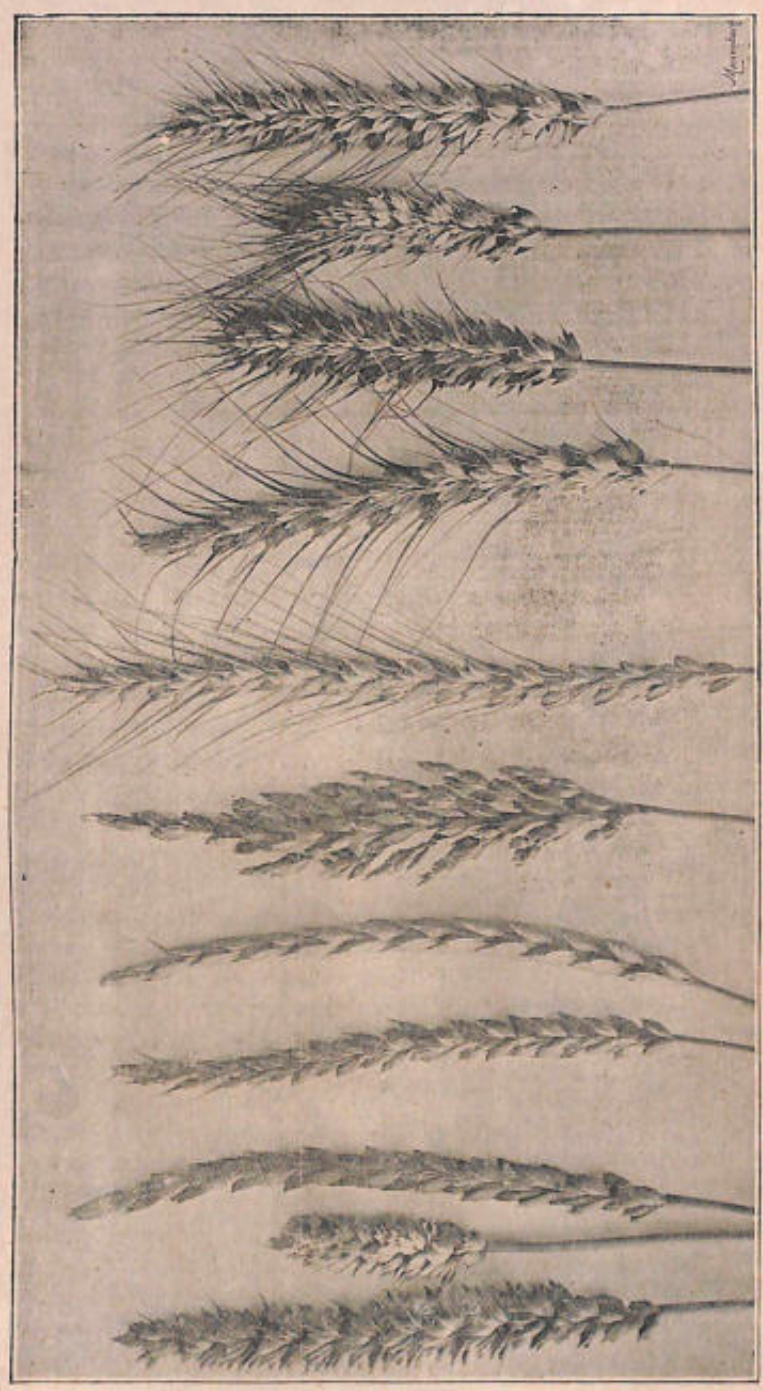


Hardcastle. Mainstay. Spelt. (*T. spelta.*) Hard Wheat. (*T. durum.*) Grey Wheat. (*T. turgidum.*)

Reduced One-half Natural Size.

monstrosity in the Grey wheat has produced the branched variety known as Mummy wheat (*T. decompositum*, *L.*) In No. 7 the spelt is again in evidence, though influenced by a bearded form. In Nos. 8 and 9 is seen the effect of the Grey wheat, as also in No. 11, which is, however, nearer the selected Bearded wheat.

The different forms of cultivated barleys are probably all varieties of a single species. The spikelets are borne in groups of threes on the opposite sides of the axis, but as a rule only the centre one of each group is perfect and produces seed. The ear is thus two-rowed



1. 2. 3. 4. 5. 6. 7. 8. 9. 10. 11.



1. 2. 3. 4.

1.—Bere (Parent). 2 and 3.—Crosses (Progeny). 4.—Two-Rowed Barley (Parent).

FIG. 3.—PARENTS AND CROSS-PROGENY OF BARLEY.
A little less than Natural Size.

(distichous), while the barren spikelets from opposite groups are placed close together along the centre of the flat face of the ear, hiding the axis. This characterises *Hordeum distichum, L.*, which includes all the two-rowed forms; the ears may be white or coloured, and the glumes may be adherent or free from the grain. On the other hand, the two lateral spikelets may be fertile and the central barren, giving a four-rowed variety (*H. vulgare, L.*). If all three spikelets in each group are perfect, a six-rowed ear results (*H. hexastichum, L.*). If we accept the view that these forms, which Linnæus regarded as distinct species, are really merely varieties of one, we see a reason for the ease with which cross-fertilisation has been carried out between them. Messrs. Garton tried to obtain a variety in which all the spikelets should be perfect, while the size and quality of the seed should equal that of the best cultivated two-ranked forms. By crossing a six-ranked with one of the best two-ranked varieties, known as Golden Melon, they succeeded, after further crossing, in getting composite varieties bearing grains equal in size and quality to those of the Golden Melon, but, being six-ranked, three times more numerous (see Fig. 3). Similarly, by the repeated crossing of the three well-marked forms of oats, a considerable quantity of luxuriant new forms have been obtained.

As Mr. Carruthers observes, the important services rendered to agriculture by these gentlemen will be more appreciated when they have secured enough seed of the new varieties to permit of their being grown as farm crops.

For the interesting illustrations which accompany this article we are indebted, through the kindness of Mr. Carruthers, to the Royal Agricultural Society.

A. B. Rendle.

IV.

Neuter Insects and Darwinism.

MR. PLATT BALL (5) does not think that Herbert Spencer's reply (2) to Weismann's argument concerning neuter insects, in the *Contemporary* controversy, is convincing, and he adduces certain new points to which he thinks Spencer's arguments do not apply. I propose to examine again the questions in dispute, and to try to ascertain how much weight is to be attributed to Mr. Ball's contribution to the discussion. I do not propose to take the question of use-inheritance as my chief point, but to consider what are the most logical conclusions we can draw from the facts before us as to the explanation of the phenomena presented by social polymorphic insects, or those in which several forms are produced by one parent. As I have no original knowledge of these insects, I intend to take the facts chiefly as they are accepted and presented by the three disputants above mentioned.

Weismann (1) limits himself to the phenomena presented by ants. He says that it may be taken for granted that the ant workers have arisen through phyletic metamorphosis of fruitful females. What other origin could they have had? he asks. As evidence of this he mentions that to this day there are some species in which the workers closely resemble the females, and that in other cases intermediate forms have frequently been found. This is the most extraordinary assumption with which to begin his argument, for the one thing obvious and certain is that the ant workers have not arisen by the phyletic metamorphosis of fruitful females. The very fact that the workers are sterile precludes this assumption. They are the daughters and sisters of fruitful females, not the offspring of parents possessing their own peculiarities. No animal can be said to have arisen by phyletic metamorphosis from its mother; and as all the ancestors of the neuter ants were fruitful females, it is difficult to see where the phyletic metamorphosis comes in. None of these ancestors have exhibited degrees of modification which lead to the peculiarities seen in the workers.

Whether the natural selection of females which have possessed in different degrees the property of generating workers adapted to their work is the true explanation of the evolution of the workers, that is the question I am about to consider. But to call this hypothetical

process the phyletic metamorphosis of fruitful females is simply a misuse of terms. Phyletic metamorphosis means the gradual change occurring in a series of organisms of which each is the offspring of the preceding. The origin of the neuter social insect from the perfect female is immediate and sudden. In fact, it is not what has always been understood as phyletic metamorphosis, but what is commonly and universally known as sexual reproduction.

But the problem is why is the neuter insect different from its mother, instead of being similar to her? Mr. Spencer has given the obvious answer which every biologist might have been supposed to know, namely, that the larva of the neuter insect is underfed, an explanation which is ignored by Weismann throughout his article. There are various instances which support the conclusion that in all cases the peculiarities of the sterile persons are due to the conditions to which the larva is exposed, but the one thoroughly-established fact is that the hive-bees do actually produce perfect queens out of any worker-egg or worker larva taken at random. This is sufficient proof that the difference between a worker and a queen is not a constitutional difference, but a difference due to the conditions of development. Without at present going into the theory of biological determinants, it must be admitted that peculiarities which are hereditary and to be regarded as evolved are predetermined in the egg. The fact above mentioned concerning the bees, as well as other similar facts to which Spencer alludes, show that the peculiarities of neuter insects are not predetermined in the egg, but related to certain definite conditions of development. They are not, therefore, either inherited or evolved. It may be said that we do not know this for ants, but at least we know nothing to the contrary.

It follows from this that Weismann is not justified in describing the differences between the worker ant and the perfect female as retrogressive and progressive, implying and maintaining that these differences have been gradually evolved. Among the instances of retrogression he refers to are the absence of the receptaculum seminis, and the reduction in the number of egg-tubes, a reduction which appears in different degrees in different species of ant, the persistent tubes numbering 12, 5, 3, 1, or none at all. It is clear that since the larval ant has the inherent potentiality of perfect generative organs, the reduction in the worker is not due to degeneration but to arrested development. It may be maintained that certain qualities in bullocks can be produced by the selection of bulls and cows, not by the appearance of these qualities in themselves, but entirely because they breed the kind of bullocks required. But no one will maintain that the sterility of the bullock is one of the characters that have been so produced. It makes no difference to the argument that in one case the testes are removed by amputation, in the other the ovaries are not fully developed. The cases are similar in the predetermination of the organs in the ovum. Supposing a

certain number of young males in a herd of deer were regularly castrated, it could not be said that a caste of stags without antlers had been evolved by natural selection or by phyletic metamorphosis.

Other cases of retrogression mentioned by Weismann are the reduction in the number of facets in the compound eyes and the absence of wings. It is obvious that here again we must hold, until the contrary is proved, that the organs fail to develop in consequence of the insufficient nutrition of the larva, and that the eggs in respect of their potentialities or "determinants" are all alike. It is well-known that in true cases of phyletic degeneration there is in the majority of instances actual retrogression in the course of individual development, as, for example, in the cases of the teeth and hind limbs of whales. No evidence has been produced that anything of the kind occurs in the development of the neuters of the social insects.

A very important factor in the peculiar polymorphism of these insects is the phenomenon of insect metamorphosis. The wings and other important organs of the perfect insect are developed not continuously, but within a certain brief period, *i.e.*, during the pupa stage. There is good reason for the conclusion that the conditions to which the larvæ are exposed effect definite differences in the metamorphosis. It is known that temperature alone produces definite results in the colours of certain lepidoptera, that is to say, the temperature to which the larvæ are artificially exposed. Weismann says that the changes in the thorax of the worker ant are just those which would arise through transmission of the deteriorating effect of disuse, but the workers are sterile. He ignores, what is much more important, that the changes are just those which would arise from incomplete development in the metamorphosis, and the worker larvæ are reared in conditions which are known to lead to such incompleteness in the metamorphosis.

Are we to be told, then, it will be asked, that the workers have always been exactly as they are now; have had their peculiar differences fully expressed since the first moment when the solitary ancestor began to be social? To which I reply that the conditions under which the worker larvæ are reared may have been gradually diverging from the conditions required for the production of the perfect insect, and the constitutional peculiarities of the perfect insect have probably undergone gradual modification, but the differences between the worker and the perfect female have always been the direct result of the differences in the conditions under which the larvæ were reared. There is no reason, so far as minus differences in structure are concerned, to suppose that the females have acquired by variation and selection the constitutional property of producing workers of a certain kind. There is no reason to suppose that in the beginning the eggs of the female ant could not be developed into wingless neuter ants as they are now.

Let us now turn to the alleged progressive development of the workers, and consider whether the argument of the selectionist is any stronger in regard to these. The first instance cited by Weismann is the great increase in the brain. The females, never having had any brains to speak of themselves, are alleged to have acquired, as the result of selection, the property of generating workers with large brains. In the first place, I would point out that I know of no careful comparison between the brains of a newly-emerged worker ant and a newly-emerged queen. It is in accordance with well-established facts to conclude that after emergence the brain of the worker ant grows larger, while that of the perfect female diminishes, simply as a result of use and disuse in the individual. Spencer maintains that apparent gains on the part of the worker are really due to the persistence of organs and characters which have diminished in the fertile insects. I am not prepared to say that these two principles fully explain all the apparent plus modification exhibited by sterile workers; but I do maintain that no sufficient evidence has yet been brought forward to show that constitutional characters are present in the workers which have been evolved in them alone, and are neither due to the action of conditions on the individual development nor to inheritance from the original ancestors.

Weismann lays great emphasis on the enlarged heads and jaws in the so-called soldier caste in certain species, such as *Pheidole megacephala* and *Colobopsis truncata*. But it must be pointed out that the very fact that this character distinguishes a special caste of workers is difficult to reconcile with the explanation upheld by Weismann. For, as Lubbock says, the question arises whether those different kinds of workers are produced from different eggs. Lubbock (3) speaks from special and original knowledge of these insects, and he is disposed to concur with Westwood, that the inhabitants of the nest have the instinct so to modify the circumstances producing this state of imperfection, that some neuters shall exhibit characters at variance with those of the common kind. Considering the established fact concerning bees, it is by no means an extravagant suggestion that where there are two or even more castes of workers, the eggs are all essentially similar, and simply developed under definite differences of conditions. If this is the case, and the presumption is that it is, then there is no evolution of the castes in the sense of the modification of the characters predetermined in the eggs. It must be remembered that in all these polymorphic social insects the larvæ may be said to be artificially developed. The conditions in which the larvæ live are contrived by the full-grown workers, and are kept wonderfully constant. We cannot say, as even Weismann would admit, that the small feet of Chinese ladies are due to phyletic metamorphosis or to Natural Selection. Their growth is moulded and checked by the unyielding influence of material bonds, and we have good reason to suppose that equally rigid external conditions mould the developing larvæ of ants,

although Lubbock assures us that the exact mode by which the differences are produced is still entirely unknown.

Into what obvious fallacies Weismann is capable of falling in consequence of his over-zeal for his own views is shown by two cases which I will cite from his first article in the *Contemporary*. To prove that pammixis alone suffices to bring about the complete disappearance of characters, he adduces the degeneration of the feeding instinct in *Polyergus rufescens*, which starves to death unless crammed from the crop of a slave-ant. He states correctly that not only the males and females, but the workers also in this species, have altogether forgotten how to recognise their food. He says that this would be an excellent example of the transmission of functional degeneration—"if only these amazons were not sterile." But, of course, the males and females are not sterile, and Weismann is inadvertently aiming weapons at himself. Another curious fallacy in the same article is the reference to the skin armature of hermit-crabs as one of those passive organs which cannot be strengthened by use, and yet which disappear when they cease to be useful. This case seems so convincing to Weismann that he can only suppose that Spencer ignores it because the philosopher is unacquainted by personal observation with facts so familiar to the naturalist. And yet the wonder really is that Weismann should ignore the fact that the rigid cuticle of crustacea is not merely an armour of defence, but an external skeleton to which the muscles are attached, and which is necessary to their movements. Every naturalist ought to know that in the tail of the hermit-crab the muscles and the cuticle are equally degenerate, have diminished *pari passu*, except certain appendages forming the apparatus at the end of the tail by which the hermit holds on to the shell he inhabits, and in this apparatus both the muscles and the cuticle are well-developed. In this case the weapon so confidently aimed at the despised philosopher bursts in the naturalist's hands, and does more damage to himself than to his adversary.

I have not hither to dealt with the question of habits and instincts. It is confidently held by selectionists like Mr. Platt Ball that the wonderful instincts of the sterile forms of social insects could never have been present in the ancestral perfect insects, and can only be explained by the natural selection of parents which had the power of producing workers that possessed them. Spencer maintains that the instincts are inherited from ancestral perfect insects, in which they were developed by habit and practice. That this is largely true there can be little doubt, when we consider the numerous intermediate cases between solitary females and the most highly-developed communities. What remains, it seems to me, is easily explained as the result of individual education. It is certain that the newly-emerged worker wasp begins making cells and feeding larvæ in imitation of her mother, having no sexual instinct and no males to mate with. The worker wasp inherits much directly from her mother, although not sexual instincts, and learns much by imitation. It is true that

Spencer probably falls into error in deriving the swarming of bees from a nuptial flight, because the nuptial flight takes place separately in a different manner. But it must be remembered that it is the old female which leads out a swarm, and her successor leads out the next, and so on. It is well-known to all experts in bee-keeping, and is stated in the manuals, that the real cause of swarming is simply want of room. The cells are full either of honey or larvæ, and there is not room for any more combs. The workers force the old queen to come with them to find a new home. The story is that they prevent her from destroying the young queens, in order that the old hive may not be left without a queen. But in most that has been written about bees, there is a great deal of teleology and anthropomorphism, which it is impossible to accept without scepticism. The fact is that the workers have one set of instincts, to collect honey, build cells, and rear the progeny of a perfect female. When there is no more room to do this in the cavity they inhabit, they take the female to another cavity. It is practically the custom among skilled apiarians to prevent swarming by simply adding super-hives.

We see, therefore, that although Mr. Spencer's suggestion that swarming is to be regarded as a modification of the nuptial flight, leads to difficulties which Mr. Platt Ball rightly holds to be insuperable, it has not been shown that the present condition of the hive is in can only be explained by the natural selection of indefinite variation. The true explanation can only be reached by regarding the structures and habits of bees, not as advantageous, but necessary, that is to say, as the necessary results of conditions. Mr. Ball argues that the special instincts commenced in fertile daughters who helped the queen-mother at the cost of delay or neglect of their own maternal functions, a step towards the formation of a neuter caste. It seems to me impossible to believe anything of the kind. It is much more reasonable to believe with Mr. Spencer that the workers looked after the eggs and larvæ of the perfect female simply because they were themselves sterile. They were sterile through imperfect nutrition in the larval state, and, therefore, having no sexual instincts, but retaining the maternal, they proceeded from hereditary instinct and from imitation to rear their brothers and sisters. No one can deny that it is the queen or perfect female which has been modified rather than the workers, in view of the fact that the pollen-brush on the legs is entirely wanting in the queen bee, though well-developed in the neuter, while in every other species the pollen-brush is present in the perfect female. Are we not entitled to regard this as evidence of the transmission of the effects of disuse? The female bee transmits the pollen-brush to her imperfect descendants in every generation, simply because they remain permanently at an earlier stage of development. No doubt the pollen-brush would be found to be represented as a transitory stage in the development of the queen bee, if the history of her metamorphosis were completely traced.

Anyone who candidly and thoughtfully considers the views I have tried to express will see that Mr. Platt Ball's objections are superficial difficulties due to zeal for the dogma of natural selection. In science everyone should take as his watchword *surtout point de zèle*. It is true we cannot maintain that the honey-pot modification in ants was originally possessed by perfect males and females. But why should we believe that the peculiarity is inherited at all? There is nothing unusual in ants or bees regurgitating honey, feeding one another or larvæ. A differentiation of habits must ensue when a number of neuter insects live together in a community, simply because all the individuals cannot be similarly related to their conditions and their work. Mutual relations are established between them, and those persons which remain in the nest, receive food, and give it back again, may well, when food is plentiful, become distended. The fact that one of these honey ants lives in Mexico and one in Australia shows that the peculiarity has arisen twice independently. It is not yet perfectly certain that the persons with the distended abdomens are a separate caste: every individual may be at times in this condition.

Mr. Ball's statement of the case of the termites turns out to be a characteristic anachronism. Like a good many other faithful adherents of the theory of natural selection, he seems to have neglected to take advantage of sources of information more recent in date than the "Origin of Species." Not having made a special study of polymorphic insects, I found it difficult to obtain original papers containing the results of research on these social neuroptera. I had a strong conviction that when the subject was properly investigated it would be found that the different castes were reared under different conditions. At last I find that, according to Emery (4) the investigation has recently been carried out, with the results which I anticipated. Grassi, an Italian biologist, has satisfied himself and published direct evidence that the termites have the power of regulating the number of workers and soldiers by rearing them at will by appropriate feeding and treatment, as they also hasten the sexual maturity of other individuals by special nourishment, and so produce the supplementary sexual individuals which were so long a mystery. Emery concludes that the workers of all social insects are reared in a similar way from germs which are capable of producing normal sexual individuals.

Mr. Ball's last objection as to the evolution of sterility in neuters is a mere bladder which scarcely needs special pricking. It has been disposed of by the general position taken up by Spencer and myself. The only reasonable view is that the sterility of workers is the necessary result of the habit of the ancestral insect to bestow maternal care on her young. Is it not a fact that in the human species the eldest sister in a large and struggling family has her chance of marriage reduced? The characteristics and the usefulness of old maids in the human species are familiar enough; does anyone

suppose that they are the result of the phyletic metamorphosis of fruitful females? Weismann triumphantly adduced the case of neuter insects in social species as an incontrovertible proof that natural selection could do every thing without the possibility of any transmission of somatogenic or so-called "acquired" characters. As in many other instances, the argument is produced by looking at the phenomenon upside down. I believe that some day it will be generally admitted that the whole natural selection doctrine is, in the etymological sense, preposterous. There was a time when it was universally held that the sun moved round the earth. So it does, apparently. It has a motion across the heavens from east to west in relation to a human observer. The belief was true up to a certain point. But Galileo discovered that the earth moved round its own axis. In relation to the whole solar system the sun is fixed—the earth moves. So it is with the evolution of organisms. Assuming the occurrence of the modifications, it may be said from an anthropomorphic point of view that natural selection preserves them. But it is a much deeper truth that evolution is the result of successive modifications or changes in succeeding generations, and the mere survival of these modifications is not the cause of them. Whatever may be the case in other instances, it is clear that the peculiarities of neuter insects as compared with the perfect individuals of their species, are the direct result of the conditions of life. Under similar conditions the eggs all develop into similar individuals; the differences of the individuals correspond to the differences of conditions. The differentiation has doubtless been progressive, because the differentiation of conditions has been progressive. If we start from a solitary perfect female which had developed the habit of bestowing maternal care on her young, the gradual development of the system naturally follows. There is no reason to believe that the arrest of the metamorphosis by insufficient food, etc., has been evolved specially in the social insect. It occurs in the Axolotl, and also in the common tadpole; in the former naturally, in the latter artificially. The social insect is not a community, it is a family. The queen is merely a breeding mother. As soon as she began to feed her larvæ piecemeal, instead of laying an egg in a store of food, it was inevitable that some of them should be inadequately nourished. These by instinct and imitation fed their brothers and sisters, and when food was abundant perfect insects were produced. But the mother having less work to do and more food, became increasingly fertile, so that in subsequent generations more workers were produced, and she had still less work to do. So long as the perfect female started in spring alone without help she continued to perform maternal duties herself. Where she survived the winter with a number of workers, the need for work on her part ceased. As soon as the workers existed the relations of these and the female to the outer world were different. The consequent modifications of the perfect female were transmitted

to her descendants. Thus the workers also change, for though the eggs of one female are all alike, the imperfect development of these eggs may well result in a different worker now from that which was produced in previous generations. When the female with some workers survived the winter, her specialisation went much further, for after this she never worked at all. In considering how this change took place in the hive bee, we must remember that this form was originally domesticated in a warmer climate. It is not native to Britain in the wild state. In the case of ants and termites, the continuity of the community could probably be easily explained from their fossorial and constructive habits, which prevent their destruction by the seasons. It has been proved by Lubbock that a single female can originate an ant community, although she probably does not usually do so. The termite king and queen appear to be too much specialised for reproduction to found a community without the help of workers. So far, then, from being a triumphant proof of the all-sufficiency of natural selection, the peculiarities of social polymorphic insects, rightly regarded, offer the strongest support to Lamarckian principles. It may be said that the degeneration of working organs and instincts in the so-called queens, and their increased fertility, were advantages to the species and therefore survived, but it seems to me that it is difficult to avoid the conclusion that these modifications are the direct result of disuse and stimulation, and that they are transmitted.

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J. T. CUNNINGHAM.

V.

Continental Growth and Geological Periods.

IN the early days of Geology a period indicated a certain section of the earth's history distinctly marked off from that which preceded and that which followed. Each period had a fauna and flora peculiar to itself by which it could be recognised through the fossil remains found embedded in its rocks. There is no doubt that the Mosaic account of the Creation gave traditional support to the notion of distinct breaks in the geological chain. Each period represented not unnaturally to early thinkers a separate creation followed by complete destruction. What was first noticed were the salient differences between the fossil contents of strata geologically far apart such as the reptiles of the Lias and the fishes of the Chalk. Ingenious men found then, as Mr. Gladstone does now, a parallelism in the order of creation between the Mosaic account and the record of the rocks.

When, however, the record was further searched, interesting links were discovered, which, if they did not actually bridge over the differences, led men to think that, could all be restored, the earth's history would be found to be one continuous record unbroken by cataclysmic collapses and successive repairs. Curiously enough, these ideas held their ground in face of the uniformitarian theory of the earth given to the world by Hutton at the close of the last century. Lyell, following on the same lines, with a wealth of illustration and rare literary skill, showed that a true interpretation of nature in the past was to be sought in the action of present causes. The science of geology was thus put on a stable base, and men were taught to arrive at their opinions by reasoning upon facts instead of merely giving free scope to their imaginations. The doctrine of uniformity may not be theoretically correct; indeed, uniformitarianism is a misnomer even as applied to Lyell's conception of the history of the earth. It might with quite as much justice be called development. The popular conception, however, of uniformity was that things are now as they have been in the past and as they will be in the future. From this it resulted that "periods" came to be looked upon only as arbitrary divisions of geological history which enabled one to grasp the sequence of geological events. Indeed, it is but fair to say that there is not in the whole of Lyell's "Principles," for which no

one has a profounder regard than myself, any indication of how the distinctive geological and physical features of the several periods came about, or, indeed, why they should exist at all. As a student of Lyell from my earliest dabblings in geology, this was long a perfect mystery to me. In putting forward the following suggestions as to how the periods were evolved, I do so with all humility, looking upon them as a development of the great master's work.

Sedimentation and Land Making.

It is a well-known axiom in geology that the land is being lowered at an average rate of about 1 foot in 4,000 years by meteoric action, by rain and rivers, or all those chemical and mechanical forces that come under the general term subaërial denudation. To this is to be added the mechanical abrasion of coasts. The matter carried into the ocean in solution in river waters is, I have shown on the average of many years, about one-third that in suspension. This seems at first blush a large proportion, but when we consider that the matter in solution is a much more constant quantity than the matter in suspension, our surprise is modified.

The sedimentary matter—which is chiefly silica either in the form of grains of sand, or in a much finer state of comminution, as flour of rock mixed with the decomposition products of various rocks, notably felspathic, forming what, when deposited, we call clay—is laid down in a more restricted area than the matter in solution. Mixed with these products of denudation are calcareous particles, mica and other minerals, which all go to make up one or other of the various sedimentary strata. These are, as a whole, often looked upon as the effects of mechanical erosion, but, so far as this is an expression of dynamic action, they are only partially so. Chemical forces have, in my opinion, much more to do with loosening the bonds of the rocky particles than has mere pounding of the boulders along shores and river-beds; they also effect the separation of the rock-masses. To be impressed with this fact one has only to look at some of the enormous masses of rock in mountain districts in Wales, moved to their present positions during the last phase of the Ice-age. Although they have been exposed to nothing more than meteorological influences since, they are frequently split up into many separate blocks and are much weathered. Likewise, in granitic districts enormous masses of granitic sand are, as we may say, liberated by the decomposition of granite.

To these sediments must be added boulders and pebbles which go to form conglomerates. These are, however, seldom delivered into the ocean by large rivers. They remain in the higher reaches or mountain tributaries, so that the boulder-beds found in the sea are either the products of coast erosion, the dynamic undermining of cliffs by wave action assisted by meteorological influences, or are formed by mountain torrents with swift gradients, or are carried by glacial agency or by floating ice.

These mechanical products of denudation are sifted out and arranged by the ocean waves, tides and currents, in the order of their sizes and specific gravities, so that the boulder-beds are mostly beach deposits, while the pebbles, gravel, grains of sand, and finely-triturated material are carried out further and further from the coast and into deeper water in the inverse order of the size of the particles.

It is well known that the thickest deposits accumulate nearest to the land-masses; I do not indeed think that any fixed line or margin can be drawn, within which limit deposits may be considered terrigenous and outside oceanic, but it is a general principle to be kept in mind. When coast waters are shallow and what is called the continental sub-aqueous plateau extends far out from land, then these mechanical sediments will under ordinary circumstances extend the furthest.[1] But in the extremely interesting "Reports on the Dredging operations off the west coast of Central America to the Galapagos, to the west coast of Mexico and the Gulf of California,"[2] by the U.S. Fish Commission steamer "Albatross," Alex. Agassiz says, "I was struck while trawling on our second line between the Galapagos and Acapulco to observe the great distance from shore to which true terrigenous deposits were carried. There was not a station there occupied of which the bottom could be characterised as strictly oceanic." . . . . "A very fine mud was the characteristic bottom in we brought up, often very sticky, and enough of it usually remained on the trawl even when coming up from depths of over 2,000 fathoms materially to interfere with the assorting of the specimens contained in our trawls"; along with the mud, logs, branches of trees, twigs, and decayed vegetable matter usually came up. The distance of the Galapagos Islands from the nearest land in South America is between 500 and 600 geographical miles, while the line between Acapulco and the Galapagos is about 1,100 miles.

The 2,000-fathom line often comes within 100 miles of the coast. Doubtless opposite to the mouths of great rivers, such as the Amazons and the Congo, terrigenous deposits will have a wide extension over the ocean floor. The Indus and Ganges spread their deposits over 700,000 and 900,000 square miles respectively. ("Conditions of Sedimentary Deposition," p. 500.)

There is, however, no doubt that the matters in solution have a much wider extension, only limited, in fact, by the area of the ocean itself. These matters are removed from the water mostly by organic agencies and consist, in the largest proportion, of carbonates and sulphates of lime. Whether any direct precipitation of the matters in solution takes place in the ocean bottom is a subject of surmise, but

[1] *See* "Conditions of Sedimentary Deposition," by Bailey Willis, *Journal of Geology*, vol. i., no. 5, p. 498.

[2] Bulletin of the Museum of Comparative Zoology at Harvard College, vol. xxiii., 1892.

there are grounds for believing that they may be chemically deposited as well as organically separated.

Orographic Relation between Mountain Ranges and New Land-areas.

Leaving for future consideration the regional variations of level which occur in the earth's crust like slow pulsations, and which do not appear to be directly connected with sedimentation, we will briefly examine the evidence pointing to the relation between mountain ranges and new land areas. It is a pretty-well-established fact due to the labours of Hall, Le Conte, Dana, and numerous other investigators, that mountain ranges are built up out of great thicknesses of sediment. Upon this phenomenon is based my theory of the origin of mountain ranges by sedimentary loading and cumulative recurrent expansion.[3] The evidence that mountain ranges are composed of great thicknesses of sedimentary rocks, often with very little unconformity between the rock-groups of which they are built up, is an open book to anyone who takes the trouble to carefully examine the sections, maps, and descriptions of any of the known mountain areas in any part of the globe. It is true of the Rocky Mountains, the Andes, the Himalayas, the Alps, the Caucasians, the mountains of the Turco-Persian frontier as also of the older chains such as the Appalachians and Urals, which have been greatly denuded. It will no doubt turn out to be equally true of the Thian Shan and the great ranges of Central Asia bordering Chinese territory; but these have been so far very little studied.

An examination of the excellent geological map of the world recording all the information up to date, which the labours of Jules Marcou have given us, will show that the rocks comprising the newer mountain chains, which are generally considered to have been upheaved in Tertiary times, have a wide extension beyond the limits of the main mountain-masses. Roughly speaking, the Cretaceous and Tertiary may be said to occupy the half of Europe and a large area of Northern Africa, together, doubtless, with the bed of the Mediterranean Sea. It is just where these rock-groups are most developed and underlaid by more or less conformable Mesozoic groups of great thickness that the mountain-masses occur. This is true even of the Apuan Alps as shown in Stefani's excellent sections.[4] These rock groups appear to extend from the Caspian to the Himalayas, but further eastward we get largely into the unknown.

In North America, though we have not such full information as in Europe, the Cretaceous appears to occupy, or did occupy (having been

[3] "Origin of Mountain Ranges"; London, 1886.—*Also see* Outline of Mr. Mellard Reade's "Theory of the Origin of Mountain Ranges," *Phil. Mag.*, 1891, pp. 485–496.

[4] "Le Pieghe dello Alpi Apuane, contribuzione egli studi sull' origine delle Montagne." Firenze: 1889.

denuded from large areas), about half of the continent, and the same may be said of South America, though in both continents large areas remain to be geologically mapped. It must be understood that I am merely speaking in the rough, to convey the idea I wish to impress upon those who read this, and subject to future correction.

Though there are fresh-water and fluviatile deposits among these rock-groups, especially in the Tertiary, the greater bulk are of marine or estuarine origin. There are also great tabular masses of igneous rocks[5] especially in North America, and volcanic action has over all the areas named played a prominent part from the Tertiary until recent times.

Although there is such a development of Cretaceous and Tertiary rocks on the land areas, there is no doubt a great deal, we cannot say how much, below the sea. The West Indian Islands give evidences in their fauna of a former land-connection with South America,[6] and it may be that the whole area of the Gulf of Mexico and the Carribean Sea is occupied by the same deposits overlaid by great thicknesses of post-Tertiary accumulations. There is little doubt that much of the sea-bed of the Behring Sea and the Aleutian Islands, together with a strip of the sea-bed (none can say how wide) along the western coast of North America, was land in Pleistocene times, as proved by the discovery of mammoth remains Pribilof Islands and the N.W. coast of America,[7] as also on Santa Rosa, one of the largest of the coast islands of California[8]; and it is not improbable that Cretaceous and Tertiary rocks may occupy much of this area also.

Without traversing the whole of the known globe and making incursions into the China Seas, Malayan Archipelago, New Guinea, New Caledonia, Australia, and New Zealand, it may be well to mention that Tertiary rocks are found far to the north on the land bordering the Arctic Seas.

It is in these Tertiary areas that the greatest mountain chains of the world are situated, and it is further conceded by most geologists that they are the highest because they are the newest and have not

[5] *See* "Report on the Geology of the High Plateaus of Utah," 1880, U.S. Geographical and Geological Survey of the Rocky Mountain Region, by Captain C. E. Dutton; *also*, "A Geological Reconnaissance in Southern Oregon," by Israel Russell, 4th Annual Rept. of the U.S. Geol. Survey.

[6] *See* letter No. 3 by Alex. Agassiz to C. P. Patterson, on the Dredging Operations of the U.S. Steamer "Blake" from 1878–9. *Bull. Museum of Comparative Zoology*, Harvard College, p. 299.

[7] Dr. Geo. Dawson, *Q.J.G.S.*, vol. l., pp. 1–9, 1894.

[8] "The Flora of the Coast Islands of California in Relation to Recent Changes of Physical Geology," J. Le Conte, Bulletin viii., California Academy of Sciences, pp. 515–520. Extraordinary fluctuations of level are also recorded in the successive sea cliffs up to 1,200 feet above sea level in the island of San Clemente off the coast of Southern California in Pleistocene times. *See* "Post-Pliocene Diastrophism of the Coast of Southern California," by A. C. Lawson, Bulletin of the Department of Geology, University of California, 1893.

been exposed to such long-continued denudation as the Appalachians and Urals, for instance.[9]

Establishment of New Land-areas Connected with Mountain Upheaval.

It is evident from the foregoing considerations that the establishment of new land-areas, whether as continental additions or otherwise, is closely connected with mountain upheaval: the two go together. As I have shown, mountain ranges are never formed on the sites of old denuded land-areas, they never arise in the middle of a continent, but are always preceded by great sedimentation. An old continent may subside in the middle or at the margin, and the inland seas or other submerged areas receiving the sediment may in process of time be upheaved vertically and folded horizontally by lateral pressure, and so become eventually an integral portion of the old land. In my "Origin of Mountain Ranges" I have sought to show that this movement is due to expansion, the initial cause being the heating of the sediments and undercrust by the rise of the isogeotherms or surfaces of equal temperature brought about by the accumulation of sediment. It is unnecessary to explain this principle here, as it can be studied in the original work, but I am pleased to point out that Mr. W. J. McGee adopts this theory in his "Memoir on the Pleistocene History of North-East Iowa" (eleventh annual report of U.S. Geol. Survey, 1889-90, pp. 351) with the suggestive addition, since that the sediments rest on inclined surfaces and are themselves inclined, the movement of expansion must have taken the line of least resistance, and the expanding strata must have moved seawards.[10] Sedimentation, land-making, and mountain building, we have every reason to believe, are directly related in the chain of cause and effect.

[9] "Contributions to the Study of Volcanoes," Judd, *Geol. Mag.*, 1875, pp. 143–152. "The Geological History of some of the Mountain Chains and Groups of Europe," Ramsay, *Mining Journ.*, 1875.

[10] Mr. McGee says:—"The sediments of the successive mantles wrapped about the young continent were dropped from the cold waters of deep ocean, and were long chilled by contact with the waters; yet by the end of the Silurian they were so deeply buried as to no longer feel the chill waters, and to be heated by conduction from the earth's interior; and so the isogeotherms, or planes of equal temperature, rose from the former ocean floor into and through the lower sediments, and their temperature was greatly increased. As they were heated they expanded; a part of this expansion was vertical, and so peripheral portions of the continent were elevated, not by the building up of successive sheets of sediment, but by their own expansion; yet a considerable part of the expansion must have been horizontal, and must have resulted in either mass or particle movement, or both combined; and since the sheets of sediment rested on inclined surfaces and were themselves inclined, the movement of expansion must have taken the line of least resistance, and the expanding strata must have crept seaward, while the strata at some distance from the shore line must have been compressed horizontally, perhaps buckled and crumpled, and thrown into anticlinal and synclinal folds concentric with the shore, or elsewhere crushed and broken into fragments along the planes of least strength."

STABILITY OF CONDITIONS INDICATED BY THE ACCUMULATION OF GREAT THICKNESSES OF SEDIMENT OVER CERTAIN AREAS.

The accumulation of great thicknesses of sediments extending over millions of square miles, such as took place in Cretaceous and Tertiary times over what is now the western half of the North American Continent, points to an extraordinary constancy of conditions over a vast period of time. Greater than this, perhaps, is the record of the earth's history within the Appalachian chain at an earlier period ending with the close of the Palæozoic. We will, however, for the nonce, confine ourselves to the first-named area, in which are situated the Rockies, the Cascades and Coast Ranges, extending with their analogues through 60 degrees of latitude from Central America to Alaska. It is next to impossible with our present lack of information, though the geology of the North American continent has progressed with rapid strides, to sketch out the disposition of land and sea which endured through the Cretaceous and Tertiary.

It is highly probable, however, that the old Laurentian areas of Canada and the eastward areas of the United States, with other land stretching into the Atlantic, provided at least part of the sediment. It is pointed out that the deposition of sediment, leaving out of account matters in solution, takes place over a smaller area than th[at] of the land from which it is derived. This would seem to indica[te] that though general conditions of stability of land-areas obtained, fluctuations of distribution occurred throughout the history of these periods in North America. Land to the westward, an extension of the Asiatic continent eastward, may very likely have sent important contributions towards the making of the future land. But, as I have said, these are speculations which, if we had all the complicated facts before us instead of buried beneath the sea, might not yield satisfactorily to imaginative reconstruction, much less to minds in the position that ours are now.

It is highly probable that many fluctuations of level occurred, and that sediments were raised above the level of the sea and again redistributed. Still, on the whole the locus was the same, and so in time the sea-bed got built up, until the internal forces of the earth, reacting upon this varied sedimentary load, began their rôle of mountain- and land-building, and the establishment of the western area of the North American continent.

In like manner, we might trace the growth of the European continent as it now exists, together with that of a large part of Asia, where the record of the rocks tells the tale of long-continued periods of sedimentation, culminating in the upheaval of the Alps, the Caucasians, the ranges of Turco-Persia, and the Himalayas.

The story might be extended to South America and Australasia, but for the purpose of illustration the two examples given are sufficient.

The later rocks of the series more or less represent land condi-

tions consisting of lacustrine and fluviatile deposits, showing that the establishment of these land-areas dates from a very early time.

The history of these Cretaceous and Tertiary areas is one of both maturity and decay. They are in some portion of the latter stage now, and eventually will probably give place to new land-areas formed out of rocks now being constructed in the sea-beds from their own dissolution.

Lithologic Characteristics and Differences of the Rock-groups of some of the Great Periods.

It is a remarkable fact, but no less true, that there are not only palæontological homologies running through the rocks of the Tertiary group over the known world, but lithological analogies also. There are diversities truly, but similarities also. Though conditions are recurrent through the ages, as pointed out so strongly by the late Sir Andrew Ramsay, it is recurrence with a difference. There has been lithological evolution as well as organic.

When we go further back in the world's history and consider the rocks constituting the Carboniferous formation, the persistence of characteristics over large areas of the earth's surface is most remarkable. The repetition of coals, sandstones, and shales in the Carboniferous of the North-American Continent and in that of Great Britain seems almost to point to a common origin. They repose upon great limestone formations, indicating deep-sea conditions. The coal formations in both countries indicate, according to all reliable observers, a gradual sinking of the earth's crust and simultaneous building up of the land by sediments of sand and mud, so that successive terrestrial growths and land-surfaces are marked by each bed of coal, excepting indeed in those cases that may arise from floating vegetation gradually getting water-logged and sinking onto an estuarine floor. These long-prevailing conditions are represented by thousands of feet of rocks. It is evident from the lithological resemblances, the occurrence of minerals in the same form and from similar orders of succession, that the physiographic conditions producing them were of world-wide extent and vast continuance. The character of the flora was the same in the arctic as in the temperate regions, and doubtless the maintenance of similar geographic conditions was one element in this result.

If, again, we cast our eye over the description of the Trias of Great Britain and that of the United States, the same characteristics occur in both. They are not at all like the Carboniferous, which they succeed through the Permian phase; yet, though the broad Atlantic now divides them, they indicate like physiographical conditions—strange to say, they are even of the same colour, deep red and grey, and the characteristic fossils are footprints of reptiles. Red sandstone, red and grey marls, and beds of salt

predominate. Gypsum is a characteristic mineral. There is strong evidence of inland salt lakes, lagoons, and tidal shores.

It is often said, and with great force, that it is impossible to determine the age of a rock or the formation to which it belongs by its lithology. That is true, looking at the rocks separately and individually, for the same individual conditions occur in all the ages, but notwithstanding this there are broad lithological resemblances or groupings, what is called *facies* when applied to the organic remains. There are coal beds in the Western States of North America, but no one would mistake the Laramie or Tertiary rocks of those countries for Carboniferous any more than the Oolite rocks in Scotland, in which coal is found, could be mistaken for the Coal-measures; nor yet the Lias, though ironstone is found and worked to a large extent in the Cleveland district of Yorkshire. There may be land and sea conditions and deep-sea conditions represented in the rocks of any age, but independently of the fossils there are characteristic rock-groups which distinguish the one period from the other.

Even so superficial a stratum as the Drift has common characteristics over Britain, Europe, Asia, and North America, a fact which does not merely point towards analogous climatic conditions but towards similar physiographic conditions on a large scale. Indeed, some geologists think the Glacial period resulted from the physiographic conditions which then obtained. Then again, to come towards historic time, the post-Glacial deposits that fringe our coasts, the estuarine clays and forest-beds have a striking resemblance over very large areas, and even continents. Indeed, so important seemed these characteristic similarities and differences to our predecessors in the science of Geology, that they constructed, wiped out, and reconstructed the earth's surface and crust again and again by the aid of cataclysmic convulsions. The influence of Lyell was all towards showing the fallacy of these inferences, but then the pendulum swung too much the other way, and we were led to dwell inordinately on continuity and recurrence.

The uniformitarian theory was a great boon in bringing men's minds down from the region of pure imagination to that of fact. It was a good working hypothesis, but, like most generalisations, was a little one-sided. It represented too exclusively one side of the phenomena of the earth's history to the neglect of another—that of development. I am not one of those who think the doctrine of uniformity a "fetish," but rather a theory that kept us within the range of practical fact. For my part, I am prepared to accept anything that can be *proved*, but until the proof is forthcoming we are on safe ground if we confine ourselves to agencies of which we have experience.

T. Mellard Reade.

(To be continued.)

SOME NEW BOOKS.

DISCOVERY OF LAKES RUDOLF AND STEFANIE, a narrative of Count Samuel Teleki's exploring and hunting expedition in Eastern Equatorial Africa in 1887 and 1888. By his companion Lieut. Ludwig von Höhnel. Translated by Nancy Bell (N. d'Anvers). 2 vols. London: Longmans, 1894. Price 42s.

THE lately issued English translation of Ritter v. Höhnel's narrative of the discovery of Lakes Rudolf and Stefanie has brought this most interesting expedition before the notice of the British public, and gives us an opportunity of taking stock of the principal results arrived at. It will be obvious to any one who reads the volume, or who knows anything of the history of the business, that Count Teleki's main object in starting it was "sport"—that is, the killing and slaying of the larger mammals. The hunting passion is still so prevalent, even among the most highly-civilised races of mankind, that parties of American, German, Austrian, Italian, and, above all, British sportsmen are every year engaged in hewing their way into the still untraversed interior of the Ethiopian Continent in order to shoot the elephant, the giraffe, the rhinoceros, and specimens of the hundred species of antelopes by which it is so abundantly tenanted. To give some idea of the exuberance of mammal life in Africa as soon as the ordinary tracks are left, we have only to turn to the writings of any of the latest explorers. Vice-Consul Sharpe, speaking of a recent journey to Lake Mweru (*Geogr. Journ.*, vol. i., p. 524) says, "I doubt if game can, anywhere in Central Africa, be more plentiful than in the Mweru and Luapula countries, though there may be districts which have a greater variety. *Cobus vardoni* and *C. lechee* run in enormous herds. Buffaloes and zebras are also there in vast quantities." Captain Swayne gives a similar account of the abundance of larger animals in the interior of Somali-land, which he has lately visited.

In the present instance, however, unlike some of his brother shooters, Count Teleki was not wholly devoted to sport. He had the good sense to take with him Ludwig, Ritter von Höhnel, a lieutenant in the Austrian navy, whose acknowledged abilities have converted what was planned primarily as a sporting excursion into a scientific expedition of first-rate importance, it having resulted in the discovery of two new large African lakes previously only known in Europe from vague rumours.

Count Teleki and Lieutenant von Höhnel started from Zanzibar with a full equipment, including, among other articles, a large canvas boat, in February, 1887. Landing at Pangani, in German territory, they made the best of their way to Taveta, the "hunter's paradise" as it has been termed, at the foot of Mount Kilima-njaro. Hence, a slight *détour* to Kilima-njaro and the adjoining smaller volcanic crater of Meru was successfully carried out. The summit of Kilima-njaro was not quite attained, but Count Teleki reached a (calculated) height of 17,387 feet, and a collection of the highest-growing plants was made.

On July 15 the expedition finally left Taveta on its way north, passing round the eastern slopes of Kilima-njaro, and thence taking nearly the same route through Masai-land as that traversed by Mr. Joseph Thomson in the Royal Geographical Society's East African Expedition of 1883-4. In Kikuyu-land, where much difficulty was experienced with the natives, the party diverged to the right, and in the beginning of October reached Ndoro, at the eastern base of Mount Kenia, where they were glad to have a short respite from their laborious journey. The tents here were pitched facing Mount Kenia, so that the travellers might look at its snowy summit whenever the canopy of clouds which generally shrouds it from mortal eyes was lifted. The western side of Mount Kenia is so steep that snow can only remain on it here and there, and it is therefore termed by the Masai the "Oldonyo egeré," or "spotted mountain." From this station Count Teleki attempted an ascent of Mount Kenia. This mountain is surrounded by a belt of dense forest, through which the explorer and his men had to cut their way. At a height of about 10,100 feet the bamboo thicket was left behind; 500 feet higher the trees became much less numerous, and beyond that height only a few isolated specimens occurred. As on Kilima-njaro so on Mount Kenia, the characteristic tree is a gigantic Groundsel, allied to, but not identical with, *Senecio Johnstoni*, discovered by Mr. H. H. Johnston on the former mountain.[1] When, however, the English translator tells us that a "humming-bird was found on Kilima-njaro at a height of 13,100 feet," we are compelled to refer to the German original for an explanation, and find, as we had anticipated, that the bird in question was not a Humming-bird but a Sun-bird (*Nectarinia*), and probably *N. johnstoni*, a specimen of which has lately been brought back from Mount Kenia by Dr. J. W. Gregory. At the height of about 15,000 feet Count Teleki had a good view of the crater of Kenia, which he calculated to be from 1,000 to 1,200 feet in circumference. The bottom of the crater lay some 600 feet lower than the rim, covered with snow and ice. Count Teleki made the excursion to Kenia alone, his companion being left at Ndoro to recover his health, which had suffered much from the journey.

On November 4, after a month's stay at Ndoro, v. Höhnel's health was re-established, and he left to join Count Teleki, who had advanced a little for shooting purposes. Proceeding north-west over the spurs of the Aberdare Range, which lay to their left, at an elevation of about 7,000 feet above the sea-level, the united party crossed the Equator on November 9, and found themselves in a district inhabited by numerous bands of Masai, with whom, however, they managed to keep on good terms. Proceeding a little further, it was determined that Count Teleki should go direct to Lake Baringo with the main body of the caravan, while v. Höhnel should make a diversion eastwards, through Leikipia, as this high steppe is called, and follow up the river Guaso Nyiro, which flows to the north after rising from Mount Kenia and the Aberdare Range. Von Höhnel pursued the course of this hitherto unexplored stream for several days, when he was compelled by bad health to turn back and rejoin his companion at what Mr. Joseph Thomson calls "the pleasant resting place" of Nyemps situated near the south end of Lake Baringo. Here a prolonged stay of two months was made, during which period various

[1] Mr. E. G. Baker kindly informs me that the Senecio of Kenia is new, and will shortly be published as *S. Keniensis*. There are, therefore, now known four of these curious arborescent groundsels of the African mountains—namely, *S. gigas*, from Abyssinia, *S. Manni*, from the Cameroons, and the two above mentioned.

sporting excursions were carried out by the Count, while von Höhnel was again brought nearly to death's door by fever and dysentery.

We now come to the adventures detailed in the second volume of the English translation, which are of greater interest, as relating to a country previously unexplored. The new lakes lying north of Baringo had been heard of by Joseph Thomson, and are conjecturally represented in the route-map of his East African expedition. A bold push was also made towards them by Messrs. Jackson and Gedge during their journey to Uganda through Masai-land in 1889-90, subsequent to Count Teleki's expedition. But these explorers were turned back by the natives in the plains of Ngaboto, in consequence, as they believe, of the bad behaviour of some members of Count Teleki's party so that, in fact, Count Teleki and Lieutenant v. Höhnel are the only Europeans that have, as yet, visited Lakes Rudolf and Stefanie.

After their halt at Lake Baringo the party finally left for the north in the beginning of February, 1888, and soon found themselves in difficulties from want of water. They would have been also miserably short of provisions had it not been for the successful shooting by Count Teleki. Quantities of game, chiefly buffaloes and rhinoceroses, were met with on the plateau now crossed, which was bordered on the east by a new chain of mountains—named General Matthews' range, after the well-known premier of Zanzibar. After about fifty miles transit over the waterless steppe, a half-dried-up swamp was found at the base of Mount Nyiro containing "a little thick, green, slimy fluid" with which the travellers were at last able to wet their lips. Here also the first natives of the Samburu district were met with. They are a pastoral race, called Burkeneji, closely related to the Masai in genealogy and language. According to their information the two much-looked-for lakes were not far off to the north. A few days later, accordingly, the party arrived within view of the southern end of Lake Rudolf. Before them lay a district entirely covered with black streams of lava and dotted over with extinct craters, while from one conical mountain (afterwards named the Teleki volcano) ceaseless clouds of smoke arose. Almost at their last gasp, the travellers rushed on down to the water which lay before them clear as crystal, but to their bitter disappointment when they reached it found it to be saline. Nevertheless, it afforded them a refreshing bath, and upon treatment with tartaric acid was sufficiently improved to quench their thirst. Proceeding onwards from March 6 for nearly a month, the travellers pursued their route northwards along the eastern shores of Lake Rudolf through a miserable and thirsty country. The large "Crater-Island" in the lake which they passed presented an extraordinary appearance, being made up of a series of apparently extinct volcanoes, some of which rose to a height of 400 feet. Further north, fortunately, the country improved a little, and sufficient game was obtained to support the lives of the party, while the discovery of a small stream of fresh water flowing from Mount Kulall, apparently an extinct volcano of about 4,000 feet in altitude, materially improved their position. Further on, hippopotami and elephants were found, while the shore was haunted by immense numbers of lake-loving birds. At length, after fifty-four days' wandering in an all but uninhabited land, nearly bare of fresh water and vegetation, the travellers found themselves among the Reshiat, a pastoral race of Gallas that inhabit the northern shores of Lake Rudolf. Beyond them stretched a flat tract of country overgrown by impenetrable forest, from which a five-peaked mountain-mass (Nakuâ) and other heights rose in the distance. On the whole, the month's stay among the Reshiat was a peaceful

one, and the travellers had every reason to be satisfied with their reception, although their plans for returning southwards on the other side of the lake were not approved of and could not ultimately be carried out. They managed, however, to effect a successful reconnaissance of Lake Stefanie, a much smaller sheet of water, about 70 miles long and from 13 to 15 broad, which lies to the east of the northern end of Lake Rudolf. Like Lake Rudolf, Lake Stefanie is brackish, and has apparently no exit; its altitude was calculated at 1,740 feet, about 440 feet above that of Lake Rudolf. There are said to be two villages belonging to one of the Galla tribes near the northern shore of Lake Stefanie, where a small stream runs into it, but the adjoining district is almost uninhabited.

After a month's stay among the Reshiat, Count Teleki and his followers turned homewards, pursuing the same track along the eastern shore of Lake Rudolf, but then diverging westwards through the districts of Turkana and Suk, to their former quarters on Lake Baringo. In their return hence to the coast we need not follow them; it will suffice to say that they reached Mombasa on the morning of October 24, 1888, after about twenty-one months' absence.

The scientific results of Count Teleki's expedition have been by no means inconsiderable. The geographical and ethnological information amassed has been worked up by v. Höhnel, and published in a special part of *Petermanns Mittheilungen*, while the Geological observations have appeared in the *Denkschriften* of the Imperial Academy of Sciences of Vienna. The Zoological and Botanical collections have been entrusted to various specialists, and memoirs upon them have appeared in the *Sitzungsberichte* of the same Academy. More than a thousand specimens of Coleoptera were collected by v. Höhnel during the journey, and are referred to 247 species, among which nearly one-fourth are found to be new to science. Dr. Schweinfurth, of Berlin, a well-known authority on African Botany, undertook the flowering plants, among which we see registered *Senecio serra*, "the most elevated flowering-plant on Kilima-njaro, growing at a height of 16,278 feet, between volcanic ashes and rocks"; a curious new *Lobelia* (*L. Telekii*) from the western slope of Kenia, and a new form of Compositæ (*Hōhnelia*) from Ndoro.

Of the mammals met with during the expedition, no account seems to be given except Count Teleki's "game list," and certain incidental remarks in the narrative, upon some of which a few comments may be added. The supposed new rhinoceros found on Lake Rudolf is not very clearly described. Its chief peculiarity is its "small head" and diminutive size, being one-third less than that of the ordinary *R. bicornis*. On the other hand, the horns are "finer and more pointed" and much more flattened. The dimensions of the horns are not given, nor is it stated whether specimens of them were brought home. The occurrence of Grévy's Zebra (*Equus grevyi*) on the shores of Lake Rudolf is a fact of much interest, as it shows that the fauna of the Shoa Highlands descends nearly to this district. Originally described from a living animal sent to Paris by King Menelek, this zebra has recently been obtained by Captain Swayne and other explorers in the interior of Somali-land. But Ritter von Höhnel might as well have given us an original drawing of this zebra, instead of copying (without acknowledgment) the figure of the flat skin published in 1890 in the *Proceedings* of the Zoological Society of London. The "unknown Gazelle" of which the horns are figured (vol. i., p. 256, in the English translation) would appear to be Grant's

Gazelle (*Gazella granti*), which is mentioned by name in other passages of the narrative. The so-called "Kaama" antelope met with in the Baringo district (vol. ii., p. 11), is, no doubt, *Bubalis jacksoni*, lately described by Mr. Oldfield Thomas. The occurrence of Waller's Gazelle (*Lithocranius walleri*) in the vicinity of Lake Rudolf is likewise of much interest, especially the observation that it "so much resembles the giraffe in its long neck and sloping haunches" that the explorers named it the "Giraffe-antelope."

On the whole, the English translation of v. Höhnel's volume is fair enough and reads well. The paper, the printing, and the impressions of the numerous illustrations are certainly superior to those of the German original. But its exactness can by no means be relied upon, and some ridiculous blunders are made as regards the scientific terms. We are, for instance, introduced to "humming-birds" and "tigers" in Africa, the horns of an antelope are called "antlers," and we are told that *Hydnora* belongs to the "Cytinaceæ genus." The proofs of the sheets should have been submitted to someone who had an elementary knowledge of Natural History.

P. L. SCLATER.

MAN, THE PRIMEVAL SAVAGE: His Haunts and Relics from the Hill-tops of Bedfordshire to Blackwall. By Worthington G. Smith, F.A.I., F.L.S. With two hundred and forty-two illustrations by the author. 8vo. Pp. xvi., 350. London: Stanford, 1894. Price 10s. 6d.

MR. WORTHINGTON SMITH has done yeoman's service for the elucidation of the history of primitive man in this country, and he has now made us still further his debtors by collecting together his contributions to various journals, and filling in the details of those discoveries which he has from time to time brought briefly to our notice.

The first part of the book contains an account of the lake-side dwelling of man, with his weapons of stone, found at Caddington, near Dunstable, of which the first description was contributed by Mr. Smith to NATURAL SCIENCE for November, 1892. The latter part describes some of the relics of primitive man as found on the banks of the Lea, from its source near Dunstable, in Bedfordshire, to London; together with a description of a primeval living place, or Palæolithic floor, at Stoke Newington, London, of which, it will be remembered, the first notice appeared some years ago in *Nature*.

But the book is by no means a mere reprint; the majority of it is entirely new and rewritten; the facts remain, added to and supplemented by Mr. Smith's later discoveries and re-examinations.

By "primeval savages," Mr. Smith means Palæolithic men—the men who designed, made, and used the most ancient of all stone weapons and tools recognised as such by antiquaries and anthropologists. Our knowledge of the primeval savage and his mode of life is at present little better than a shadow. But his weapons are common and well-known, and from these weapons we may gather much that is instructive of the men who formed and used them. The author believes that "man did not live in what is now Britain before what is known as the last great Glacial period."

In his introductory chapter, Mr. Worthington Smith gives a sketch of the more important osseous remains of primitive men yet discovered, and furnishes illustrations of the Neanderthal, Canstadt, Spy, Engis, Cro-Magnon, Grenelle, Furfooz, and Truchère skulls and crania, comparing them with those of present-day

Melanesians and Chimpanzees, and showing the great physical and mental gulf between man and any living member of the anthropoid apes. Of course such material as we possess is quite insufficient to permit of any accurate deductions, and it is only by the patient and careful research of such investigators as Mr. Smith that we can hope to obtain the light necessary to partially solve this important and interesting question. After a brief and vivid, though not particularly pleasing, sketch of the habits and life of our ancestors in their wild state and savage homes, Mr. Smith plunges into the main subject of his work, Caddington.

Caddington, the site of the discovery of an undisturbed living and working place of primeval man, is thirty miles from London, and the excavations, specially made for Mr. Smith, are half a mile west of the village, in so-called brick-earth resting upon chalk-rock, about 600 feet above Ordnance datum. The nature of the deposits and the contour of the country around leads Mr. Smith to the conclusion that in former times the higher ground bordered a lake, or series of large and confluent pools of water.

Chapter IV. treats of the geology of the Caddington position, the nature of the deposits, and of the old Palæolithic floor, with the flints found thereon. Of the flakes found on this floor Mr. Smith has replaced more than 500 on to either other flakes, or more or less perfect implements, or cores, from which they were originally struck off. This proves that the remains left behind by the inhabitants of this part have suffered little or no disturbance since their time.

Chapter V. is devoted to the story of the discovery of implements in the Dunstable district, and the gradual tracking of the gravel which contained them to various pits and excavations, until at last the reward came, and the Caddington gravel pits yielded up their treasures. There are two or more types of implements described by Mr. Smith. The first, or "ochreous" type, which came from the upper brown stony clay, and which are all slightly abraded, and the white, lustrous, and sharp-edged type found on the Palæolithic floor itself. The evidence at hand goes to show that the latter are the more recent of the two sets.

It was in May, 1889, while making an examination of the exposed vertical bank of a clay pit, that Mr. Smith noticed indications of a horizontal streak in the brick-earth, at about six feet from the surface. He had previously suspected the existence of a floor, or working site, from the fact that the sharp and unabraded chips of flint were usually met with in the fallen material. In this streak, he here and there noticed a stone embedded, and at one place saw a small flint half projecting from the face of the excavation. On removing the flake, he found it to be a beautifully made scraper, and saw that it was identical with one he had found two years before in the same locality. More discoveries followed, but it was not until the end of March, 1890, that he established the fact of the finding of a Palæolithic floor or living surface, similar to the one discovered by him several years previously at Stoke Newington.

The succeeding sixty pages of the book are taken up with a description of the flint implements and flakes found on this Palæolithic floor, beautifully illustrated by Mr. Smith's own drawings, and showing the actual implements themselves, as well as the restoration of them to the rough block from which, in many cases, they were made, by the fitting on of chipped fragments found associated with them.

In Chapter X. we have a description of the Caddington position in

Palæolithic times, and in Chapter XI. a discussion as to the age of the implements themselves. Pages 179-306 deal with "Traces of primeval man," and contain a record of discoveries of implements found between Bedfordshire and London, dozens of which are figured and all of which are of Palæolithic age. The rest of Mr. Smith's work treats of Neolithic weapons which have been found from time to time over the same area, together with notes on earthworks, roads, and track-ways near Dunstable; early British hut-remains, deneholes, graves, tumuli and skeletons from Dunstable Downs. In the above imperfect sketch we have endeavoured to show the contents of this interesting book, and have not pretended to offer any critical remarks. The book is emphatically one to buy, to read, and to ponder over. From it those interested in the early history of their race will learn much, while the sketch map will enable them to follow the author in his topographical details, and assist them to obtain an excellent idea of the mode and conditions of life of their remote ancestors. Of the illustrations of flint implements we need scarcely speak, suffice it to say that they are Mr. Worthington Smith's, and that the variety and number of them should enable many a tyro to supplement our knowledge of their distribution.

We observe with pleasure, from a circular recently received, that Mr. Smith will conduct an excursion of the Geologists' Association to Caddington on May 26, in order to explain to them the structure of the district and his discoveries. May we be there to see!

Horned Reptiles in Scotland.

Mr[illegible] tio[illegible]

Some New Reptiles from the Elgin Sandstones. By E. T. Newton. *Phil. Trans.*, vol. clxxxiv., B., pp. 431-503, pls. xxvi-xli. (1893).

For some years past, palæontologists have been tantalised with reports of the occurrence of remains of reptiles allied to the Dicynodonts of Africa and India in the Triassic sandstones of Elgin; but with the dilatoriness of most Government departments—whether it be in deciding to build ships or to describe fossils—the Geological Survey of Scotland, to whom the specimens belonged or were confided, maintained a strange reserve on the subject. After a protracted period of patient (or shall we say impatient?) suspense, those interested in the subject have, however, been rewarded with the appearance of a memoir on the remains in question, which leaves little to be desired in point of completeness or in wealth of illustration; and we here beg to offer our congratulations to the Council of the Royal Society in having displayed such a wise liberality to the author in the way of plates.

Not only does the memoir before us fully endorse the aforesaid reports as to the occurrence of Anomodont reptiles in the Elgin sandstones, but it likewise shows that these animals were represented by numerous and diversified types, all of which were, however, more or less closely allied to South African forms. Unlike the African representatives of the group, which for the most part occur in the form of well-preserved skulls and limb-bones, the Elgin specimens when placed in the hands of the author of the memoir before us were in the shape of most unpromising impressions. Fortunately, he hit upon the plan of taking gutta-percha casts from these natural moulds, with the result that, after much patient labour, he succeeded in restoring the skulls of these ancient reptiles in a manner which makes their characters fully apparent; and it is from such casts that most of the figures in the memoir have been drawn.

The first remains that the author takes in hand are certain skulls

indicating reptiles closely allied to the typical African genus *Dicynodon*, which are referred to several distinct species. He is, however, of opinion that they cannot be included in that genus, and accordingly refers them to a new one, for which the name *Gordonia* is suggested; but we have considerable misgivings whether the features he regards as of generic value are really entitled to such importance. Be this as it may, we cannot help thinking that in giving to each species of this reputed genus the name of a person, the author has paid a poor compliment to him in whose honour the genus itself is named. If a genus founded on a personal name means anything at all, it means that the animals so named are dedicated to the person whose name is thus employed. Accordingly, *Gordonia* will signify Gordon's reptiles; but if we are to have *Gordonia traquairi*, *Gordonia huxleyana*, etc., etc., the system of what we may call personal nomenclature appears, to our thinking, reduced to an absurdity. We are not fond of personal nomenclature at all, but the degree to which it is carried here is certainly excessive, seeing that, out of the nine forms described, seven have such names both generically and specifically; while an eighth has its generic name alone thus compounded.

As the so-called *Gordonia* is closely allied to *Dicynodon*, so the skull described as *Geikia* displays equally close affinities with *Lystrosaurus* (*Ptychognathus*); but if palæontologists are right in keeping, on account of the absence of tusks, *Udenodon* apart from *Dicynodon*, the author will be justified in giving a new name to the tuskless Elgin form. We wish, however, that he would not continue to employ names like *Ptychognathus*, which, on account of preoccupation, are shown to be inadmissible; and he need not have gone out of his way here, and in the case of *Pariasaurus*, to ignore the most elementary principles of classical transliteration, by an adherence to an incorrect way of spelling which has been put right in some of the very works he quotes.

Perhaps the most interesting of all the forms described is the remarkable horned skull designated *Elginia*, two of the figures of which are here reproduced on a greatly reduced scale. After describing the characters of this extraordinary skull, which, it will be observed, has the whole of the temporal region completely roofed over, the author proceeds to discuss its affinities. Here he, unfortunately, displays such a total want of grasp of the relationships of the various recent groups of reptiles as cannot but detract from the value of his deductions. He says, for instance, that "although the Elgin Reptile undoubtedly possesses several important Lacertilian characters, and apparently finds its nearest living allies in that group, yet it presents peculiarities of structure which prevents its being referred to the Lacertilia." Seeing that it has a fixed quadrate, a lower temporal arcade, and pterygoids meeting in the middle line, we perfectly agree with the latter part of this sentence. Imagine, however, our astonishment when, after reading a few lines further, we found that the author actually includes the living New Zealand *Sphenodon* among Lacertilia, although he has separated it from the true Lacertilia. This is about as good as if some new shining light in zoology were to propose to class tigers and elephants in the same order!

When an author appears so incapable as this of grasping the relative importance of structural features in recent reptiles, it is not much good attempting to follow him in his discussions on fossils. He considers, however, that *Elginia* has its nearest ally in the gigantic *Pariasaurus* of the South African Karoo beds; and in this we believe he is right. When, however, he proceeds to observe that both

have little or no affinity with the Anomodonts, and show Lacertilian resemblances, we are again so staggered that, did we not recall the circumstance that he includes *Sphenodon* among Lizards, we can hardly realise that he intends to be serious. Has the author, we may ask, ever compared the pelvis and humerus of *Pariasaurus* with those of a typical Anomodont? and if so, does he still hold the opinion he expresses? In the same section we find a great deal of discussion as to which apertures are the posterior nares in the skulls of the African and Elgin forms, and much importance is attached to the conclusion reached. We would, however, remind the author that the Chelonia and Crocodilia show that the position of these apertures is of extremely little classificatory import. Even if he be right in stating that they have the same position as in *Sphenodon*, this in no way



Superior and right lateral aspects of cranium of *Elginia mirabilis*, together with a side view of the anterior teeth. About one-quarter natural size.

shows that the forms under discussion are more nearly related to Lizards than to Anomodonts; since we thought every tyro knew the structural resemblances between the tuatera and the last-named group.

Although we regret we cannot congratulate the author on the manner in which he has handled an admittedly difficult subject, yet science is indebted to him for having so carefully restored the skulls of the Elgin Anomodonts, and for having provided us with figures which admit of our drawing our own conclusions.

We may add that the close similarity of the Elgin fauna to that of the African Karoo beds, and the Indian Panchet and Maleri beds, serves to confirm the original view as to the Triassic age of both the latter.

R. L.

THE AUSTRALIAN MUD-FISH.

TRAVELS FOR ZOOLOGICAL RESEARCH IN AUSTRALIA AND THE MALAY ARCHIPELAGO. [Zoologische Forschungsreisen in Australien und dem malayischen Archipel.] Pts. I. II. Edited by Richard Semon. Jena: Gustav Fischer, 1893.

THE munificence of Dr. Paul von Ritter, founder of the Professorship of Phylogeny and Theories of Descent at Jena, has already rendered signal service to Biological Science. We have often referred to the researches of the Ritter Professor, Dr. W. Kükenthal, which are of fundamental importance for the understanding of the vertebrate animals. We now have to welcome the first instalment of another great work rendered possible by the same generous patron. Between June, 1891, and the end of the following year, Professor Richard Semon, of the University of Jena, was enabled to undertake one of the most important scientific expeditions for collecting that has hitherto been made, to Australia and the Malay Archipelago. Now, the collections have been assorted and arranged, and the publication of the results is commenced in a series of fine quarto volumes. Dr. Semon estimates that there will be five volumes devoted to the Vertebrata, the first relating to the *Ceratodus*, the second and third to the Monotremes and Marsupials, the fourth and fifth to Sirenians, Edentates, *Amphioxus*, and miscellaneous other types.

Professor Haeckel appropriately prefaces the work with some general observations on the relationship of the Australian Fauna, and then Dr. Semon contributes a brief sketch of his journey, enumerating the various centres at which collections were made. Biologists, however, will turn at once with greatest interest to the first part of Dr. Semon's detailed monograph of *Ceratodus*, which is destined to inaugurate a new era in our knowledge of the recent mud-fishes. As patriotic Britons we peruse it with mingled feelings; for had not the collector despatched ten years ago by the Royal Society of London and the University of Cambridge betrayed his trust, the credit of unravelling these problems would have belonged to our own nation.

It is impossible to do justice to Dr. Semon's work on the Australian mud-fish in a brief notice; and it is too early to discuss the conclusions, only the first stages in the life-history of the animal being as yet described in a general way. The facts in reference to the distribution, mode of life, and propagation of *Ceratodus* are set forth in a more exhaustive manner than by previous observers; and the embryological part is illustrated by exquisite plates of segmenting eggs and of immature fishes. *Ceratodus* is believed to be confined to the Burnett and Mary rivers, Queensland; and although, as already generally known, it crops the vegetation with its great teeth, the most important part of its food consists in the small animals living between the fronds and leaves. It seems to be quite powerless to move on land, and it never lies dormant in "cocoons." The eggs are laid separately and loosely among the vegetation; the fore-limbs begin to appear at the end of about fourteen days, and the hind-limbs not before two-and-a-half months. In the course of development, *Ceratodus* presents most resemblance to the Cyclostomes and the Amphibia; but it is noteworthy that external gills are absent at all stages, and there is no trace of a suctorial mouth. There is no rudiment of a connection between the pectoral and pelvic fins; and many features are entirely peculiar. The large dental plates result from the fusions of small teeth, exactly as might have been expected from what is known of extinct mud-fishes. As to the all-important

question of the mode of development of the limbs, we must be content to wait for the details to be published later; we can only hope that the delay in issuing the remaining sections of the volume will not be long.

Geography as it is Taught.

Philip's Systematic Atlas, Physical and Political, specially designed for the use of Higher Schools and Private Students. Containing over 250 maps and diagrams, in 52 plates, with an introduction and index of 12,000 names. By E. G. Ravenstein, F.R.G.S. London: George Philip & Son, 1894.

This astonishing production is, say Messrs. J. Scott Keltie, H. J. Mackinder, and E. G. Ravenstein, in a preface, "intended to meet the requirements of pupils in higher schools, of teachers and of other students of geography, for whom neither the ordinary school atlas nor the general reference atlas is entirely adequate." If, then, this atlas represents in any way the methods of teaching favoured by these gentlemen, we are sincerely sorry for any students who come under the hands of Messrs. J. Scott Keltie, H. J. Mackinder, and E. G. Ravenstein. Geography, as certainly Mr. Scott Keltie and Mr. Ravenstein ought to know, is much too important and interesting a science to be distorted and obscured by the wholesale introduction of imperfect text-book information from other sciences. There are, for instance, two plates supposed to illustrate the geographical distribution of plants and of animals. On one plate there are eleven separate maps, on the other nineteen, most including at least a hemisphere and illustrating such points as the range of rhubarb, the Cassowary tree, "nut" trees, Wallace's zoogeographical regions, serpents and lizards, the beaver, and the orang-utang, the orders of mammals, and so forth. It is needless to say that it is impossible to give correct information on such a scale, and that the maps as they appear are useless to the zoologist and botanist and pernicious to the general student. There is, again, a "geological" map of Europe on the scale of 637 miles to the inch, coloured to show "Cainozoic and Recent," "Mesozoic," "Palæozoic," and "Igneous" formations. Two little hemispheres of about three inches diameter show the religions of mankind under the simple if unsatisfactory headings "Heathen, Hindu, Bhuddist (*sic*), Mohammedans, Christians." Two similar maps show the races of mankind—"Aryans, Semites, Hamites, Mongolians, Malays, Hottentots, Negroes, Australians and Papuas, Eskimo, American Indians, Mixed." Do Messrs. Scott Keltie, Mackinder, and Ravenstein regard these tit-bits of inaccurate folly as geography? They certainly are neither geology nor anthropology.

This desire to crowd in a senseless appearance of information reaches even the topographical plates. Thus in a corner of the plate entitled Great Britain and Ireland is a "map" of London on the scale of three miles to the inch, with the following eight references:—1. Zoological Gardens; 2. Kensington Museum; 3. National Gallery; 4. Westminster Abbey; 5. British Museum; 6. Law Courts; 7. St. Paul's Cathedral; 8. The Tower. For whose benefit is this, and why the invidious exclusion of the Houses of Parliament and Madame Tussaud's?

Apart from the singularly incompetent choice of subjects and editing, the plates are clearly printed and, at least by daylight, well coloured. The physical maps are specially useful; the political maps are up to date; the maps showing densities of population are of a kind to please those people who like to think they know

about things. The commercial maps are not so satisfactory; it is difficult to follow the highways of commerce by land and by sea upon them. The editors say that, in the construction of the maps showing the distribution of plants and animals, "particular attention has been paid to such products as are useful to man and enter largely into commerce." We have already referred to the absurd inadequacy of these, but in the introduction, as distinguished from the preface, the contradictory statement is made that "our maps show the supposed native countries of these plants" (*i.e.*, characteristic plants and those of economic importance), "and not the countries in which they are being cultivated at the present time." Messrs. Scott Keltie, Mackinder, and Ravenstein have associated their names with what is in many places a mere burlesque of the great science of geography.

Dredging and Distribution in Danish Seas.

Det videnskabelige Udbytte af Kanonbaaden "Hauchs" Tvogter i de danske Have indenfor Skagen i Aarene, 1883-86 (Chef: Premierlieutenant C. F. Drechsel). Udgivet paa Bekostning af Ministeriet for Kirke- og Undervisningsvæsenet ved C. G. Johan Petersen. Pp. 464, 6 pls. and 2 maps, 4to; atlas of 43 maps, fol. Kjöbenhaven, 1889-93.

This work, which has just been published under the auspices of the Danish Government, is one of no common interest for those who believe that a careful investigation of the conditions affecting the distribution of common animals within a limited area is likely to be as much utility in the solution of important biological problems as the collection of new species from far-off lands.

In the year 1883, Captain Drechsel applied to the authorities of the Zoological Museum in Copenhagen to recommend him a naturalist who would spend a few weeks on board his ship making collections for the Museum and investigating the animal life of the Danish seas in general. The choice fell upon Dr. Petersen, and has been amply justified by the results of his work published in the volume now under notice.

During the first season Captain Drechsel and Dr. Petersen elaborated a plan of campaign, which was "not merely to seek for species new to the fauna of the district, but first and foremost to acquire a knowledge of the distribution of the commonest animals, and at the same time to investigate as far as possible those natural conditions, such as temperature, composition of the bottom, etc., which might be supposed to exercise an influence upon the distribution and life of the animals. This plan was carried out in the course of the three following summers, 1884-86, and, with this object in view, dredgings were made at as many stations as possible in the Kattegat, so that on the basis of these observations conclusions might be drawn regarding the distribution in those waters of at all events the commonest animals, especially the invertebrata." Wherever possible both dredge and trawl were used, and the process was carried on till it became possible to predict with a fair amount of certainty what species would be obtained from any particular haul.

More than 500 stations were occupied, and in almost every case, in addition to the exact geographical position, there are given the depth, the nature of the bottom, the temperature of the water and its specific gravity both at the surface and the bottom. The material obtained was placed in the hands of different specialists for examination, and their reports are incorporated in the present volume. The

five fascicules of text include memoirs by different specialists—on the Echinodermata and Mollusca, by Dr. Petersen himself; on the Cephalopoda, by Dr. H. J. Posselt; on the Polyzoa, Annulata, Cœlenterata and Sponges, by Inspector Levinsen; on the Crustacea Malacostraca, by Dr. Meinert; on the Diatoms, by Professor Cleve; and on the Bottom Deposits and the Hydrography of the Kattegat, by Dr. Rördam.

Turning first to the chemical and physical portions of the work, we notice that the greatest percentage of calcium carbonate is found in the deepest part of the Kattegat, perhaps in correlation with the saltness of the water, while the deposits from the bottom of the Baltic contain little, if any, of this substance. It appeared that during the three years (1884–86) in which the observations were made, the saltness of the water in the Kattegat increased, while at the same time the mean temperature diminished. This result is confirmed by independent observations from other sources, and is attributed to an unusual prevalence of N. and N.W. winds during those seasons. These facts are shown very clearly by a plate containing nine little maps.

The account of the Echinodermata and Mollusca by Dr. Petersen need not be noticed at length here, except for the purpose of calling attention to the admirable series of 26 maps by which the distribution of the various species is illustrated. No new forms are here described, though some additions to the Danish fauna are recorded. The Mollusca, in particular, have already been discussed by the same author in an elaborate thesis published about five years ago.[1] The section dealing with the Cephalopoda by Dr. Posselt is not merely an account of the work taken by the "Hauch," but is virtually a systematic monograph of the Danish forms, and contains some novel information of great importance. Dr. Meinert's report on the Malacostracous Crustacea is a very important piece of work, being in fact a revision of the Danish fauna, so far as this group of animals is concerned. The number of species recorded is increased from 177 to 253, and nine new species are described, of which three are referred to new genera. The memoir is illustrated by fourteen maps and two copper plates. Inspector Levinsen has dealt with the Porifera, Polyzoa, Hydroida, and Anthozoa, and in regard to the first three of these groups has given us a complete account of the Danish forms, with a very convenient dichotomous key for their identification.

The whole work is completed by a discussion of the general results by Dr. Petersen, which he has considerately printed both in Danish and English; this is naturally the most important section of all and some account of its more salient features must be attempted. The bottom of the sea is covered with three kinds of deposits, stones, sand or gravel, and clay or ooze, the last of which occurs only in the deepest places and is the most unfavourable for animal life. As to vegetation, about half the Kattegat and nearly all the Skagerack are destitute of it, and, next to the inshore waters, the richest places are the protected situations in the Kattegat. The salinity, too, increases with the depth, though it varies greatly with the seasons, the melting ice in the Baltic being, perhaps, the most important factor in this connection. As might have been expected, the temperature is most variable in the inshore waters, where it reaches 19–23° C., and least variable in the Skagerack, the deep eastern Kattegat being intermediate in this respect.

[1] Petersen, "Om de skalbærende Molluskers Udbredningsforhold i de danske Have indenfor Skagen," 8vo., pp. viii., 162, map, Kjöbenhaven; 1888.

The temperature of the bottom mud seems to be somewhat higher than that of the water in winter; for instance, while the water was about 0° C., the temperature 3–4 feet down in the mud was + 7° C. This is doubtless the reason why many animals (Gobiidæ *Carcinus*, *Cerithium*, etc.) bury themselves in winter. The influence of the physical conditions on the distribution of various animals is then discussed, and fourteen examples are given in some detail, special reference being made to different species of the same genus which appear to represent each other in different parts of the area under consideration. The author claims to have shown "that each species is distributed according to certain rules," it is seldom, however, "that one single factor is decisive, but the distribution is the result of groups of concurrent circumstances." One point of interest is brought prominently forward—"no species of animal is found everywhere in our seas." When an animal is once recognised as common it is usual in faunistic works to describe it as found "in all our sounds and fjords" as occurring "everywhere along the Danish shores," so that frequently the more abundant a species the less information is available as to its actual distribution. Dr. Petersen's careful maps furnish a marked contrast to this mode of work. After a short chapter on the distribution of certain animals outside the Danish waters, we have some observations on distribution in the post-Glacial period, from which it appears that the Baltic and Kattegat may be compared to a Norwegian fjord in which the cold water forms are not yet shut in, but in which they and the warm water form may move in and out according to circumstances. As representatives are the "cold fauna" we have *Astarte borealis*, *Idotea entomon*, *Halicryptus spinulosus*, etc., of the "warm fauna" *Ostrea edulis*, *Tapes pullastra*, and *Pecten varius*. This concludes a most important and interesting contribution to the natural history of the Kattegat—"the greatest battlefield on the coast of Europe between the ocean water and the fresh water."

WM. E. HOYLE.

THE British Oniscidæ or "Woodlice"—a group of animals somewhat neglected by naturalists—are described by Dr. R. F. Scharff in the *Irish Naturalist* for January and February, all our native species being well figured on a single plate. Fourteen species are common to Great Britain and Ireland, two occur in the large island which are not found in the smaller, while three species have been found in Ireland which have not yet been observed in England or Scotland. There are not many groups in which Ireland can claim a richer fauna than Great Britain. Foreign species receive attention at the hands of M. Adrien Dollfus, who continues his researches on the Isopoda and their geographical distribution. Four of his papers have recently come to hand, and furnish us with information on the woodlice of Venezuela, the Pyrenees, and the Seychelles.

THE Termite Ants are so constantly being brought forward in discussions on Natural Selection (see Mr. Platt Ball's paper, NATURAL SCIENCE, Feb., 1894, and Mr. Cunningham's in the present number), that those interested in the problem will be glad to know of an exhaustive paper on the constitution and development of the Termite colony, with notes on their habits, which is now begun by Drs. B. Grassi and A. Sandias in *Atti della Accademia Gioenia in Catania* (ser. 4, vol. vi.).

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments have recently been made in the Botanical world:—Mr. R. J. Harvey Gibson to the Professorship of Botany at University College, Liverpool, the endowment of which has recently been completed by a donation from Mr. Holbrook Gaskell; Dr. Carlo Casali to be assistant in Botany at the University of Rome; Dr. C Avetta to be Professor in the University of Padua, and Director of the Botanic Gardens; Mr. J. H. Burrage, of New College, Oxford, to be Demonstrator to Professor Balfour at Edinburgh; Dr. Richard Otto, recently assistant in the Institute of Plant Physiology at the Royal Agricultural School in Berlin, to be teacher of Chemistry at the Royal Pomological Institute at Proskau o/S, while Dr. Fr. Krüger of Geisenheim takes his place at Berlin. We also note that Dr. V. Schiffner will temporarily take the place of Dr. Hallier as an assistant [illegible] the Botanic Garden at Buitenzorg in Java while Dr. Hallier makes a visit [illegible] Borneo; that Professor Julius Klein has gone to Naples to undertake some [illegible]anical studies at the Zoological Station; that Dr. Robert Regel, who has for the last few years been a junior curator in the Herbarium of the Royal Botanic Gardens at St. Petersburg, has resigned this position to become Privatdocent in the University, where he takes classes in the relation of Botany to Horticulture; that Mr. Alboff, who, at the cost of the Boissier Herbarium, has for the last six months been investigating the mountain flora of the Caucasus, has returned with rich spoils; and that Dr. L. Guignard has been elected President of the Botanical Society of France for 1894. We are glad to learn from the *Kew Bulletin* that the Government of Queensland has reconsidered its decision, and that Mr. F. M. Bailey, the abolition of whose post was recently reported, has been re-instated as Colonial Botanist. "It would be poor economy to do away with so important a post, especially when it has been filled so long and ably by a man like Mr. Bailey, who has added largely to our knowledge of the plant products of the colony.

FURTHER good news for Botanists is that the botanical explorer, Captain John Donnell Smith of Baltimore, Md., has opened his private herbarium and library to the students of botany of the Johns Hopkins University. This herbarium is one of the largest and best selected private herbaria in existence, containing not only collections from all parts of the world, but also the plants collected by Mr. Smith and his correspondents in Guatemala, many of which are the type specimens of new species. The owner intends to present both herbarium and library to the University, so soon as a building shall be offered for their reception, and provision made for their maintenance. The library contains about 1,300 volumes, chiefly relating to American botany. Captain Donnell Smith has recently started on another visit to Central America.

MEANWHILE Botanists nearer home are likewise busy, as the following items show. Dr. Baroni of Florence intends to monograph the genus *Atriplex*, and requests materials from his colleagues. The tenth Congress organised by the French National Society of Horticulture will meet at Paris during the General Horticultural Exposition, May 23–28. Among others, the following subjects will be discussed:—Chloro-

phyll in its relation to the vigour of cultivated plants; capillarity and the preparation of the soil; and means for hastening the nitrification of nitrogenous substances and rendering them more easy of assimilation. Under the direction of Professor P. Lachmann a botanical garden has been established in the mountains near Grenoble, at a height of 1,875 metres. Nor should we forget the Royal Botanic Society of London, on whose pecuniary difficulties we commented in our last number (p. 233). We then suggested that they were in part due to the Society's exclusiveness as regarded its Gardens in Regent's Park; but an important step has since been taken by the Council. A regulation has been made which will doubtless increase the popularity of the Gardens. Permission is now given to smoke therein.

AMONG Zoologists the following appointments are to be chronicled:—Dr. F. Zschokke to the Professorship of Zoology at Basle University, in place of Dr. L. Rütimeyer, who has long been in failing health; Dr. K. Zelinka to be Professor-extraordinarius of Zoology at the University of Vienna; Mr. S. H. Reynolds to be Demonstrator of Biology in University College, Bristol; and Dr. Harrison Allen to be Director of the Wistar Institute of Anatomy at the University at Pennsylvania. Professor W. Krause of Göttingen has moved to Berlin (Brückenallee, 31), where he has been placed in charge of the Anatomical Institute of the University. Mr. J. Willis Clark succeeds Professor M. Foster as Rede Lecturer at Cambridge University. We also note that Mr. C. R. Beazley, Fellow of Merton College, has been elected to the Geographical Studentship at Oxford University for the present year; and finally that Dr. Hans Bruno Geinitz, the veteran geologist and palæontologist of Dresden, now in his eightieth year, retires this month from his professorship in the Technical High School, where he will be succeeded by Professor E. Kalkowsky, of Jena.

SEVERAL scientific men have of late received honours from various universities. Cambridge University has conferred the degree of Doctor of Science on Dr. Santiago Ramon y Cajal, Professor of Histology and Pathological Anatomy in the University of Madrid, who, on March 8, delivered the Croonian lecture before the Royal Society, his subject being the minute structure of the nervous system, of which he has done so much to advance our knowledge. On the same day the degree of LL.D. *honoris causa* was conferred by Cambridge University on the Right Hon. the Earl of Kintore, Governor of South Australia, who, it will be remembered, recently travelled from Port Darwin to Adelaide in company with Dr. E. C. Stirling. The Senate of Aberdeen University has conferred the honorary degree of LL.D. on Mr. Henry Ogg Forbes, the well-known explorer, now Director of Museums in Liverpool.

THE following prizes and scholarships are open to competition. The Italian Geological Society offer a prize of 1,800 francs for the best memoir on the present knowledge of the Palæozoic and Mesozoic rocks in Italy. The memoir must be presented by the end of March, 1896. The Royal Society of New South Wales offers its medal and £25 for the best original communication (provided it be of sufficient merit) upon each of the following subjects, to be sent in not later than May 1, 1895,—on the silver ore deposits of New South Wales; on the physiological action of the poison of any Australian snake, spider or tick; on the chemistry of the Australian gums and resins. And the following to be sent in not later than May 1, 1896,—on the origin of multiple hydatids in man; on the occurrence of precious stones in New South Wales, with a description of the deposits in which they are found; on the effect of the Australian climate on the physical development of the Australian-born population. Competitors are requested to write upon foolscap paper, on one side only. A motto must be used instead of the writer's name, which latter must be enclosed in a sealed envelope; all communications to be addressed to the Hon. Secretaries, T. W. E. David and J. H. Maiden, at the Society's House, Sydney. Cambridge University announce that the Arnold

Gerstenberg Studentship, worth about £90 a year, will be awarded next May, on the results of an examination in Psychology and Logic, beginning on May 21. Candidates must have obtained honours in one part of the Natural Sciences Tripos, and have entered into residence before April, 1888. The student elected is required to devote himself to mental or moral philosophy. Guy's Hospital announce that two open scholarships in science, of the value of £150 and £60, will be examined for in September next; particulars may be obtained from the Dean of the Hospital.

Among appointments to be filled up we note the following:—An Assistant-Lectureship in Agriculture at the Durham College of Science, Newcastle-on-Tyne. A practical acquaintance with agriculture is necessary, and a special knowledge of animal anatomy and physiology desirable. The stipend is £150 *per annum*, with a share of certain fees. Applications and testimonials must be sent on or before April 6 to the Secretary, Mr. H. F. Stockdale, from whom further particulars may be obtained.

For many years the idea of a Free Local Museum to illustrate the Natural History, Archæology, etc., of the district of Epping Forest has been advocated by those interested in the subject, and especially by the members of the Essex Field Club. The Epping Forest Committee of the Corporation of London having carefully preserved the old building known as Queen Elizabeth's Lodge at Chingford, they will be approached with a view to the housing and arranging of the proposed collection in the Banqueting Hall of the Lodge. The district which the promoters propose to illustrate is, roughly speaking, that bounded by the Great Northern and Eastern Railway on the west, and the London and Ongar Railway on the east; and is about sixteen miles long by four wide. The Essex Field Club are willing to undertake the gathering of specimens, as well as the curatorial work necessary for the proper arrangement of the collection. A general meeting was held, in Queen Elizabeth's Lodge, on the 24th February, to consider the question. The Rev. A. F. Russell (Rector of Chingford) was in the chair, and speeches were made by Professor C. Stewart, Mr. J. E. Harting, Professor Meldola, Mr. Howard Saunders, Professor Boulger and other well-known naturalists. From a letter we have received from Mr. W. Cole, the Hon. Sec. to the Essex Field Club, we learn that the Conservators have given the Club provisional permission to place specimens in the room; but that it will be necessary to raise a small fund before the Conservators can be approached for full permission to carry out the proposed Museum. It is estimated that the sum of £300 would enable the committee to provide the necessary cases, cabinets, and museum appliances, and, considering the excellence of the idea, this money should be easily obtained. Those who desire to help the committee in furthering the educational value of this delightful forest should communicate with Mr. Cole at Buckhurst Hill.

We learn from the *Daily News* that the Geological Department of the British Museum has recently been enriched by over three hundred fine specimens of fossil plants, from the Coal-measures of Somerset, collected by the late Jas. M'Murtrie of Radstock. The same institution has been presented by the Governors of Cheltenham Hospital with the skeleton that belonged to a young Chimpanzee (*Troglodytes niger*), from Angola, which was dissected by Dr Edward Tyson and described in his work "*Orang-outang, sive Homo Sylvestris*; or the Anatomy of a Pigmie compared with that of a *Monkey*, an *Ape* and a *Man*." This, the first account of any man-like ape that could pretend to scientific accuracy or completeness, was published by the Royal Society in 1699.

The town of Macclesfield has accepted the offer of Miss Marian Brocklehurst to build and endow a Museum in the Public Park. The basis of the collection will

be a large series of Egyptian antiquities obtained by the donor herself while travelling in the East; but it is intended also to add a general collection of Natural History, with special reference to the fauna, flora, and geology of East Cheshire. Miss Brocklehurst proposes, in association with her brother, to arrange for an endowment of £100 per annum, half to be devoted to maintenance, half to the Park Keeper for curating. As the munificent donor requests to be favoured with suggestions, we would venture to remark, from our knowledge of the culture of the average Town Councillor, that no institution of the character she proposes to found can flourish under the direction of a Corporation Committee without the guidance of some educated and experienced curator. Unless adequate provision be made for such an officer, the Museum will become a mere storehouse of "curiosities" of no value to anyone.

We have received the Seventh Annual Report of the Liverpool Marine Biology Committee and their Biological Station at Port Erin, Isle of Man, drawn up by Professor W. A. Herdman. The work of the Committee was transferred to Port Erin from Puffin Island in 1892, and the present report shows how it has benefited by the change. Two dozen workers (not 60, as the report says) have stayed at the station for varying periods during the year. The Curator for most of this period was Mr. J. H. Vanstone, who resigned in September; a new Curator will be appointed by the committee after Easter. A separate aquarium and tank-house have been built for the storage of living marine animals and plants, for facilitating observations on habits and life histories, and for exhibition to the public. During the latter part of the summer a charge of 3d. was made for admission to the aquarium, and this in four weeks produced £1 17s. 10d., from which we infer that one visitor was let in cheap, else did not get his proper change. To make the aquarium more permanently attractive than the come and go of living material renders possible, it is intended to display round the walls a large collection of Manx Invertebrata presented by Mr. G. W. Wood. It was hoped that the Lancashire Sea-Fisheries Committee would by this time have erected a fish hatching-establishment alongside of the Biological Station, especially as Captain Dannevig of the Norwegian hatchery at Flödewig reported favourably on this scheme. Unexpected difficulties have, however, arisen; but the reporter seems confident that sooner or later Port Erin will become an important centre for the propagation of young food-fishes. During 1893 eight dredging expeditions were carried out, mostly under the auspices of a Committee of the British Association, and a considerable amount of the Irish Sea was explored. Over a thousand species of marine animals were collected and identified, of which 38 were new records to the British fauna, 224 new to the particular district, and 17 new to science. The Report contains four plates illustrating the arrangements of the station, a chart of the Irish Sea and a considerable quantity of interesting matter. The expenses of the year amount to about £180, in return for which a great deal of good work has been done; but we regret to see that this is not quite covered by the yearly income. Subscriptions may be sent to Mr. I. C. Thompson, 19, Waverley Road, Liverpool, while applications to be allowed to work at the station or for specimens should be addressed to Professor Herdman, University College, Liverpool.

The City of London Entomological and Natural History Society has published its Report for the year 1893. The President for this year is Mr. J. E. Clark, and the Secretary Mr. C. Nicholson, 202 Evering Road, Upper Clapton, N.E. This Society has for its object the diffusion of the science of Natural History, by means of papers, discussions, exhibitions, and the formation of collections for reference. The meetings take place on the first and third Tuesdays in each month, from 7.30 to 9.30 p.m., at the London Institution, Finsbury Circus. The entrance fee is two shillings, and the annual subscription five shillings. The papers in the Report deal entirely with entomological matters, but that by Dr. Buckell on "Specific Nomenclature: Present, Past, and Future," should interest

all systematists; while Mr. J. E. Robson's discussion of the cause of Melanism in Lepidoptera, which he ascribes to anything that impedes the direct rays of the sun, should be noted by all who deal with the problem of animal colouration.

THE annual meeting of the Quekett Microscopical Club was held on February 16. Mr. E. M. Nelson, the President, gave an address, in the course of which he reviewed the work done in connection with the microscope during the past year. There was, he feared, a serious danger likely to occur to the "microscopy" of the future owing to the neglect of viewing opaque subjects with a stereoscopic binocular. He did not believe any observer, however eminent, who had not previously passed through a special training of viewing such objects in that way, could form correct ideas of the shape of objects solely by alteration of focus. The Club has now over 340 members, and on May 4 will hold an exhibition at Freemasons' Tavern.

THE lecture programme of the Royal Dublin Society, recently issued, includes "Creeping and Flying Reptiles, and their Modern Descendants," by Professor A. C. Haddon; "Our Western Fisheries," by Rev. W. S. Green; "Nature and Myth in India," by Dr. V. Ball; "Bacteria," by Dr. E. J. McWeeney; "Food, what it is and what it does," by Dr. J. M. Purser; "Reproduction and Rejuvenescence," and "On the Borderline between Animal and Vegetable," by Professor M. M. Hartog. Meanwhile, a very successful course of lectures on Geology has lately been given by Professor Grenville A. J. Cole at Belfast. A specially encouraging feature was the readiness of the students to take up practical work. Interest in glacial problems seems to be reviving in Ireland; we notice in the *Irish Naturalist* for January a paper by Professor Sollas on the drift-beds of the coast south of Dublin, while in the February number of the same magazine Miss Thompson gives an account of what is being done to elucidate glacial problems around Belfast.

THE Natural Science Club of Cambridge University signalised its five-hundredth meeting by a conversazione on March 12, at which lectures were delivered by Messrs. A. Russell Wallace, W. M. Conway, and C. V. Boys. The Brighton and Sussex Natural History and Philosophical Society also held a successful *soirée* at the Brighton Pavilion on March 2, this being the first gathering of the kind for nine years. Mr. W. M. Conway lectured on his Karakoram experiences with the aid of a magic lantern. Among a large number of exhibits calculated to attract the general public, we noticed a case of objects discovered in the Lavant Caves in Goodwood Park, and shown by Mr. J. Lewis, of New Shoreham, who favoured us with the following information:—The caves are situate on the southern slope of the Downs. One of the Roman encampments is in the vicinity, showing the district to have been inhabited at an early day; and other evidence to this effect is afforded by the circumstance that there is a levelled plateau above the caves. The caves were accidentally discovered about a year ago, and were explored by Mr. Dawson and Mr. Lewis. They consist of subterranean passages and galleries branching one from the other, and comprise about an acre of flooring. The average height of the passages is 45 feet, and these are carefully arched. The caves appear to have been hewn out by some early race with picks and wedges of deerhorn. There are indications that the object of the excavators was not, as at Cissbury, to find flints; but the caves were probably used as stores or hiding places by the people dwelling in the neighbourhood. The objects found therein were Neolithic flint implements, flint spear heads and scrapers, a portion of red deer-horn used as a pick, a lamp made of chalk, pottery, bronze articles, a Scandinavian ornament, etc., all which indicate that the occupation of these caves must have extended over a considerable period.

THE excited protests of some of our effeminate fellow-citizens will not, of course, have any effect on the foundation of the British Institute of Preventive

Medicine in Chelsea, as mentioned in our last number (p. 233). This action of what we are pleased to think is only a small section of the enlightened British public may, however, be instructively contrasted with that of certain foreign nations on whom your average Britisher would probably look with scorn. Not to mention New York and Calcutta, the Hungarian Government has established a similar institute at Buda-Pesth, while even "the unspeakable Turk" contemplates founding in the chief town of every province what our contemporary *Nature* is pleased to term an "antirabific laboratory." One such institution has for some time been doing excellent work at Constantinople.

THE North Pole is certainly being boomed just at present. Nansen started for it not so long ago, and Mr. F. G. Jackson has not been back two months from his trial trip. Now we learn that the latter gentleman is soon to set off again, while some Americans are going to have a look for Nansen. Mr. A. C. W. Harmsworth, a Fellow of the Royal Geographical Society, has generously offered to pay all the expenses of a fully-equipped expedition led by Mr. Jackson, which will proceed to Franz Josef Land with a view to exploring it in a northerly direction, and advancing as far as possible towards the Pole. The American expedition, the cost of which will be defrayed by the *Chicago Herald* and other journals, will be commanded by Mr. Walter Wellman. The whaler "Ragnvald Jarl" of Aalsund, has been chartered and will be manned by Norwegians. The immediate object of this expedition will be to discover the whereabouts of Nansen, but a return will be made to Europe for the winter if possible, although stores will be taken which will enable the explorers to winter in Spitzbergen. Both these expeditions will leave about May. We also heard a short time ago of an elaborate expedition inaugurated by Mr. R. Stein, of the United States Geological Survey, and patronised by the National Geographic Society of America. The proposal is to establish a depôt at Cape Tennyson, at the northern entrance to Jones's Sound, and then to follow the coast of Ellesmere Land westward, where an advanced depôt will be established to form the base of operations in the following year, when an endeavour will be made to connect with the discoveries of the Greely party on Greely Fjord. In this connection we may mention that Dr. Erich von Drygalski gives an account in the *Verhandlungen der Gesellschaft für Erdkunde zu Berlin* (vol. xx., nos. 8 and 9, 1893) of his journey along the West Coast of Greenland in 1892-3. Starting from Christianshaab, the party journeyed by boat and sledge round the fjords and islands to some miles north of Upernik. The object of the expedition, the cost of which seems to have been defrayed by the Society, was to settle certain points concerning the Ice-Age. In the same journal, Dr. Vanhöffen gives an account of the fauna and flora observed at spring-time in the northern part of the island.

THE British Association will hold its sixty-third meeting at Oxford, in the week beginning Wednesday, August 8. This date is not very convenient for University people, but was chosen to suit the president, Lord Salisbury, the attraction of whose presence will doubtless counterbalance the inconvenience of date. The list of presidents of the various sections has been announced as follows:—(*A*) Mathematics and Physics, Professor A. W. Rücker; (*B*) Chemistry, Professor H. B. Dixon; (*C*) Geology, Mr. L. Fletcher; (*D*) Biology, Professor Bayley Balfour; (*E*) Geography, Captain Wharton; (*F*) Economic Science and Statistics, Professor Bastable; (*G*) Mechanical Science, Professor Kennedy; (*H*) Anthropology, Sir W. H. Flower; and (*I*) Physiology, Professor Schäfer. On the Friday night Professor J. Shield Nicholson of Edinburgh will give a lecture, and on Monday Mr. W. H. White is expected to discourse on naval construction.

THE German Zoological Society will meet in the Zoological Institute, Munich, from April 9 to 11, and on Thursday a visit will be paid to the fish-breeding establishment on the Starnberger See. Foreign Zoologists are specially invited.

CORRESPONDENCE.

Professor Milne-Edwards, M. Grandidier, and Æpyornis.

I venture to think a protest ought to be raised by some British naturalist against the tone of the note in this month's Natural Science on the recent French discovery of *Æpyornis* bones. For many years there has been a generous rivalry, fruitful of enterprise and hard work, between the French and ourselves in the matter of Mascarene Zoological discovery. When, then, as in the case referred to in your article, the French have succeeded in first making known to the world a set of new and interesting fossils, it ill becomes a British Journal of the standing of Natural Science to complain in so cavalier a manner of the exact method in which the discovery is announced.

Were the complaints correct in themselves there would still be little excuse for such an attack, but as a matter of fact the only alleged fault of the authors which, if true, would have caused difficulty to the real worker on specimens, as opposed to a mere critic, practically does not exist at all in the paper referred to. Far from stating the case that "names are applied to collections of limb-bones which may or may not belong to the same species, and no particular bone is taken as the type specimen," the species are distinguished mainly on size, and in four cases out of seven one bone only, either tibia or tarso-metatarsus, has its dimensions fully and exactly recorded, and is therefore clearly in each case the type of the species. Of the other three cases, one was founded on these two bones together and the other two on the two chief leg bones, which may have been found in conjunction, and are in all cases most carefully measured. In a group where size is of the most vital and diagnostic importance, to dismiss these measurements with the remark that "though some measurements are published, we have sought in vain for any adequate diagnosis" is surely rather disingenuous.

Considering the fact that one of the authors attacked, Professor A. Milne-Edwards, holds the highest position in the French zoological world, is a naturalist of whom any nation might be proud, and one to whom many British zoologists, especially those of our National Museum, have been again and again indebted for assistance in various ways, while the other, Mons. Grandidier, simply as a private person, has spent a fortune and a lifetime in furtherance of the scientific knowledge of Madagascar, it would surely have been better had Natural Science recognised in a somewhat different way the magnificent work done by the two eminent Frenchmen it now, not for the first time, so gratuitously attacks.

Natural History Museum,
March 5, 1894.

O. T.

[Mr. Thomas seems to believe that some personal animus has influenced our remarks on the work done by MM. Milne-Edwards and Grandidier on the Æpyornithidæ. Let us at once assure him that none more than ourselves have greater respect for the personality of these eminent Frenchmen. It is the principles, not the men, that we attack; and in reply to the second paragraph of Mr. Thomas's letter we have to say:—That in three cases (*Æ. mulleri*, *ingens*, and *beisilei*) specific names are given to a collection of bones, or more than one bone, and since it is not definitely stated that they belong to the same skeleton, it must not be assumed that they do. The inconvenience resulting from this mode of procedure is apparent; for in the case of *Æ. ingens* measurements are given of the femur, tibio-tarsus, and

metatarsus. Now it is stated that the femur is different from that of *Æ. titan* described by Andrews. On referring to the paper quoted, we find that the femur of *Æ. titan* is *not* described, though a femur is provisionally referred to that species. On the other hand, the femur referred to *Æ. ingens* by Milne-Edwards may well have belonged to *Æ. titan;* and, indeed, if the bones referred to *ingens were* associated, then, as far as one can judge from measurements, *ingens* is a synonym of *titan;* for the measurements of the tibia of *ingens* given by Milne-Edwards agree almost exactly with those of the type tibia of *titan* :—

	ingens.	*titan.*
Length	81 cm.	80 cm.
Circumference at narrowest	20·5 ,,	20·7 ,,

It is true that the authors state that *Æ. titan*, though as tall as *Æ. ingens*, was less massive and its leg articulations were smaller. If this opinion is founded on the femur, we have shown that it rests on an insecure foundation. If it rests on the tibia, why are no measurements given of the articular ends in which the main difference seems to lie? Slight variations of measurement are of small value and insufficient for the establishment of specific distinctions, for among individuals in the Ratite birds size varies very greatly.

Of the new genus proposed, "*Mullerornis*," no diagnosis is published, the only detail furnished being "that they are of medium size, not having the massive and heavy appearance of *Æpyornis*, approaching rather the Cassowaries. We only know the bones of the foot."—ED.]

LIVERPOOL MARINE BIOLOGICAL STATION.

IN reply to an enquiry of ours about the Port Erin Biological Station, which we notice on p. 316, Professor Herdman writes to us :—"We are organising a Liverpool Marine Biology Committee dredging expedition for Easter week-end, like those we have had at the same time in recent years. Besides that party, several zoologists and botanists will be at work at the station during the last week of this month and during April, including Professor Weiss, Dr. Hurst, and Mr. Gamble of Owens College, Mr. Beaumont of Emmanuel College, Cambridge, Mr. Thompson of Liverpool, and myself."

A CORRESPONDENT calls our attention to the fact that Professor Lapworth is also "new blood" on the council of the Geological Society. We regret that in our note last month his name was omitted; apparently we could not realise the fact that so distinguished a geologist had not been on the council before.

STRANGE are the ways of the American place-hunter and strange, as we have noted before now, is the system under which scientific appointments are made in the United States. One of our Transatlantic correspondents complains that he has no time for scientific work. "At present," he writes, "I am very busy, being engaged in politics, as I am a candidate before the Republican Convention for the nomination of State Geologist and have the most flattering prospects; my only opponent is a local collector." As our friend might possibly obtain the appointment, we have sufficient regard for his reputation to suppress his name.

TO CORRESPONDENTS.

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NOTES AND COMMENTS.

The Extinction of Wild Animals.

WE were glad to see in the *Pall Mall Gazette*, early in April, an excellent, if somewhat tardy, notice of Dr. Hart Merriam's Bulletin on the American Hawks and Owls. Zoological periodicals, from time to time, attempt to impress upon the public the necessity of concerted effort, to prevent the wholesale extinction all over the world of those animals which interest the sportsman, either directly for their own sakes, or indirectly because they rival, or are supposed to rival, him in his attacks upon game. But, save by publishing an occasional set of letters, on the decoration of the human female by the skins of birds, the general papers take little interest in wild animals. So-called birds of prey form one field in which general papers like the morning and evening dailies might both interest their readers and assist zoologists. Here is another. The extremely interesting and peculiar fauna of Africa is becoming rarer year by year. At a recent meeting of the Zoological Society, African hunters from various parts of that continent were present, and all of them had to tell how animals, a few years ago abundant near the coast, must now be sought a month's journey inland. Will no paper take up the cry "Africa for African animals"? At the least the various European Powers might be persuaded to do something for the indigenous fauna of their vast hinterlands. The hunter is a curious survival of the instincts of primitive man, and it may be that he, too, should have place in the international preserves of Africa. But the mere sportsman must be either suppressed or regulated, else the highlands of Africa will become as devoid of large animals as the heights of Dartmoor.

Lord Lilford on the Preservation of Wild Birds.

On the same subject hear Lord Lilford: "A cry comes up from many parts of our islands," he remarks (in the *National Review* for April), "that many of our native birds are rapidly disappearing."

We regret to have to agree with such a competent ornithologist that the cry is probably well-founded. Interesting hawks and owls are apt to be classified under the simple title of "vermin"; and, in spite of a well-known remark of the late Professor Thorold Rogers, vermin are not as a rule allowed much law. Nor is this antipathy to the winged creation, which justly arouses Lord Lilford's anger, at all confined to the present century; he quotes from a statute of the sixteenth to this effect: "45 item that no person shall suffre no kyte, busserd, pye, nor fleshe crow to brede, and their yonge to fly away from the grownd uppon payne of losing xiid." Presumably the fine was large enough to secure obedience. Nor, unfortunately, does the redundancy of negatives in the above sentence imply an affirmative. There is no doubt at all about the relentless policy ordained.

Lord Lilford alludes to the sparrow in North America. A few years ago, comparatively speaking, there were no sparrows at all in the United States. Now there are so many that a bulky volume has been written to bring home to them their iniquities. And after the impeachment, the sentence is pronounced that the unfortunate birds are to be dealt with by the help of "London purple" and "Paris green." These are, it is perhaps hardly necessary to state, euphemisms for deadly compounds of arsenic. The sparrow has, however, triumphed over these kindly attentions on the part of its hosts; not so the rarer Accipitres of our islands. Lord Lilford may the honourable boast that he himself has never offered a price for the killing of any bird. We respectfully offer him our congratulations; but it is clear that he is a real *rara avis* among ornithologists. The main claim for distinction in that science, as it appears to us, is to possess the largest possible collection. It is all very well for ornithologists to complain; but they are among the chief sinners, and the blood of most rare birds lies upon their heads.

A propos of the persecution of rare birds so ill-advised as to occasionally visit the inhospitable shores of Great Britain, anyone who takes up those for the most part exceedingly arid catalogues of the birds of a county cannot fail to have been struck by the remark, of not infrequent occurrence, that such and such a rare species is known by one example "shot" at this or that locality. The excuse offered is that the visits are literally flying visits, and that there is no chance of a permanent settlement. This may be true, but it is by no means proved. The birds have had no chance of proving or disproving the allegation. The British Ornithologists' Union comprises, nearly, if not quite, all of the ornithologists of this country, and Lord Lilford is its president. If they were to follow the excellent example of the president and resolutely decline to buy any specimen of a scarce bird shot in this country, the supply would cease with the cessation of the demand. At the very lowest estimate no harm would be done by the adoption of such a course. And it is possible that the fauna of the country would be ultimately enriched,

to the benefit not only of the professed ornithologist but also of the lover of birds who is not an ornithologist.

Wild Cats at the Zoo.

Still *à propos*! A writer in the *Spectator* accepts the two reputed wild cats in the Zoological Gardens as genuine specimens of the real *Felis catus*. He may be perfectly correct, and, if so, the Zoological Society is greatly to be congratulated. For the true wild cat will be soon—and, in the opinion of some, is already—as defunct as the dodo. No doubt there are plenty of large and fierce tabby cats to be found in Sutherlandshire, and in other places in the extreme wilds of Scotland; but there is also no reason to hesitate before referring a good many of these to a mixture of the original wild cat with various pussies whom the delights of sport have permanently allured from the domestic fire-side. Anyone who lives in a neighbourhood where there is game, knows how mysteriously grimalkin disappears, to be possibly recognised later nailed to some tree trunk or barn door. We believe that a true character of the wild cat is the entire absence of white hairs about the body. Many cats, most flagrantly domestic, have the black feet and short tail tipped with sp[illegible]ck of the wild cat; as a matter of fact, there seems to be little ti[illegible]ubt but that the domestic cat is of mixed breed like the domestic -og. There is, doubtless, a considerable strain of *Felis catus* to be traced therein, but also some mixture of Egyptian or Oriental breeds. The gamekeeper, who is so praised by the writers of that rapidly-increasing class of book which has been termed "scientifico-literary," is looked upon with abhorrence by the naturalist. Under the heading of "Vermin," it is probable that the last genuine wild cat, if not already extinct, will soon pass away. The author of that delightful book, "The Naturalist in Siluria" (Sonnenschein, 1889), thought he had records of wild cats in Ross; but in the description of the supposed *Felis catus* there is too much of what De Quincey has called "demoniac inaccuracy" to admit of an unprejudiced judgment. When one has the misfortune to come across the carcass of a "wild" cat gracing a barn door, it would be worth while to secure the skull. Dead cats *do* tell tales, for Dr. Hamilton assures us that the skull of the true *Felis catus* is a perfect image in little of that of the tiger, while the domestic cat reveals its Egyptian strain by a much narrower cranium.

The White Rhinoceros.

Some thirteen years ago that mighty hunter, Mr. Selous, contributed to the *Proceedings of the Zoological Society* an excellent paper upon the African Rhinoceroses. Formerly, and chiefly through the energies of the late Dr. Gray of the British Museum, a goodly number of species were believed to exist in that continent. Mr. Selous showed that there were really only two—the

"white" and the "black." The distinctions of previous writers were mainly based upon the varying lengths and the proportions of the horns. The African Rhinoceros "Theodore," until recently alive at the Zoological Gardens, during the course of his long life in that menagerie belonged, on those assumptions, to two, if not three, different species at various times.

At a recent meeting of the Zoological Society, Mr. Coryndon gave some additional and welcome details about the now very rare "white" Rhinoceros. It is curious that this animal should have got the name, for it would appear to be blacker than the "black" Rhinoceros. The question of colour gets more involved from the circumstance that the older writers on the natural history of Africa distinctly speak of it as paler than the black *Rhinoceros bicornis*. *Rhinoceros simus* is now so rare that till quite lately there was only a single specimen in Europe. The Museum which had this unique animal was Leyden. Now, however, that Mr. Coryndon has succeeded in procuring two specimens in Mashonaland, we shall shortly see one at the British Museum; of the other the skin is to be placed in the Tring Museum, and the skeleton has been purchased by Cambridge University. The stuffed skin has been on exhibition at Mr. Rowland Ward's. In our opinion the head of Mr. Rothschild's specimen does not hang quite naturally, but the general effect is good, and the wrinkling of the skin round the nose and at the base of the ear is remarkably accurate. The bulk of the huge beast may be understood from the fact that it took thirty-seven "boys" to carry the skins and skeletons of two individuals. The approaching extinction of the animal is due chiefly to the fact that it cuts up into unusually good beef. The flavour of strange animals is almost invariably said to resemble veal, but this has apparently never been asserted of the white Rhinoceros. Opinions as to the disposition of the Rhinoceros vary. Mr. Selous regards them as not in the least dangerous, except through accident; though a wounded Rhinoceros may charge with terrible impetus, and may thereby produce direful results, he thinks that this is due to confusion and fright rather than to a wish for revenge. On the other hand, there are some who have stated that the Rhinoceroses of Africa are savage in their character.

Mr. Coryndon states that *Rhinoceros simus* stands, when fully adult, at least six feet high at the shoulder; it is, without doubt, the larger of the two certainly known species of Africa. Its main difference from *Rh. bicornis* is the square muzzle and the habit of cropping grass; the black Rhinoceros, on the other hand, has a pointed and slightly prehensile upper lip, which permits it to feed more easily upon the succulent twigs of shrubs.

THE LEPIDOSIREN AND THE MINHOCÃO.

ONE of the most interesting exhibits of the season was made at the same meeting of the Zoological Society held on April 3 last,

when Dr. Günther showed a few examples of the American *Lepidosiren paradoxa*. Considerable doubts in the minds of some people hung over the real existence of an American Dipnoid as distinct from the well-known African *Protopterus*. Dr. Howard Ayers, of the United States, declined to believe in its separate individuality. A few years ago (*Zool. Jahrbucher*, 1887) Dr. Baur emphasised the distinctness of the American from the African fish, and there is now not the faintest doubt about the matter. On all hands, however, it was admitted that the *Lepidosiren* of South America was a rare animal. According to Dr. Baur there were only four specimens of the fish in European Museums in 1887. Since then Professor Giglioli has got others, and the total number is increased by a quantity of specimens in the hands of a London dealer. But all this time in certain districts of Paraguay *Lepidosiren* was a common article of food. Few though the specimens of the *Lepidosiren* were in the Museums of this part of the world (apparently there were none at all in Rio de Janeiro), they succeeded in getting two names: it was attempted to distinguish *L. dissimilis* from *L. paradoxa*. The rarity of the fish induced various legends. It was reported by Saint Hilaire, on the authority of a Brazilian gentleman, that the *Lepidosiren* inhabited the great depths of huge lakes, into the waters of which, like the celebrated Snapping Turtle, it was able to drag horses and horned cattle. The native name was *Minhocão*, which signifies "large earthworm." And a further legend (no doubt derived from the name) shifted the scene of its operations to the soil, from which, as it passed through, it uprooted trees. Nothing more has been recently heard about this terrestrial sea-serpent; but there is no longer any room for doubt that the American *Lepidosiren* is an altogether distinct form from the African *Protopterus*.

In the fish exhibited at the Zoological Society Dr. Günther and Mr. Howes pointed out the curious peculiarity that the hind limbs of the male were ornamented with numerous branched villous processes. Figures of these were subsequently published by Professor E. Ray Lankester in *Nature* for April 12. Nothing of the kind is to be seen in its ally, the mud-fish of Africa.

A Blood-Sucking (?) Earthworm.

Speaking of wild beasts that are at once large, ferocious, and African, a correspondent sends us an interesting note about an earthworm. Africa has already produced the largest earthworm (*Microchæta rappi*) known to science, with the possible exception of *Megascolides australis* from Australia; but these are giants of a perfectly harmless kind. The worm to which we now refer is said, by Mr. Alvan Millson, Assistant Colonial Secretary at Lagos, on the West Coast of Africa, to inspire dread among the natives of that coast. Its appearance is against it: the worm is not only large—three or four feet

—but it is either of a rich, raw-beefy colour or of a lowering black, the difference of colour being a mark of a difference of species. On one occasion a number of natives were collected together when one of these giants strolled casually into the camp; the result appears to have been a rapid flight on the part of the natives. The reason for the awe-inspiring character of the worm is its reputed habit of sucking blood. It does not seem probable that the most recent results of zoological research are known in tropical Africa; but it is a curious coincidence that this research has tended to show that the line of separation between the leeches and earthworms is by no means so wide as it was at one time thought to be. This big earthworm of West Africa inhabits a locality that is remarkable; it does not, as do most earthworms, burrow constantly in the ground, throwing up castings, but lives in deserted hills of Termites.

ANOTHER FORMIDABLE INVERTEBRATE.

Latrodectus formidabilis is an apparently well-named spider which flourishes in Chili to the detriment of any human beings who have rashly interfered with it. The spider is not large as spiders go; it is only 10 *mm.* (not quite half-an-inch) long. But, like many small creatures, its malignity and venom are out of all proportion to its size. Dr. Philippi tells us (*Zool. Garten*, vol. xxxv., no. 2), on behalf of a correspondent, Herr Möhlen, that an unfortunate man, bitten by the Arachnid, lost consciousness in half-an-hour, and was weak in the joints for a month afterwards. Children have been known to be killed by it. The pest is not a rarity either, for in a certain section of country two or three webs were found in every suitable locality that was examined. Another species, closely allied, lives in Spain and Italy. The Chilian spider is "warningly" coloured, with two bright red spots above, and two below upon its black abdomen. As we have noted before, Professor F. P. Borne has an elaborate monograph on this spider; several parts have already appeared in the *Actes Soc. Sci. Chili*.

CONTINUITY IN SPECIES.

ONCE upon a time it was thought an obstacle to the theory of Descent in general, and to evolution by means of Natural Selection in particular, that the gaps existing between genera and between species were so hard to bridge over. Connecting links were absolutely demanded by the theorisers, but the working naturalist was unable to supply them. Let us not be misunderstood! We do not refer to intermediate forms between the larger groups, for these the ingenuity of morphologists soon discovered to exist in almost embarrassing profusion, nor do we refer to such interesting series as that furnished by the ancestors of the horse; but we refer to those minute gradations of form, each inappreciably different

from the one before it, but all constituting a chain that should join, without a single break, two clearly distinct species. Anyone reading the interesting and most valuable book by Mr. Bateson that we review on another page, would suppose that precisely the same difficulty occurred to-day. It is the Discontinuity, as he phrases it, between existing species that has led Mr. Bateson to seek among the facts of variation for some means of overcoming or getting round this initial difficulty; and he thinks that he has found the solution of the problem in a precisely parallel Discontinuity of Variation. In short, *Natura facit saltum*.

Now we are not concerned to deny that discontinuous variations, more commonly known as "sports," often occur; though whether these, under natural conditions, give rise to similarly modified offspring, such as might be held to constitute a new species, or even variety, is a very different question, that has hitherto been answered in the affirmative only in a very few instances. But this also may pass. What we wish now, is to warn our readers against taking this difficulty of discontinuity too seriously. One noticeable form of advance, during recent years, has been the often successful attempt to work out the problem of the Origin of Species in particular cases. This is not to be done by collecting large numbers of co-existing specimens and arranging them in a line. By such methods discontinuity is likely enough to be found even within the limits of a species, as shown in our note on "Mathematical Biology" (vol. iv., p. 83.) The safest, if not the only method, is to take the actual historical facts presented by fossils from successive horizons, elucidating them by the facts of individual growth. It is thus that Beecher has traced the evolution of the species of *Bilobites* (*Orthis biloba*), and that Hyatt and others have been able to connect the species of Ammonites, *e.g.*, *Coroniceras* (NATURAL SCIENCE, vol. ii., p. 279). Even sixteen years ago, in a very remarkable paper on "Transition Forms in Crinoids" (*Proc. Acad. Nat. Sci. Philadelphia*), Wachsmuth and Springer came to the conclusion that the changes between Burlington and Keokuk species "were made by a series of slow and gradual modifications of specific characters, which correspond in a striking manner with the changes in individual life by growth." To this conclusion, which is supported in the paper by abundant detail, they were led through the examination of a previously unworked passage-bed between the Keokuk and the Burlington. Had this never been discovered, or had it not been so fortunately rich in Crinoids, the species of the Keokuk would be still regarded as "discontinuous" both with one another and with their Burlington ancestors.

We cannot give more instances of continuity here. Mr. Bateson, of course, does not dispute them, or if he does, there are plenty more to be found in palæontological literature, especially of recent years. Neither do we dispute Mr. Bateson's instances of discontinuity. But we do wish to suggest that many cases of apparent discontinuity

between species, or examples of discontinuous curves of error within the limits of one species, may be due merely to the fact that we are totally unacquainted with the ancestors of the individuals in question.

FAUNAL SEQUENCE IN EUROPE AND SOUTH AMERICA.

IN recent numbers we have had something to say about the correlation of geological horizons in different parts of the world (vol. iii., p. 404, and vol. iv., p. 8). In this connection, a paper by Mr. W. Möricke on fossils from the Lias and Inferior Oolite of Chili (*Neues Jahrbuch für Min.*, Beilageband ix., 1894) proves of considerable interest. It is astonishing to see how many species are considered to be identical with those of Europe, and still more so to see how the European succession is paralleled on the other side of the globe. Thus, a Lower Lias fauna is indicated by *Arietites cf. rotiformis*, *Spiriferina rostrata*, *Terebratula perforata*, Piette, *Rhynchonella plicatissima* and *R. belemnitica*, Quenst.; a Middle Lias fauna, by *Hildoceras lilli*, *Ægoceras jamesoni*, *Deroceras armatum*, *D. submuticum*, etc.; an Upper Lias fauna by *Harpoceras subplanatum*, *Hildoceras levisoni*, *H. comense*, *Pholadomya fidicula*, etc. In the Inferior Oolite the exactitude of the correlation seems even more remarkable: the Upper Lias strata "ar[e] followed by red, sandy limestones, which correspond to the zon[e] of *A. sowerbyi* and *A. sauzei*. Besides *Sphæroceras sauzei* itself, [they] contain *S. cf. brocchi*, *S. polyschides*, *S. polymerum*, *Hammatoce[ras]* (*Sonninia*) *polyacanthum*, Waagen, etc. . . . These limestones with *A. sauzei* are overlaid by a reddish-brown iron-shot oolite, several metres thick, which is rich in fossils . . . and yields the following species: — *Coeloceras humphriesianum* (d'Orb. *non* Sow.), *C. blagdeni* (Sow.), *Hammatoceras alleoni* (Dum.), *H. gonionotum* (Ben.)," etc. These are considered to indicate the *Humphriesianum*-beds.

It is certainly startling to read of South American species as definitely occurring in the *Humphriesianum*- or *Sauzei*-beds, especially when we remember that the latter have only been recognised in this country within the last few years. It is, however, possible to suggest a doubt as to the correctness of all these determinations and correlations. *Hammatoceras alleoni* and *H. gonionotum*, for instance, belong to a decidedly lower horizon in Europe; while species, as yet unnamed, which might be mistaken for *C. humphriesianum* or *C. blagdeni*, occur in this country in close association with those *Hammatocerata*. Moreover, it may not prove safe to trust these identifications too closely; at all events, fig. 4 of plate ii., which is named *Leioceras subplanatum* (Oppel), shows a specimen differing from that species in numerous points, and rather resembling *Grammoceras fallaciosum*, Bayle.

Apart from these minor doubts, we are fully prepared to learn that the faunal sequence in South America corresponds remarkably with that in Europe. Whether, as a consequence of this, it will be

necessary to allow for the deposition of the strata of any given zone a vastly greater length of time than has hitherto been conceded, or whether Dr. Walther's explanation, quoted in our last number (p. 245), will suffice—these are questions that will soon demand an answer.

The Legs of Trilobites.

In Natural Science for September, 1893, we called attention to the interesting discoveries of Messrs. Valiant, Matthew, and Beecher, which enabled them to give a somewhat definite account of the appendages and antennæ of these extinct crustacea. Dr. Beecher has followed up his account by two more papers, the first of which (*American Geologist*, January, 1894) deals with "the mode of occurrence, and the structure and development of *Triarthrus becki*, and is founded on specimens obtained from the Utica shale (?) of Rome, New York. In this paper the definite structure of the legs of the thorax has been worked out in detail, and shows each limb to consist of "two nearly equal members, one of which was evidently used for crawling, and the other for swimming." "Each limb is composed of a stem, or shaft, with an outer branch (exopodite), and an inner branch (endopodite)." "The precise form of the coxal joint of the stem (coxopodite) has not yet been clearly made out. It is followed by a broad joint about twice as long as wide, which may be referred to the protopodite." Dr. Beecher says: "The final conclusions to be reached from a complete study of the development and structure of these animals can only yet be surmised. It is quite evident, however, that they are related to the true Crustacea."

In the second paper (*Amer. Journ. Sci.*, April, 1894) the structure of the appendages of the pygidium of *Triarthrus becki* is shown to be as follows: "The endopodites preserve their slender, jointed distal portion, but the proximal part is composed of segments which are considerably expanded transversely, thus making a paddle-like organ, the anterior edge of which is straight, while the posterior one is serrated by the projecting points of the expanded segments. These points bear small bundles of setæ."

An Eocene Pteropod.

The identification of a Pteropod in British Eocene strata is of considerable interest, when we remember that no less than six species are known from similar beds in the Paris basin. Mr. G. F. Harris gives a description and a figure of the new form, which he names *Euchilotheca elegans*, in the *Proceedings of the Malacological Society* (vol. i., no. 2). The specimen is incomplete, was found at Bracklesham, and belongs to the well-known Edwards Collection in the Natural History Museum. In the same journal, Mr. R. B. Newton redeems in part a promise

made in his Systematic List of the Edwards Collection, by describing, in conjunction with Mr. Harris, several of the molluscan species recorded under *nomina nuda* in that Systematic List. We hope that these authors will continue the good work now begun, and rid scientific literature of a mass of names which are of no value and should never have got into print. An excellent point in these descriptions, is the placing of n. sp. (new species) after the names where given, and so making it clear that this is the first occurrence of the new specific name in literature.

AIDS TO GEOLOGISTS.

IT has often been our duty to criticise geological and palæontological text-books with a certain severity, and we have lamented the want of a good elementary text-book of palæontology in the English language. We, therefore, take the more pleasure in directing the attention of students to the second edition of Professor Cole's very useful "Aids to Practical Geology" (8vo. London: Griffin & Co. Price 8s. 6d.) It is true that this does not pose as a text-book, is indeed intended to supplement the text-book, but for all that it will prove more reliable and intelligible than many more pretentious manuals. The style is clear, the figures truly illustrative of the text, the examples of genera on the whole well chosen, the information commendably up to date, and the views of the more advanced as well as of conservative workers prudently set forth. As the first edition was, we believe, stereotyped, there are not many alterations in the body of the text, but this edition has been brought up to date by the insertion of notes in smaller type. As a rule the English student has to rely on foreign text-books in these branches of science; but this is a case in which we honestly think even the Germans might do worse than translate from us. We also take this opportunity of recommending to students, and indeed to all geologists, the thoroughly practical work of Mr. W. H. Penning, "A Text-book of Field Geology, with a section on Palæontology by A. J. Jukes-Browne" (Stanford), which has for some time been out of print. A second edition, however, has just been re-issued, with the addition of a chapter on Pioneer Surveying, chiefly illustrated by South African instances. It is a pity that the whole work could not have been brought a little more up to date, and issued as a third edition; but even in its present form, it is the best of the kind available.

FOSSIL PALM-FRUITS.

IN a recently-published monograph of the genus *Nipadites* (*Journal Linnean Society*, vol. xxx., p.143), Mr. Rendle figures a fine specimen of these fossil palm-fruits from the south coast. It measures five inches by four and a half, and is by far the finest example yet found in this country. For its discovery we have to thank Mr. Clement

Reid, who came upon a number of the fruits on a newly-exposed portion of the beach at West Wittering, near Selsea, in West Sussex, and who has since seen it at Hengistbury Head in Hampshire. More than fifty years ago, James Scott Bowerbank, the founder of the genus, described a number of species from the London clay at Sheppey; these are of various sizes, from that of an almond fruit upwards, but are all smaller than the specimens now in question, which closely resemble those found, often enclosed in concretionary nodules, at Schaerbeek and Woluwe, near Brussels. The only living representative is *Nipa fruticans*, a dwarf palm confined to the brackish estuaries and sea-board marshes of India and Malaya as far as Borneo, New Guinea, and the Philippines. The fruits are borne in large heads, and when ripe drop into the water, where they may float for some time. It is of interest to note that many of the fossil fruits are much bored by *Teredo*, showing that they also floated before becoming water-logged.

Anatomy and Plant Classification.

We would refer our readers to last month's number of the *Journal of Botany* for an able review, by Mr. C. B. Clarke, of the recently-issued volume of De Candolle's "Monographiæ Phanerogamarum," vol. viii., comprising the Guttiferæ, and the work of Julian Vesque. A novel feature is the wholesale use of anatomical details, special importance being assigned to the histological characters of the leaf. As the reviewer observes, the anatomical structure of leaves is rapidly altered by the environment, moisture, shade, and warmth. Hackel, a most painstaking and competent worker, could make no use of it in the grasses. Anatomical, like all other characters, have a variable value, and "are only one among a large number of other characters at least equally entitled to weight. Therefore, anatomical characters are not to be given a superior value, and to be allowed to swamp all other considerations, any more than the number of the stamens. There is no reason to suppose that the anatomy of the leaf is a better clue to the affinity of the plant than the position of the ovule." Mr. Clarke refers to Dr. Palla's revision of the genus *Scirpus*, where the attempt at a classification on a single character, the anatomy of the stem, has brought together plants from the whole range of the large tribes Cypereæ and Scirpeæ, which, though otherwise very different, display similar stem-characters. Happily, Mr. Vesque has not done this. His characters from leaf-anatomy are strictly subordinated to the general weight of all other characters, and "the utmost that can be said against the anatomical method, as introduced by Mr. Vesque, is that it makes his book 15 per cent. heavier than it otherwise would be; it does not make it more troublesome to find other matters."

An instance of the limits to the use of anatomy in the classifica-

tion of plants is supplied by a paper by Miss Smith in the last issue of the *Journal of the Linnean Society* on the anatomy of a plant from Senegambia. The plant is one of striking and unusual appearance, but has neither flower nor fruit; and suggestions as to its affinity ranged from Lycopods and Conifers to Melastomaceæ, Acanthaceæ, and Thymelæaceæ. In plain English, nobody could make it out. Examination of the internal structure of the stem revealed the fact that it belonged to the Dicotyledonous division of flowering plants, while, according to the author, the occurrence of islands of soft bast tissue in the wood, and groups of phloëm at the periphery of the pith "considerably narrows down the systematic limits within which the plant must come." This notwithstanding, Miss Smith is doubtful whether its affinity is with the Loganiaceæ, Acanthaceæ, Peneaceæ, Gentianaceæ, or Melastomaceæ—a somewhat wide range —or even with any of these families.

THE PRESERVATION OF HERBARIA.

BOTANISTS who dry plants may be glad to know of a time-saving device suggested by C. Michener in *Erythea* (March). Elsewhere, as in the climate of San Francisco, the problem is, how to dry your driers? for given a good supply of dry warm driers the rest is easy in comparison. The method described is as follows: In one corner of each drier, about an inch from its edges, a brass eyelet is inserted; through the eyelet is run a piece of light strong fish-line twenty-five to thirty feet long. On each line are strung from seventy-five to one hundred driers, which are kept from slipping off by a small iron ring at the ends of the cord. The eyelets are of such a size that the cord slips through them easily, and is no hindrance to using the driers, which remain strung during the whole operation, while laying the plant piles, changing the driers, and drying them. The outdoor part of the apparatus consists of several upright posts about thirty feet apart, into which hooks are screwed at every two feet. To dry a set of driers, the rings at the end of the cord are slipped over the hooks, and the driers distributed along the cord. Two posts can carry three or four lines, stretched one above the other. To bring the driers in, and get them into a pile ready for the next change, it is only necessary to push them along the line till they are in contact, and then unfasten the rings from the hooks. The author claims these advantages for his method: it does away with half the labour of spreading the driers to dry, and nine-tenths of the labour of picking them up when dried; both sides of the drier are exposed, and hence they do not come in contact with any surface, damp from dew, frost, or moisture; and the length of time required to dry them is also reduced. In the case of a sudden shower, the driers can be very quickly brought in, and there is no danger of their being blown away.

Recent Researches in Botany.

One of the early numbers of Natural Science contained an account of Treub's researches into the embryology of the genus *Casuarina*. So many new and important facts were brought to light in the course of his work that Treub was impelled to separate the family Casuarineæ, which this single genus constitutes, from the apetalous group of Dicotyledons, where it had hitherto been placed near the Cupuliferæ, and make it the sole member of a new class of flowering plants. This new class he named *Chalazogams*, the rest of the Phanerogams forming one great class of *Porogams*, the main distinctive character being the mode of approach of the pollen-tube to the embryo-sac in the process of fertilisation. In the second class the tube followed the normal course, namely, down the micropyle of the ovule, and so directly on to the top of the embryo-sac; but in the Casuarineæ an indirect route was taken, the tube passing along the side of the ovule down the chalaza, and entering one of the many macrospores or embryo-sacs at the lower end. Other points of interest in the embryology of this anomalous genus were the existence, not of a single macrospore, but of a number, which formed a sporogenous tissue, and of which several might develop very considerably; the presence of long tails to the developing macrospores, up which the pollen-tube ultimately made its way; the frequent appearance in the sporogenous tissue of pitted cells (tracheids), for which Treub could suggest no function; the branching of the pollen-tube, with the formation of recurved blind endings; the absence of antipodal cells in the embryo-sac; and finally the early assumption of a cell-wall by the egg-cell before the arrival of the end of the pollen-tube in its neighbourhood.

In the *Transactions of the Linnean Society* (2nd series, Botany, vol. iii., p. 409), Miss Benson now describes the results of some work on the Embryology of the Amentiferæ which, following on the above, are of great interest and importance. In the first place, she shows that it is no longer possible to classify merely the Casuarineæ by themselves as Chalazogams, for in the alder, birch, hazel, and hornbeam the pollen-tube takes the same indirect route, while there are other points of agreement between the strange Australian genus and our British catkin-bearing trees. Thus, whereas in the normal type hitherto found to be almost universal among Angiosperms, the macrospore originates from one of a single "axile row" of cells (archesporium), in the genera studied by Miss Benson a large number of such rows was present, forming a sporogenous tissue, in which, moreover, spindle-shaped cells frequently arose, developing, in the case of *Castanea*, into large and conspicuous tracheids. The prevalence of "cæca," formed by the embryo-sac, recalling the "tails" of the macrospores in *Casuarina*, is remarkable, as is also the branching of the pollen-tube and the formation of recurved endings.

These points of agreement are, however, "so mixed up with striking distinctions," that, so far, the author cannot claim to have found a counterpart to the extraordinary facts recorded by Treub. "We know nothing of an embryo-sac without antipodals, for they are present and very conspicuous in the British Amentiferæ. In *Betula*, *Alnus*, and the Cupuliferæ I have observed the fusing of the polar nuclei, and in every genus the presence of synergidæ and a *naked* egg-cell."

Miss Benson, in continuing her work, hopes to find some clue to the solution of the vexed problem of the homology of the antipodal cells and the fusion of the polar nuclei of the embryo-sac.

THE four papers contained in the first instalment of volume viii. of the *Annals of Botany* are somewhat diverse in origin. The first, by Professor Campbell, hails from California, and includes the result of some work on the development of *Marattia Douglasii*, the author having been fortunate in finding a large number of very young plants, and also the prothallia and embryos, while collecting in the Hawaiian islands two years ago. Professor Campbell claims to have shed more light on the position of the Marattiaceæ and the origin of Eusporangiatæ from the Bryophytes. Points of resemblance between the sexual generation of *Marattia* and the Liverwort *Anthoceros* are adduced in support of this descent.

In the next, Mr. H. H. Dixon describes his researches on the fertilisation of the Scotch Fir, carried out in the Botanical Institute of the Bonn University under Professor Strasburger's supervision. It is found that in *Pinus silvestris*, as already shown for the Yew by Belajeff, the nucleus of the pollen-tube is not the one efficient in fertilisation, as was formerly believed to be the case in Gymnosperms. As Strasburger had previously demonstrated, in Angiosperms this nucleus is asexual, the male sexual nucleus arising from one of the cells originally behind the nucleus of the pollen-tube. In the Scotch Fir a pair of cells borne on a stalk-cell are formed as the result of a few cell-divisions on the side of the pollen-grain opposite the point from which the tube emerges, and it is the nucleus of one cell of the pair which is sexually functional. The author traces their separation from the stalk-cell and passage down the tube, in which they are pursued and ultimately passed by the nucleus of the stalk-cell, which then becomes indistinguishable from the original nucleus of the pollen-tube. All four nuclei pass into the female cell, their path through which can often be traced by the grains of starch which enter at the same time. Only one of the pair of nuclei unites with the female nucleus, the other remaining in the protoplasm with the two asexual nuclei from the pollen-tube. In the Angiosperms, where a similar division into two similar sexual cells occurs, Strasburger was able to observe in one case the fertilisation of an oosphere by the two nuclei, indicating that both cells are truly sexual. It is suggested that the formation of two sexual cells in *Pinus* came about when the branching of the pollen-tube, which

the author observed, was the rule, there being some probability of two branches reaching different oospheres. The author was unable to demonstrate a perforation of the pollen-tube, as Karsten has recently done, in the genus *Gnetum*, though the apex was often seen to be provided with a deep pit.

Mr. Farmer contributes the only indigenous paper—some studies on Liverworts, in which the histology of the nucleus is discussed in some detail. The last communication, by Mr. G. T. Peirce, is dated from Leipzig, and is a somewhat exhaustive account of the mode of twining of the dodder, including its response to geotropism and other external stimuli, and the mode of growth and penetration of the suckers into the tissue of the host-plant; in the last process chemical activity is found to take part.

Antarctic Explorations.

On p. 234 of our March number, we called attention to the efforts of the Scottish Geographical Society to awaken interest in Antarctic problems. We have now to record a paper by Dr. John Murray in the *Scottish Geographical Magazine* for April, entitled "Notes on an Important Geographical Discovery in the Antarctic Regions." The Norwegian schooner "Jason," Captain Larsen, while looking for seals, in November last year, found a comparatively open sea to the south of Louis Philippe Land and Joinville Island, and proceeded down along the margin of Antarctica to 68° 10′ S. latitude. This is over three degrees further than Ross's 1843 limit, and deserves the importance assigned to it by Dr. Murray. Some new islands were discovered by Captain Larsen at a point near 65° 7′ S. and 58° 22′ W., two of which were active volcanoes; and to these were given the names of "Sarsoe" and "Jason." The latitude reached by this sealing schooner was not so far south as that reached by Weddel in 1823, for he penetrated past the 74th meridian; but its interest lies in the fact that, in this last exploration, the margin of the land was followed the whole way, and thus nearly 300 miles of the southerly extension of Antarctica have become known to us.

In giving publicity to this interesting record, Dr. Murray points out what additions can be made to our geographical knowledge by a small sealing schooner, and urges the importance of a British expedition, properly equipped, and provided with steam-power, to investigate the geographical and natural history problems awaiting solution. We are glad to read that Captain Larsen left the Falkland Islands on the 17th January for another trip to the south, and we trust that this hardy seaman will have further discoveries to announce on his return home.

Johns Hopkins' University.

As we dealt largely in the April number with various English institutions, it is interesting to compare with them an institution in

America that combines many of the features scattered among various bodies in London. This institution is the University founded in Baltimore by Johns Hopkins, who died in 1837. The University Circular for February of this year contains an account of the foundation for the twenty years now completed, written by the President, Daniel C. Gilman. The aims of this University were formulated in an inaugural address by the President, as follows:—"An enduring foundation; a slow development; first local, then regional, then national influence; the most liberal promotion of all useful knowledge; the special provisions of such departments as are elsewhere neglected in the country; a generous affiliation with all other institutions, avoiding interferences and engaging in no rivalry; the encouragement of research; the promotion of young men, and the advancement of individual scholars, who by their excellences will advance the science they pursue, and the society where they dwell."

After paying a tribute to members of the academic staff, Dr. Gilman writes:—"The first requisite of success in any institution is a body of professors, each of whom gives freely the best of which he is capable. The best varies with the individual; one may be an admirable lecturer or teacher; another a profound thinker; a third a keen investigator; another a skilful experimenter; the next a man of great acquisitions; one may excel by his industry, another by his enthusiasm, another by his learning, another by his genius; but every member of a faculty should be distinguished by some uncommon attainments and by some special aptitudes, while the faculty as a whole should be united and co-operative. Each professor, according to his subject and his talents, should have his own best mode of working adjusted to and controlled by the exigencies of the institution with which he is associated."

About some Little-known Molluscs.

For many years past conchologists have been accustomed to reckon the three marine genera *Siphonaria*, *Gadinia*, and *Amphibola* as Pulmonates, despite certain striking peculiarities in their anatomy, on account of the common possession of a pulmonary cavity, supplemented, it is true, in the case of *Siphonaria*, by a gill.

In 1892, however, Haller showed that the last-named was, in point of fact, an Opisthobranch, and its true position next to *Umbrella*. Just lately Plati has come to the conclusion, from a careful investigation of *Gadinia*, that it, too, is an Opisthobranch, so that only *Amphibola* is left with the Pulmonates by Bouvier, who studied it in 1892.

I.

Continental Growth and Geological Periods.

(*Continued from p.* 298.)

Persistency of Drainage Lines.

THE ideas and considerations dealt with in the preceding part of this article bring us naturally to what is taking place in our own time. Since the Tertiary upheavals new lines of drainage and channels of erosion have been established, by which the waste of the land has been and is conveyed to the lowest levels, which are generally, but not always, in the ocean. That is to say, in some cases the drainage lines terminate in inland seas or hydrographic basins, which the evaporation exceeds the rainfall. Such are the Aralo-Caspian basin and the Central Asian basin[11] in which are situated the lakes Balkash and Alakul, the Great Salt Lake and other areas in North America. Mediterranean seas, connected by an outflow or inflow, or both, like the Black Sea and the Mediterranean, bays such as the Gulf of Mexico, the Gulf of California, Hudson's Bay, etc., also receive their quota of sediment.

Though the general levels of the Continental lands have fluctuated, the main drainage lines—established and deeply cut into the strata they traverse—seem to be very persistent, so that the waste of a large part of the North American Continent has travelled down the Mississippi to the sea since Tertiary times. Similarly, that of the South American Continent has followed the lines of the Amazons, La Plata, and Orinoco, while that of the western area of both continents, cut off by the divide of the Andes and Rockies, extending from south lat. 50° to north lat. 70°, has perforce been cast into the Pacific Ocean or the embayments connected with it.

In the Continent of Africa the Congo delivers its load into the broad Atlantic, almost opposite to the Amazons, with a flood almost equal to, if not superior to it. The same persistence is seen in the rivers of Europe and Asiatic Russia and the Northern portion of North America delivering into the Arctic Ocean, and in the great rivers of China flowing eastward into the shallower eastern seas. These lines of drainage, as I have sought to show in the "Origin of Mountain

[11] *See* the "Physical Conditions of the Aralo-Caspian Region," Wm. Hewitt, Pres. Address L'pool Geol. Soc., *Proceedings*, 1892-3.

Ranges," have become fixed in their positions through the upheavals of mountain ranges; and the modifications they have undergone in their courses have been mainly confined to the hydrographic basins in which they exist, the most fluctuating portion of the rivers being on their own deltas. In cases where the levels of the watershed may not have been very marked, alterations of the drainage basins themselves have arisen through differential subsidence or elevation. Changes such as have been noted in the Oxus and Jaxartes in Central Asia have hardly involved a rearrangement of drainage basins considered on the large scale. The Ganges and Brahmapootra have rolled their floods into the Bay of Bengal since the final upheaval of the Himalayas. But it is unnecessary to multiply these instances.[12]

A FUTURE PERIOD.

It is now time to ask ourselves what is the meaning of this constancy and the persistence of these important physical features. Though differential subsidence and elevation on the large scale are marked on the one hand by raised beaches on the land, and on the other by deep-cut river beds below the level of the sea, as in the Congo and the great rivers on the North American Continent, what may be called the rugosities of surface represented by mountains never disappear excepting by denudation. Further rugosities are developed by faulting and mountain elevation in the locus of mountain ranges, by a persistence of the movements which have initiated them; but the great levellers, the waters and the atmosphere, with their combined chemical and dynamic action, bring their materials again under the displacing action of gravity, whereby, like water itself, they finally find their lowest level. Hence sediments accumulate and have been accumulating through Quaternary time, and were these actions to continue long enough without compensatory elevation, the whole of the land, as has been pointed out again and again, would disappear. On the other hand, were compensatory elevation to go on long enough, the whole of the sedimentary deposits would be stripped from the land, and we should be permitted to see what no one is certain he has ever yet seen, that is, the original crust of the earth.

It thus appears that, though these denudations are of long continuance, they must in the course of geologic history come to an end, for we find most of the land-areas covered with sedimentary deposits.

It follows, then, if the geologic history of our planet is to continue in the form in which it has done in the past, and not to terminate in stripped land-areas or universal levelling, that the sediments on the coasts and seas bounding our continents, which have been accumulating since Tertiary upheavals, must be themselves eventually raised above the waters, and joined on to the dry land. In what way has

[12] Ramsay's "River Courses of England and Wales," *Q.J.G.S.*, 1872, and "On the Physical History of the Rhine," *Royal Instn.*, 1874, may be studied with advantage in this connection.

this occurred in the past? If we ascertain correctly, it will be the key to the future—a truly geological prophecy. That there is a relation of cause and effect between subsidence, sedimentation, and subsequent upheaval, it has been my object to point out in the "Origin of Mountain Ranges." In development of this idea, I seek to show how, as a consequence of this action and interaction, the earth's strata come to be divided into rock-groups containing distinguishing fossils and having each its characteristic lithological grouping, which we know as Periods.

If a great group of physical features lasts throughout vast eons of time, such as I have shown has happened even during the Quaternary period (which we are living in now, and which is likely to continue much longer), it is evident that the deposits will have a characteristic lithology—looked at in the large way—and a fossiliferous *facies* will distinguish them from the Tertiary and preceding rock-groups. Representatives of the modern mollusca and the vertebrate and mammalian fauna and of the modern flora will be here and there embalmed to give further distinction to the strata and joy to future geologists. Among these fossils, works of art and fragments of ships, together with bricks and clinkers from ocean steamers, such as even now are occasionally dredged up in the Atlantic, will be a feature. I understand that when soundings come up with cinders attached to them the heart of the mariner rejoices, knowing that he is on beaten tracks, if so inappropriate a phrase is allowable in ocean navigation. Truly, if the world continueth through another geologic period, and there seems no reason to assume that it will not, as the internal forces are still alive making for rejuvenescence, the Quaternary will be the most distinct and remarkable period of all. Not only will the remains of man and his works up to the present be embalmed, but also those works still in the potentialities of the future to which we may, considering the progress of the last fifty years, look forward with mysterious expectation, if not awe. The future great period will include the present and terminate with the completion of the Quaternary.

Sediments of Existing Seas.

If the preceding reasoning has any cogency in it, there must exist on our coasts, at the mouths of the great continental rivers, enormous sedimentary deposits laid down since the close of the Tertiary. The denudation of the land since then, though late in geologic time, has been enormous. Whole areas have been stripped of their Tertiary covering and in mountain districts thousands of feet of strata removed. To go no further than our own isles, the Tertiary rocks that remain are but a fragment of what once existed.

The Miocene rocks of Antrim, Staffa, Eigg, Rum, and Skye consist chiefly of lava flows and ashes of great terrestrial volcanoes, which fill up the undulating valleys of the Chalk in Antrim, and those of Oolite and Silurian gneiss in what is now the West of Scotland.

According to Ramsay, these islands formed part of a "vast continent, to which the British Islands were united, and which, embracing Iceland, spread far to the north and west into the area of what is now the Atlantic, and on the south was united to Africa, when as yet the Mediterranean had no existence."[13]

Long before the extreme denudations represented by these fragments of a once continuous sheet took place, old rivers intersected this ancient land and scooped out valleys in the Miocene lavas and hills, which were again partly filled by torrents of basalt and obsidian. "Thus it happens that in the old volcanic plateaux, valleys a thousand feet deep have been excavated, and the whole region has by denudation been changed into a line of fragmentary islands the high sea-cliffs of which attest the greatness of the waste they have in time undergone."[14] Sir A. Geikie[15] says, speaking of Eigg, "Lastly, from the geology of this interesting island we learn, what can be nowhere in Britain more eloquently impressed upon us, that, geologically recent as that portion of the Tertiary period may be during which the volcanic rocks of Eigg were produced, it is yet separated from our own day by an interval sufficient for the removal of mountains, the obliteration of valleys, and the excavation of new valleys and glens where the hills then stood." Though we may not claim all this denu[dation] tion for Quaternary time, since much of it may have taken place du[ring] the Pliocene with resultant Pliocene rocks now sunk beneath the [sea], these quotations from eminent geologists may well serve to give us a[n] inkling of the powers we are dealing with. According to Dana, th[e] length of the Quaternary period up to now is one-third of the Tertiary. This is but an approximate guess, but nevertheless a valuable one.

Turning our attention to North America, we may say that the Mississippi is at least as old as the Quaternary, and probably very much older. The elevation of the Rocky Mountain regions compelled the drainage of the continent to take a south-western course, while the older land of the Laurentian Highlands and the Appalachians blocked it from direct connection with the Atlantic except in the northern portions. It has been shown that since the elevation of the Uinta Mountains, which began in Cretaceous times, 3½ cubic miles of rock have been removed from every square mile of their surface.[17] A large area of the central Mississippi Valley is occupied with Cretaceous rocks, while the southern part, bordering the Gulf of

[13] "Physical Geology and Geography of Great Britain," 5th ed., p. 263.

[14] "Physical Geology and Geography of Great Britain," 5th ed., p. 356.

[15] "On Tertiary Volcanic Rocks," *Q.J.G.S.*, vol. xxvii., p. 310.

[16] "Manual of Geology," 2nd ed., p. 586.

[17] "Origin of Mountain Ranges," p. 243. In a most interesting paper by Dr. Andrew Lawson, entitled, "The Post-Pliocene Diastrophism of the Coast of Southern California" (*Bulletin* of Dept. of Geol. University of California), it is shown that Pliocene sediments of over a mile thick, called the Merced series, were laid down on the Californian coast. This is an extraordinary example of the accumulation that has taken place in only the closing phases of the Tertiary period.

Mexico, is Tertiary (Eocene and Miocene), and a smaller portion consists of Quaternary deposits. This would seem to show that for a considerable period of time after the upheaval of the Rocky Mountain region continental conditions prevailed over this area, and that the sediments from its erosion and waste now lie at the bottom of the Atlantic.

Unfortunately we cannot read the earth's history with any fulness or accuracy. We may only see as through a glass darkly, but still we are justified in concluding from a consideration of these examples, which could be multiplied from well-nigh all the known world, that an enormous erosion of the land has taken place during the Quaternary period, and that these sediments, probably with those of an earlier period, now lie on the ocean floor, are still accumulating, and will accumulate until such time as the earth's living forces bring them up from below the waters to take their place as mountain, plain and valley in the unceasing cycle of the earth's changes.

Conditions of the Earth's Crust in which Sediments Accumulate, and Rate of Accumulation.

The conditions of the crust of the earth upon which these sediments are being laid down are as varied as the sediments themselves. While in the north-eastern portions of North America, from New York to Baffins Bay, there are no evidences of volcanic activity either at the present time or late geological past, south of New York we gradually approach a volcanic and earthquake area, crossing the Gulf of Mexico, and culminating in Central America. The West India Islands give frequent evidence of volcanic instability by the raised coral formations which are there met with, together with foraminiferal deposits considered to be of deep-sea origin.[18] Within this basin-shaped and almost closed Gulf of Mexico, deep-sea oozes are being laid down, and into this area, at one locality or another, the Mississippi has delivered its daily burden of sediment through, at least, Quaternary time. These spoils of the continental land will probably be interbedded with the lime deposits worn from coral reefs and with the more purely oceanic accumulations of foraminifera and other deep-sea forms of life.[19]

Fluctuations of level of the Mississippi mouth and valley may have given the terrigenous deposits a wider distribution than what obtains now. Borings in the Mississippi Valley show a subsidence of at least 630 feet at New Orleans.

If the land were upraised so as to represent the physiography of the time of this elevation, the sediments now being brought down this great drainage channel would be delivered further into the

[18] "On the Elevated Coral Reefs of Cuba," W. O. Crosby, *Proc. Boston Soc. of Nat. Hist.*, vol. xxii.; "The Geology of Barbados," Jukes-Brown and Harrison, *Q.J.G.S.*, 1892, pp. 170–226.

[19] *See* three letters by Alex. Agassiz—"On the Dredging of the U.S. Steamer 'Blake,'" *Bull. Museum of Comparative Zoology, Harvard*, vol. v., 1878.

Mexican Gulf. Likely enough in portions of the Gulf in time past there may have been outflows of lava on the sea-bed. Volcanic intrusions in the form of sheets insinuated between the sedimentary beds are another form in which it is highly probable these inland igneous forces have developed themselves. If there be any justice in these inductions there must be immense accumulations existing in the Gulf of Mexico, for its area is not above one-third of that from which the sediments have beeen derived.[20] In addition we have the great denudations from the mountainous regions of tropical America and from Mexico.

The land sediments and the lime and silica eliminated from the waters of the sea by organic agencies, and ever being renewed by the decomposition of the rocks through the solvent action of rain-water aided by humic acids, must, together with igneous flows, intrusions and ashes, be building up rock-groups which will eventually form the framework of new land. None the less is this the case when the earth's crust has been long in apparent repose; for there surely will come a critical time when they will begin to react on the earth's interior, so that before being elevated into new land these portions of the earth may go through the volcanic cycle of change. We might in this way travel over the earth's surface and find in every part modified phases of the same actions in progress—for our planet has not yet lost its vitality. All along the west coast of the two Americas volcanic activity is adding to the thickness of the sea-bottom, and the ceaseless denudation of the great mountain ranges provides a covering on a still grander scale. On the Atlantic coast of South America the conditions of the coast are more general, but here a still greater load of sediment is being deposited by the Amazons, La Plata, Orinoco, and other great rivers. On the African coast it is the same, and two of the greatest rivers of the world—the Amazons and Congo—pour their tropical floods and the spoils of the land on opposite sides of the Atlantic in the same parallels of latitude.

I have shown that the sediment from 21,000,000 square miles of land, the estimated area of the land draining into the Atlantic, would, at the rate of one foot per 3,000 years, fill up the North and South Atlantic, estimated at 21,000,000 square miles, two miles deep, in 32,000,000 years.[21]

With all this variety I have little doubt that the deposits as a whole will be differentiated from any that have gone before so as to justly enable the time in which they were laid down to be called a Period.

[20] In a valuable paper, entitled, "The Gulf of Mexico as a Measure of Isostacy" (*Ann. Journ. of Science*, vol. xliv., 1892, p. 188), McGee estimates the area of degradation at 1,800,000 square miles, and that of deposition at 100,000 square miles or one eighteenth. This may be true in relation to what is now taking place, but my assumption is that the area of deposition has shifted from one *locus* to another, being conditioned by elevation and subsidence.

[21] "Denudation of the Two Americas," Presidental Address, L'pool. Geol. Soc., 1885.

This will be due mainly to the persistence of the physiographic features of the land-areas, which will only change when the internal forces of the earth, reacting on the sedimentary mantle, develop expansion therein, and by lateral and vertical pressures and movements oftentimes renewed, develop those ridges of the earth called mountain chains, and so diversify the planet's surface by sketching out new land-surfaces where now is sea, thus modifying the form and conditions of the old continents.

General Conclusions.

The result of our reflections upon the group of facts which it has been the object of this paper to bring together in systematic order tends to show that the growth of land-areas of the globe is governed by certain laws of development. The records of the rocks tell us pretty plainly that there must, throughout geologic time, have existed land-areas on the globe comparable in extent with those now existing. It is not my intention to touch upon that vexed question, the permanence of ocean basins, or to sketch out the lines of former land-extensions. We have seen that land-areas grow by accretion from existing land. The ruins of former continents have added to their extent, so that by process of accretion their outlines and physiographic features have altered; therefore, the present continents, though the outgrowth of earlier ones, may be vastly different in form, position and orography from their predecessors. That the land-areas should have been preserved through geologic time, considering that their mean heights are so little above the water, has always presented itself to my mind as a geological crux. We now see that the waste of the land and the collection of the resultant sediments in the bordering seas is Nature's means of renewal, and we further gather that continuity of land-areas throughout geologic time, so necessary for the preservation of terrestrial life, is in this way secured. The origin of mountain ranges and the growth and decay of continents are thus closely related. New lands are the consequents of sedimentary loading and recurrent expansion, acting through a chain of events which I have dealt with in the "Origin of Mountain Ranges," and which seem to me to be the explanation which brings together all the hitherto isolated facts of geology into one comprehensible whole. Our planet's history is not one of fortuitous accident, but of development by law, and, as I believe, in the manner herein of necessity slightly and concisely sketched out.

It is through the labours of many geologists gathering facts out in the field in both hemispheres that we are enabled to approach the subject with any probability of a reasonable solution. As these facts accumulate we shall be in a better position to test, modify, and develop our theory. In the meantime, I have reasonable hopes that this outline may form a not unsuccessful guide in directing future investigators towards a true explanation of the earth's geologic vitality.

T. Mellard Reade.

II.

Wind and Flight.

IT is generally known that all birds turn their heads to the wind when they wish to rise. But the reason of this is not so simple as it is thought to be. If the wind were of perfectly uniform velocity, the bird could gain no assistance from it, and it would make no difference whether he looked down the wind or against it. The moment he left the ground, becoming part of the moving current, he would have, independently of what was gained by his own efforts, no relative velocity. What we feel as a wind might be looked upon as due to the earth revolving within its envelope of air, and this could not possibly affect the bird when once he had left the ground. However strong a breeze may be blowing, people in a balloon will not be conscious of it, unless they look down towards the earth and see the trees and the houses passing rapidly beneath them. If, however, as he rises, the bird is continually entering more rapid currents, then the inertia due to his having just left a stratum where the velocity is less, will cause the wind to act upon him as upon a kite, and assist in lifting him; whereas, if he has no inertia or momentum, he will not rise any more than a kite that has broken its string. It is well to insist upon this point, since even Professor Marey ("Vol des Oiseaux," p. 286) seems to have fallen into the error of supposing that a perfectly uniform breeze can be of assistance to a bird in mounting, if he turns his head towards it.

Mr. R. C. Gilson recently proposed to me to make some experiments in order to discover the rate of increase of velocity with increase of altitude, and to determine, if possible, up to what altitude the increase continues. These experiments were carried out, as far as circumstances allowed, with the assistance of Mr. J. T. Gilson last January. The place chosen was New Romney. There the large extent of flat sands and the absence of trees or cliffs render any irregular diversion of current unlikely. Experiments were first made at low levels with an anemometer. The following figures represent the averages struck from a considerable series of observations. It will be seen that with an occasional exception, due no doubt to the gustiness common to almost all winds, the increase in velocity grows less rapid as the elevation increases.

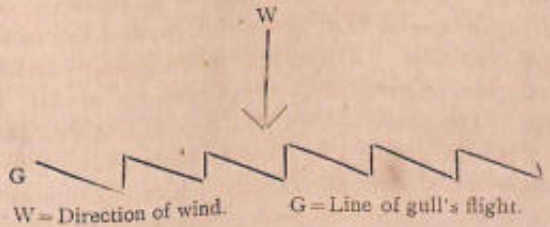
Altitude.	Velocity of wind per minute.	Altitude.	Velocity of wind per minute.
2 inches.	515 feet.	4 ft.	918 feet.
1 ft.	736½ ,,	7 ft. 6 in.	1,021 ,,
2 ft.	770 ,,		

A smaller series of similar experiments carried up to a height of 10 ft. 6 in. showed the same tendency. For greater altitudes the anemometer was to have been run up the string of a kite by means of a second string working through a pulley. But, unfortunately, an accident to the anemometer after a few trials made this method for the time impossible. We then fastened the kite to a spring balance and observed the pull at different heights, using a sextant to discover the exact elevation. Owing to alternating hurricanes and calms all work of the kind was often out of the question, and it is much to be regretted that we had not time to repeat the experiments. They seem to show, however, that there is a steady, though diminishing, increase up to a height of 240 ft. The pull registered at 357 ft. strikes the only false note in the scale, and when that was measured there was a drop in the wind that was perceptible to ourselves.

Altitude.	Pull of Kite.	Altitude.	Pull of Kite.
350 feet.	24 lbs.	126 feet	15–20 lbs.
420 ,,	25 ,,	60 ,,	14–19 ,,
357 ,,	19 ,,	42 ,,	11–20 ,,
204 ,,	18 ,,		

At low levels the jerking of the kite was a difficulty; there is also the question whether the greater length of string carried as the kite mounted higher may not have to some extent modified the results. Probably, however, the error due to the latter course was but slight.

Since making these experiments with a kite, I have tried the plan of letting go a number of small balloons, inflated with hydrogen gas in different degrees and allowing them to race; the larger rising to a height of 500 ft. or more, quite outpaced the lower. But as far as I could judge, small differences of elevation at the higher levels did not much affect the velocity. It would seem that larks in mounting continue to face the wind when they are too high to derive any benefit from it.



W = Direction of wind. G = Line of gull's flight.

While at New Romney I saw two illustrations of the cleverness of birds in turning the comparative slowness of the lower currents to account. Some gulls, wishing to make headway against the blizzard, flew so close to the ground that their wings almost touched it. On

another occasion several were progressing at right angles to the wind, without any apparent movement of their wings, by flying downwards and then making a rapid turn and facing the wind, which, naturally, lifted them as they rose from the slower to the more rapid currents.

The annexed diagram will show this clearly.

At the same place I also made some experiments with a vane working vertically up and down. These were suggested by Lilienthal's book, "Der Vogelflug als Grundlage der Fliegekunst," where he comes to the fantastic conclusion that the normal direction of a wind blowing over a level plain is from 3° to 4° upward. If a vane under these conditions were to point upward, it would be more reasonable to regard the fact as an indication of the eccentricity of all vertical vanes, or of that particular one. My own vane had for its larger arm a piece of thin deal 1 ft. long by 6 in. broad, exactly balanced by a lump of lead attached to the shorter arm. It is true that so small a deviation as 3° to 4° would be hard to detect, but this instrument seemed to indicate that a wind blowing over a level expanse is perfectly horizontal. Experiments on the direction of the wind on, or on either side of, a small barrier, had more interest for me. While standing on a bank only 2 ft. high, its tripod lifting it 4 ft. above the bank, the vane pointed decidedly upward. Five yards to leeward of a bank 8 ft. high, it indicated that the wind blew downward, making a large angle with the horizon; there was but rarely an upward gust. Ten yards from the bank the direction was still mainly downward, but with not infrequent upward movements. At twenty and thirty yards distance the wind came in wild gusts, as often upward as downward. To windward of a bank only rising 5 ft. above the level at a distance of 12 yards, the vane was not quite steady, but, on the whole, horizontal. At six yards there were occasional upward swings; at four yards there was a decided upward tendency, and this though the bank itself presented only a very gentle upward incline.

These results seem to me most important in connection with the soaring of birds. Near a mountain top there must be a perpetual alternation of up and down gusts which a bird can utilise. Happily, there are still hilltops in Great Britain over which the raven and the buzzard can be seen rising in majestic spirals without a motion of the wings. Over a level plain is there any varying velocity available? It is hardly likely that after, say, 400 feet from the ground there is any appreciable increase of velocity with increase of altitude. There is, however, as I have remarked above, an irregularity of another kind even in the steadiest wind. It comes in gusts, and a gyrating adjutant or vulture, long practised in feeling the pulse of the wind, may perhaps make use of this fact, descending when the breeze blows strong, turning when it slackens, and holding himself so that the breeze, when it freshens again, may lift him like a kite.

F. W. HEADLEY.

III.

The Natural History of the Flower.

THE past year was one of some interest in connection with this subject, in that it was the hundredth year since the publication of Christian Conrad Sprengel's book "Das entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen,"—"The Secret of Nature revealed in the Formation and Fertilisation of Flowers." Many papers have been written to celebrate the centenary: we may mention those of Knuth, Kirchner and Potonié, and one by the writer (in this Journal for April, 1893). A facsimile reprint of the classic work has also been published by Mayer and Müller, Berlin (8 marks), and will be found almost indispensable by those who take up this subject. Though it is 100 years since Sprengel started the study of flowers from this point of view, it is only during the last 40 years that any further work has been done, during which time the progress made has been very considerable. Of this work an excellent historical review is contained in the important paper by Macleod (see list), to which we refer later on. It is, unfortunately, in Dutch, but is not difficult to read.

The number of papers published during the last year is not very great, but many are large and some important. We shall deal in the first place with some of the smaller ones. Baroni has described *Rohdea japonica*, showing that it is fertilised by snails, insects, and perhaps worms. Briquet, in his monograph of *Galeopsis*, describes its mechanisms in detail. In *Freycinetia* Burck discovers the first case of a flower visited by bats. The male and female flowers are on separate plants (diœcious), and have large, bright rose-coloured fleshy bracts. These are visited and eaten by a bat (*Pteropus edulis*), which carries the pollen on its head from flower to flower. Miss Keller gives an account of the fertilisation of *Monarda fistulosa*, making the remarkable statement that in this conspicuous flower the anthers open in the bud and discharge some of their pollen upon the stigmas, which also open. The latter close; the flower opens and goes through the usual subsequent history, showing the protandry common in the genus. The same flower has also been described by Robertson and by the writer, who have agreed in looking upon it as well adapted to obtain cross-fertilisation by the agency chiefly of butterflies and large bees. Miss Keller's flowers were examined in November, and the lateness of the season may account for the curious results obtained. Kirchner gives an account of various

plants, usually, but according to him wrongly, supposed to be fertilised by wind alone. These he states to be, to some extent at least, fertilised by insects. Among them are the vine (*Vitis vinifera*), mistletoe (*Viscum album*), *Castanea sativa*, *Chenopodium album*, and others. He also discusses many Umbelliferæ, and gives several examples of protogynous species (that is, having flowers in which the pistil matures before the stamens), chiefly American, in this usually protandrous order (that is, having flowers in which the anthers are ready to shed their pollen before the pistil is ready to receive it). Lagerheim describes insect fertilisation (entomophily) in one of the Cyperaceæ (*Dichromena ciliata*), found near Panama, which possesses colour as an attraction to the insects. Meehan again mentions a number of flowers that appear to be regularly self-fertilised, *e.g.*, Eye-bright (*Euphrasia officinalis*), evening primrose (*Œnothera biennis*), *Cakile americana*, *Trifolium hybridum*, *Malva rotundifolia* (apparently fertilised in the bud), and others. Raunkiaer describes the Danish Cyperaceæ, stating that they are all protogynous, but especially the hermaphrodite ones. Robertson continues his series of papers on flowers and insects in Illinois, describing the mechanisms and insect visitors of various flowers. One paper deals entirely with Umbelliferæ and one with Labiatæ, and reviews of these orders are given, summing up their general features. Scott Elliot describes the visitors of a few British plants, but no points of special interest are brought out. The writer has described several species of *Phacelia*, all showing a method of fertilisation similar to that of *Hydrophyllum*, but some of them adapted for cross-, some for self-fertilisation.

We now turn to the larger papers upon flowers and insects. Several such have appeared, notably by Heinsius, Knuth, Macleod, and Verhœff. Each of these deals fully with a number of plants in some definite locality, and describes the mechanisms and the insect visitors, so that a comparison can be drawn between the various districts. Heinsius describes a number of flowers and their visitors in the Netherlands: 410 visits were noted, 341 on plants also investigated by Müller. In only 140 of these cases were the visitors identical with those noted by Müller; but their general character, as to families, was almost the same. Knuth gives a sketch of the flowers and insects of Capri; only 43 species of the latter were found visiting flowers, while on the mainland there are at least 5,000; the flowers are in consequence sparingly visited and depend mainly upon self-fertilisation, in some cases showing modifications in the floral mechanism calculated to ensure this. The same writer describes the flowers and insects of the North Frisian Islands for comparison with the mainland. Verhœff describes those of Norderney (West Frisian Islands). The results of these writers agree fairly well with one another, and with those of previous workers, and tend to show the relative poorness of the islands in insects and the correlated fact that many flowers usually fertilised by insects have taken more fully to

self-fertilisation, or to vegetative reproduction; the number of wind-fertilised plants, on the other hand, is larger in proportion than on the mainland. Lastly, Macleod publishes the first instalment of a great paper discussing the whole flora of West Flanders. A few new mechanisms are described, as well as many other plants which show some difference in their mechanism and visitors, when compared with the observations made by Müller and others who have studied them in different regions.

The chief interest of recent work upon flowers and their insect visitors seems thus to be in showing the variableness both of the mechanism and of the list of visitors, and the manner in which, when visitors are few, plants become more adapted to self-fertilisation, or take more largely to vegetative reproduction, a point which has been well brought out by Warming in his work upon flowers and insects in Greenland. It is evident from this that it is desirable to study each plant over a great part of its distribution area, and that it will not any longer suffice to say that a plant behaves in a certain way, or exhibits such and such a mechanism, without specifying where and when the observations were made. By careful study of plants in this manner, having regard not merely to their mechanisms for fertilisation in different places, but to the size and other relations of their parts, the profusion or otherwise of vegetative reproduction, the general composition of the flora (which largely determines the competition for insect visits), and other points, we may expect to obtain much valuable information bearing on the problems of geographical botany, of variation, and of evolution. It may here be pointed out that we possess at present scarcely any knowledge of the British flora in this direction. By a careful study of the genus *Medicago* in England, Burkill has observed that, while the mechanism is apparently the same as on the Continent, the visitors of the flower differ, and include a much larger proportion of flies than are observed abroad. The same worker, together with the present writer, has observed various flowers in Wales, and the results (as yet unpublished) support this view of the greater proportion of flies. Now this is just what Lindman and Aurivillius observed in the flora of northern Europe (Norway, Spitzbergen, Nova Zembla, etc.); hence our flora would seem rather more closely allied to these northern floras in this respect than to the nearer continental flora. It is to be hoped that this point may be worked out more fully.

Turning now to work in another direction, it is interesting to note that the experimental study of floral phenomena is making steady progress, and coming more and more into prominence. The most important paper of the year is probably that of Vöchting upon the influence of light upon the formation and development of flowers, especially cleistogamic flowers (those which never open and are necessarily self-fertilised). Plants of various kinds were grown at different distances from the laboratory windows during their flowering

period; the flowers developed under these conditions being carefully measured and compared with the normal type. The first plant described is *Mimulus Tilingii*; here the flowers, developed in poor light, were smaller than the normal, and in the extreme cases did not develop at all, but aborted in the bud stage. An interesting point in the gradual diminution of the corolla was the difference in the rate of decrease of the upper and lower lips. Down to a certain size of flower their proportionate dimensions were nearly constant, but below this the upper lip decreased much more rapidly than the lower. In *Linaria spuria*, a plant which very commonly bears cleistogamic flowers in the wild state, the decrease of illumination resulted in flowers of this type only. Similarly with *Lamium amplexicaule* and other plants. Chickweed (*Stellaria media*), it was found, could be made to produce either ordinary or cleistogamic flowers by suitable illumination. In the wild state it is cleistogamic in bad weather. In *Melandrium album* it was observed that the corolla was the first organ to show a decrease with diminution of the light. From these experiments it follows that, for the normal development of a flower, a certain intensity of light is required; but this varies largely in different species. Low intensity of light results in reduced flowers, the reduction showing first of all in the corolla; in plants with a tendency to cleistogamy, flowers of this kind are produced. Hence we may look upon decrease of light, perhaps, as the primary cause of cleistogamy, calling forth incipient flowers of this type, upon which natural selection may come into play. Vöchting also observed, most markedly in *Mimulus Tilingii*, that the low degree of illumination tended to increase the vegetative activity of the plant. In *Mimulus* the axis of the inflorescence gave rise, instead of flowers, to numerous vegetative shoots. By suitable treatment, the flowers were prevented even from beginning development, and vegetative reproduction alone occurred. Cleistogamy has also been studied by Graebner, who considers that it may be brought about by other agents as well, *e.g.*, by temperature or the weakening action of fungi, etc., or, again, by an unsuitable relation of light to temperature (if one be raised, the other must also be raised, or abnormal floral development will occur). This view is also taken by the present writer, who has studied the behaviour of *Salvia verbenacea* and other plants.

It is thus evident that a study of the natural history of the flower from a physiological standpoint has already led to important results and may be expected to yield many more in the future. The present writer has studied the phenomena bearing on the various distributions of sex by the experimental method during the last four years, drawing therefrom the general conclusion that all these, as well as cleistogamy, are primarily due to the action upon the plant of external causes. Natural Selection apparently only begins to act later on, when the phenomenon in question has been to some extent produced by these causes.

In conclusion may be mentioned some lines on which it seems that profitable work may be done. Experimental investigation into the origin of such phenomena as the various forms of flowers in the same species is much to be desired; also full and thorough study of the flowers and insects of definite districts, especially in the tropics. Our own country remains still to be investigated in this respect. Isolated observations are of little value; the mechanisms of almost all the species of the British flora have been described—on continental plants. We need a thorough study of the whole flora of certain regions, having regard to the composition of the flora, the time of year, the insect visits, the vegetative reproduction of the plants, the exact sizes of their parts, and a comparison of the mechanism with that of the same plant as elsewhere observed. As a model of such work may be taken Macleod's last paper on the flowers of West Flanders. As another line of research, may be suggested the comparison of the same flower as regards size of parts, mechanism, visitors, etc., at different times of year, having careful regard to the weather, in order to obtain an insight into climatal effects upon the parts of the flower, the dichogamy, etc. Those who have access to a botanic garden may also do useful work in observing the mechanisms of exotic flowers, especially those belonging to the many natural orders hitherto untouched, though, owing to the absence of the proper insects, such observations will necessarily be one-sided. In this connection, finally, we may venture to recommend Knuth's short essay, "Ueber blütenbiologische Beobachtungen" (Kiel, 1893: Lipsius and Tischer, 1 mk.), in which is contained a good account of the present state of the flower-theory and the methods of observation in use for this investigation.

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JOHN C. WILLIS.

IV.

Scorpions and their Geographical Distribution.

IT is a well-established fact that Scorpions have been in existence since the Middle Silurian era. But, unfortunately, almost all that we know of their geological history is furnished by a few specimens from beds of this age, and by a few more discovered in strata of the Coal period. It is also well known that the structural differences between the recent and the carboniferous fossil forms cannot be justifiably regarded in the light of an advance in organisation. In fact, as has already been more than once pointed out, these animals furnish an admirable example of a persistent type of life. From the in sistence of structural characters, a persistence of habit may be belo red. At all events, there is little, if any, evidence that these owi mals, in the past days of their history, have ever gained a livelihood n y means other than those employed at the present day. There is no v eason for thinking that they have ever adopted an aquatic or E rboreal mode of life, or have ever been dependent upon running or is aping powers for the capture of prey or avoidance of enemies; nor ar there any foundation for the belief that any species has ever been elped in the struggle for existence by the assumption of any of those disguises of identity that are generally spoken of as protective colouration, or mimicry. That an abundance of time to become specialised into any number of strange forms has been granted them, must be admitted on all hands; but, perhaps, no fact connected with the existing species of Scorpions so forcibly strikes the student as the wonderful closeness of the connection between the different genera that have been established. There is, naturally, a considerable amount of structural variation, if two extremes be selected for comparison; yet the extremes are linked together by such a series of intermediate types that the division of the order into sharply-defined minor groups becomes a task of no small difficulty. Now, this is hardly what we should have looked for in a group of such vast antiquity. We should rather have expected to find the living population of Scorpions composed of a larger, or smaller, number of more or less isolated forms, the annectent types between which would have to be sought for in the records of their past geological history. But, considering that, for obvious reasons, there is practically no record of Scorpion-life between the Carboniferous times and our own day, it is, perhaps, not

surprising that such small links as we require have not yet been found.

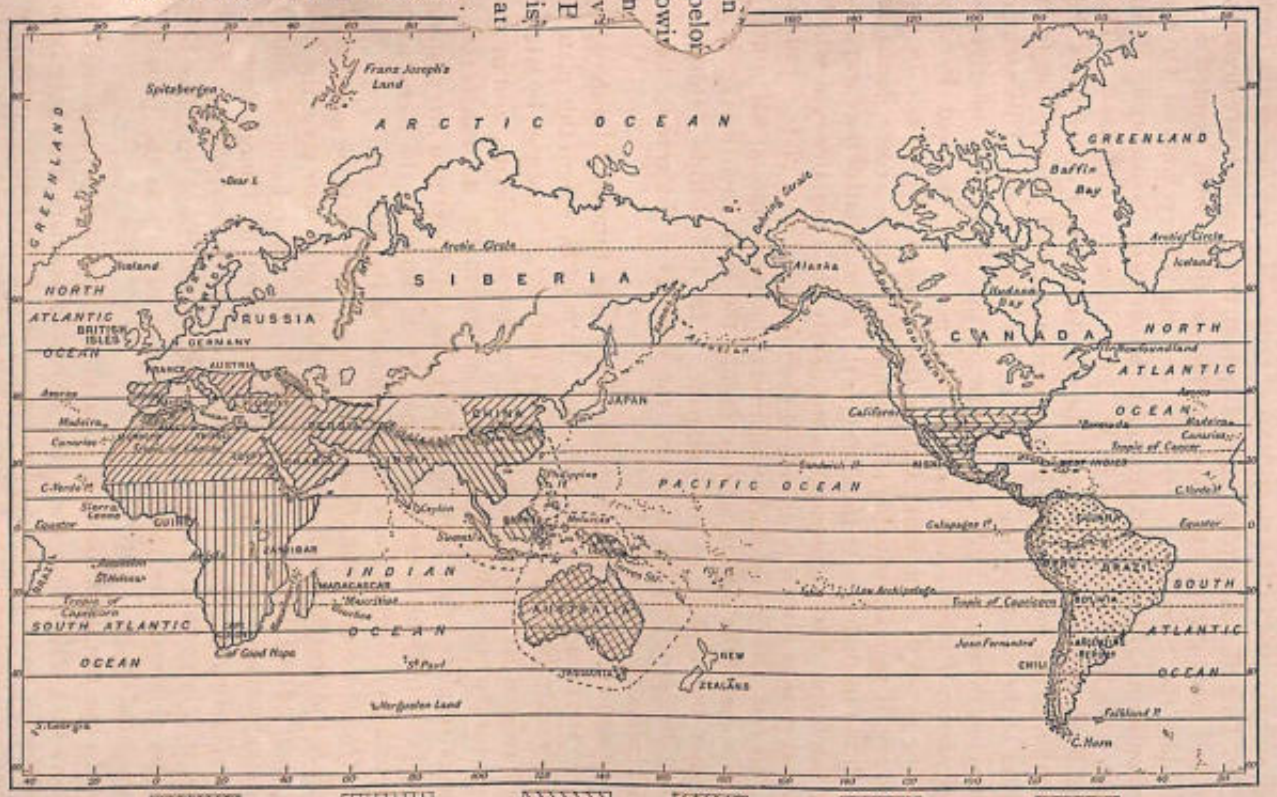
The relationship between the fossil and recent Scorpions has been summarised in the classification proposed some few years back by Dr. Thorell. According to this system, these animals are divisible into two great groups—the Apoxypodes, or those with pointed feet, including the Silurian *Palæophonus*, and the Dionychopodes for the Carboniferous Anthracoscorpii and the existing Neoscorpii, all of which have feet terminating in a couple of moveable claws. The chief distinctive feature between the two last-named divisions is the situation of the median eyes in the Anthracoscorpii close to the front border of the carapace, and in front of the lateral eyes. This archaic character, which is very noticeable in the Scorpion's relatives—*Thelyphonus*, *Phrynus*, and *Galeodes*—has not been retained by any existing genus of Scorpions; but, in the absence of evidence to the contrary, it seems justifiable to conclude that those recent forms in which the median ocular tubercle is in the anterior half of the carapace, approach the Anthracoscorpii more nearly than do those in which this tubercle is placed in or behind the middle of this plate. Judged by this test, the Scorpionidæ constitute the most specialised group; for in some of the genera of this family the eyes have moved ha far past the middle of the carapace. In the Iuridæ, Butlce Chactidæ, etc., on the contrary, the eyes are in the anterior haf the cephalothoracic shield.

Another test of specialisation seems to be furnished by the for of the sternum of the cephalothorax. In most Scorpions this plate pentagonal; but in the majority of the Buthidæ it is reduced by later compression to a small longitudinally triangular sclerite, and in t Bothriuridæ by antero-posterior compression to a transverse elongated plate. But that these two forms of the sternum are nothing but specialisations of the pentagonal type is shown by the fact that in the young of all Scorpions the pentagonal shape prevails.

One more apparent criterion of primitiveness may be mentioned. This is the presence in many of the Buthidæ of a spur upon the fifth segment of the last two pairs of limbs, this spur being seemingly homologous with a spur which has been figured by Thorell upon the corresponding segment of the legs in the Silurian *Palæophonus*. Moreover, it is important to note that among the genera of Buthidæ that possess this spur are those that have the sternum of the primitive pentagonal shape.

In the Buthidæ, then, and in no other group of Scorpions, we find genera that still retain the three archaic characters that have been mentioned, namely, the forward position of the median eyes, the pentagonal sternum, and the so-called tibial spurs. We would suggest, too, that the presence of a greater than the normal number of lateral ocelli in some of the Buthidæ may prove to be of interest in this connection. The usual number of these organs is three on each

MAP OF THE WORLD TO ILLUSTRAT[E] [T]HE DISTRIBUTION OF RECENT SCORPIONS.



Regions :—Palæarctic. (=Mediterranean.) Ethiopian. Oriental. Australian. Nearctic. (=Sonoran.) Neotropical.

side; but in some of the Buthidæ there are as many as five, and the fact that these five may be observed in genera that are not especially related points to the conclusion that the ancestors of this family possessed this number; and this conclusion seems to be borne out by the circumstance that the nearest living ally of the Scorpions, namely, *Thelyphonus*, which we believe to be the most primitive of all the Epectinate Arachnida,[1] may also present five lateral ocelli on each side.

The conclusion that the Buthidæ are, on the whole, the most archaic of living Scorpions is borne out by a study of the geographical distribution of these animals; for, as will be seen in the sequel, this family is represented by peculiar genera in all the quarters of the globe where Scorpions exist.

The absence of proofs of their great antiquity, which was referred to in connection with the inter-relationships of the existing species of Scorpions, is also revealed no less remarkably by a study of their geographical distribution; for in this latter respect they show considerable resemblance to such a relatively recent group as the Mammalia. No doubt our knowledge of the recent species of this latter class is very much more complete than is our knowledge of the Arthropods in question; but so far as we have been able to ascertain, Wallace's map of the Zoological Regions of the world, if we eliminate the northern parts of the so-called Palæarctic and Nearctic Regions where Scorpions do not occur, is closely applicable to this group of animals. We may, therefore, without further words of preface, proceed to discuss the characteristic features of the various regions.

In the Palæarctic Region the northern limit of Scorpions in Europe and Western Asia appears in a general way to be defined by the mountain-chain of which the Pyrenees is the western and the Caucasus the eastern extremity. East of the Caspian they have not been recorded north of the 40th parallel, but they are known to occur at two places upon this line, namely, at Kashgar in Turkestan, and at Pekin China. The two species from these localities are nearly allied to each other, and they unmistakably belong to the Mediterranean section of the genus *Buthus*. Furthermore, a species of *Buthus*, with distinct Palæarctic affinities, has recently been recorded from Shanghai. Consequently, we must conclude that the Mediterranean fauna extends from Spain and Algeria to the coast of China, with the probable omission of the table-land of Tibet. The southern limit of this area in China is, for want of data, uncertain; but since the Scorpions of Burma, Assam, and North India are totally different from those of Turkestan, Afghanistan, and Persia, we may

[1] Since, as Mr. Laurie has pointed out, the term Ctenophora, which I have recently assigned to the Scorpions as opposed to all other Arachnida which were called Lipoctena, is generally recognised as signifying a group of Cœlenterata, I wish to propose the terms Pectinifera and Epectinata as substitutes for the above.

safely adopt the boundaries that are usually recognised as separating the Palæarctic and Oriental Regions.

The exact limits between the Palæarctic and Ethiopian are also a little uncertain. Mr. Wallace draws the line of demarcation along the tropic of Cancer, and thus refers the northern half of Arabia to the former and the southern to the latter. But, with two exceptions, the Scorpions that occur at Aden, and at other parts of South Arabia, are either identical with Syrian and Egyptian forms, or are very nearly related to them. For instance, *Nebo*, which is met with at Aden and Muscat, is only known elsewhere from Syria and Egypt; *Hemiscorpius* has been obtained at Aden and Baghdad; *Buthus quinque-striatus*, a common Egyptian species, extends to Perim Island and South Arabia, and the Algerian and Egyptian *Prionurus australis* has been received from Muscat. On the other hand, the only Ethiopian types which are found in South Arabia are a species of *Scorpio* and *Parabuthus liosoma*, which ranges from Egypt to Masai-land. Clearly, therefore, the Mediterranean element in South Arabia far surpasses the Ethiopian, and the whole of this country must consequently be looked upon as part of the Palæarctic Region. In Africa it is impossible to draw a hard and fast line until more collecting has been done. We know nothing of the species which presumably inhabit the vast tract lying between Abyssinia on the east and Senegambia and Guinea on the west. On the west coast of the Red Sea, however, the Mediterranean element is represented at Massowah and at Zaila, in Somali-land, by a variety of the Spanish and North African *Buthus europæus*; but species of the essentially Ethiopian genus *Scorpio* extend into Nubia and Abyssinia. A similar partial overlapping of the two faunas occurs on the West Coast of North Africa, namely, in Senegambia, which is the northern limit of *Scorpio*, and the southern limit of *Buthus europæus* and *Prionurus australis*. We may, therefore, draw the line between these two regions along the 15th parallel or thereabouts.

The following is a list of the Palæarctic genera with the known range of each. Those that are peculiar to the region are marked with an asterisk:—**Heterometrus*, two species, North Africa, Syria; **Nebo*, two or three species, Syria, Egypt, and Aden; **Hemiscorpius*, Baghdad, Aden; **Iurus*, one species, Greece, Asia Minor, Egypt; **Belisarius*, one species, Pyrenees; **Euscorpius*, many species, Spain and Algeria to Asia Minor; **Prionurus*, two species, North Africa to Persia and Arabia; *Buthus*, many species, North Africa, South Europe, Persia to Pekin; **Butheolus*, two species, Sicily, Arabia, Persia; *Parabuthus*, one species, Egypt, Arabia.

The Ethiopian Region is very rich in Scorpions, and contains a large number of peculiar genera. It embraces Africa south of the 15th parallel, and Madagascar, the two areas representing distinct sub-regions. The African sub-region is peopled with Scorpionidæ, Ischnuridæ, and Buthidæ. The following is a list of the genera:—

(Scorpionidæ) *Scorpio*, about half a dozen species, Senegambia and the Congo to Abyssinia and Nyassa-land; **Miæphonus*, two species, Caffraria, Damara-land; **Œcopetrus*, two species, Damara-land, Mozambique; **Opisthophthalmus*, many species, Mozambique, Namaqua-land, Cape Colony; (Ischnuridæ) **Ischnurus*, several species, from Angola and Mozambique southwards; **Opisthocentrus*, four or five species, Congo, Transvaal, and Cape Colony; **Chiromachus*, one species, Mozambique; (Buthidæ) **Uroplectes*, several species, Nyassa-land and Cape Colony; **Lepreus*, many species, southwards from the Congo and Somali-land; *Archisometrus*, two species, Congo, Nyassa-land; **Babycurus*, several species, Old Calabar and Angola to Masai-land; *Parabuthus*, several species, Abyssinia, Benguela, Cape Colony; *Buthus*, a few species only, Senegambia and Abyssinia to Caffraria.

At the present time we unfortunately have no intimate knowledge of the Scorpions of Madagascar. Two peculiar genera of Buthidæ, however, namely *Grosphus* and *Tityobuthus*, occur, and two species ascribed to *Babycurus* have been recorded; there is also one peculiar species of *Opisthocentrus* known. The last two are African genera and the first two are essentially African in their affinities. It is, however, highly interesting to note the apparent absence from this continental island of almost all the large African genera of Scorpionidæ and Ischnuridæ. The differences between the genera of Buthidæ inhabiting Africa and Madagascar point to long separation between the two areas; while the absence from Madagascar of the Scorpionidæ seems to indicate that the latter made their way into Africa after the separation had been effected.

The Oriental Region offers some points of resemblance to the African portion of the Ethiopian; but on the whole the two faunas are very distinct. The following genera are known:—(Scorpionidæ) *Scorpio*, several species, India, Ceylon, Java; **Palamnæus*, about four species, Bengal to Borneo and the Philippines; (Ischnuridæ) **Iomachus*, one species, South India; *Hormurus*, one or two species, from Burma eastwards; (Iuridæ) **Scorpiops*, several species, North India, Burma; **Chærilus*, several species, Cashmere and Ceylon to Borneo; (Bothriuridæ) **Timogenes*, one species, Sumatra; (Buthidæ) *Buthus*, one species, India; *Isometrus*, two species, South India, Assam; *Archisometrus*, several species, India, Ceylon, and Burma eastwards; **Charmus* (*Heterocharmus*), three species, Ceylon. The region seems separable into two sub-regions, a western or Indian, containing Ceylon and the greater part of India, and an eastern or Indo-Malayan, extending from Cashmere along the Himalayas to Burma, and thence southwards and eastwards to Borneo and the Philippines. Characteristic of the Indian sub-region is the genus *Scorpio*, which in the Indo-Malayan sub-region has only two representatives, occurring in Sumatra and Java; the single Indian species of *Buthus*, too, does not appear to pass into Burma, while *Iomachus* and *Charmus* are not known from the east side of the Bay of Bengal. In

the Indo-Malayan sub-region the genus *Scorpio* is almost entirely replaced by its near ally, *Palamnæus*, which ranges from Bengal to Borneo and the Philippines. *Hormurus australasiæ* and *Archisometrus mucronatus* have approximately the same range as *Palamnæus*, and are not known from India or Ceylon, while the genera *Scorpiops* and *Chærilus* extend from Cashmere into Assam, Burma, and Tenasserim. Here *Scorpiops*, of which one peculiar species has been recorded from the Deccan, reaches its limit; but *Chærilus*, which is also represented by a peculiar form in Ceylon, passes into Java, Sumatra, and Borneo. These two genera are of special interest, inasmuch as they are allied to the Palæarctic genera *Iurus* and *Euscorpius*, and thus indicate faunistic affinity between the Oriental and Palæarctic regions. A corresponding but rather closer affinity is indicated with the Ethiopian Region by the presence of *Scorpio* in India and Ceylon, and of *Archisometrus* over the whole Oriental Region.

The Australian Region is not easily separable from the Oriental where the two are nearly contiguous. It may be divided into two sub-regions, a northern insular portion or Austro-Malaya, and a southern continental portion or Australia. The following genera are represented:—(Ischnuridæ) *Hormurus*, several species, Celebes, Papua, Solomon Islands, New Caledonia, E. Australia; (Urodacidæ) **Urodacus*, several species, Australia; (Bothriuridæ) *Cercophonius*, one species, Australia; (Buthidæ) *Isometrus*, one species, E. Australia; *Archisometrus*, about three species, Celebes and Flores, Australia; **Isometroides*, two species, Australia. The Australian sub-region is recognisable from the Austro-Malayan by the presence of the genera *Urodacus*, *Cercophonius*, and *Isometroides*, which do not extend into the latter area. This Austro-Malayan sub-region is near akin to the Indo-Malayan. Two species, namely *Archisometrus mucronatus* and *Hormurus australasiæ*, are common to the two; and the characteristically Oriental genus *Chærilus* is represented by a single species in Celebes. But with the exception of *H. australasiæ*, the genus *Hormurus*, of which there is a peculiar species in New Caledonia, does not appear to cross Wallace's Line. There are no indigenous species of Scorpions in New Zealand and none, so far as has been ascertained, in the Pacific Islands.

In North America the northern limit of Scorpions seems to be situated between the 35th and 40th parallels. They thus belong to the Sonoran Province of Merriam. To the west of the Rockies these animals have been recorded from California and Utah; and to the east of this mountain chain they appear to extend into Kansas, Carolina, and Virginia. To the south, that is in Central America, the fauna seems to blend with that of the Neotropical. At all events, we have not sufficient data to enable us to define accurately the limits between the two regions.

Characteristic genera of the Sonoran Province are **Uroctonus* (*Anuroctonus*), three species, California, Utah, Virginia; **Hadrurus*,

one species, Utah, California; **Vejovis*, several species, Carolina, Texas, and Utah; and *Centrurus*, two or three species, from Georgia, Florida, Texas, Kansas, and California. Of these *Uroctonus* does not appear to pass into Mexico; but *Hadrurus hirsutus* extends certainly as far as Jalapa, while *Vejovis* is represented by many species in various parts of Mexico, but does not, so far as is known to me, spread into Nicaragua or Costa Rica. Consequently, we must conclude that the greater part of Mexico belongs to the Sonoran Province. The genus *Centrurus*, which inhabits the southern States, seems to form part of the Neotropical fauna; for it is represented by several species inhabiting Central America, the West Indies, Colombia, etc. Similarly, *Diplocentrus* is common to the two regions, since it occurs in Texas, Mexico, the West Indies, and Brazil. But with these exceptions the Neotropical fauna of America, including Panama and all the countries to the south of it, as well as the West Indies, is very distinct from that of all other countries. The following genera are found:—(Diplocentridæ) *Diplocentrus*, two or three species, West Indies, Brazil; (Ischnuridæ) **Opisthacanthus* one species, Panama, Colombia; (Iuridæ) **Hadruroides*, four or five species, West Indies, Peru, Bolivia; **Caraboctonus*, one species, Chili, Brazil; (Chactidæ) **Broteas*, four species, Guiana, Brazil, La Plata; **Megacormus*, one species, Mexico; **Broteochactas*, three species, Guiana, Venezuela, Colombia; **Hadrurochactas*, one species, Brit. Guiana; **Teuthraustes* two species, Eucador; **Chactas* and **Heterochactas*, about twelve species, Venezuela, Colombia, Ecuador, Peru; (Bothriuridæ) **Bothriurus*, six or eight species, Peru, Chili, Uruguay, South Brazil; **Brachistosternus*, three or four species, Peru, Chili, Argentine; **Mecocentrus*, one species, Brazil; **Phoniocercus*, one species, Brazil; **Thestylus*, one species, Brazil; **Centromachus*, one species, Chili; **Urophonius*, two species, Chili, Argentine, Uruguay; (Buthidæ) *Centrurus*, many species, West Indies, Colombia, Chili; **Heteroctenus*, two species, Hayti, Mexico, Brazil; **Tityus*, many species, Mexico, West Indies, Colombia, Brazil, Argentine; **Ananteris*, one species, Brazil.

These data show how very specialised is the Neotropical fauna. The Chactidæ are confined to the region, and, with the possible exception of *Cercophonius*, none of the Neotropical genera of Bothriuridæ are found elsewhere. Moreover, three out of the four genera of Buthidæ are peculiar; so, too, is the single genus of Ischnuridæ, while the remaining genus of Buthidæ occurs elsewhere only in the Sonoran Province of North America.

Nothing is known of the Scorpions of Patagonia. Darwin, however, tells us that they are found at least as far to the south as the 47th parallel. It is, of course, highly probable that the species of this country will prove to be nearly related to those of the Argentine Republic.

At present our limited knowledge makes it hardly possible to

divide the Neotropical Region into sub-regions. In a general way, however, the northern parts of the area seem to be characterised by the presence of Chactidæ, and the southern by the presence of Bothriuridæ; and it is interesting to note that both of these groups are absent from the West Indies. There are, too, some further points of interest connected with these islands. For instance, the species known from the larger islands—*i.e.*, Cuba, Jamaica, Hayti, and Porto Rico—are, for the most part, also found on the mainland of Central or South America; while the species that have been obtained in the smaller islands to the south-east—*e.g.*, Montserrat, St. Lucia, St. Vincent, and Grenada—are peculiar forms of the Central and South American genera *Tityus* and *Diplocentrus*.

Some of the facts connected with the distribution of Scorpions set forth in the preceding paragraphs offer interesting points for further discussion.

It is to be observed, in the first place, that arms of the sea constitute barriers which these animals cannot pass; yet, although their means of dispersal are very limited, the antiquity of the group is so great that it has succeeded in distributing itself over all contiguous land areas of the globe. In fact, as we have seen, Male animals have been met with in all parts of the continents which have been explored, with the exception of the colder parts of the Northern Hemisphere, and in all continental islands with the single exception of New Zealand. But we know that in pre-Glacial Tertiary times they extended in Europe as far to the north as what is now the southern shore of the Baltic; and there cannot be much doubt, one would think, that their absence at the present day from all that area of the Northern Hemisphere which lies north of the 45th parallel of latitude is to be attributed to the recent glaciation of this part of the world.

But their absence from New Zealand is not to be so easily explained away. It is clear, however, that we have to account for the fact upon one of two hypotheses, namely, that they have never succeeded in reaching the country in question, or that they have been exterminated since arriving there.

The first hypothesis, however, seems hardly likely to be the true one, if there has ever been an uninterrupted and long-continued land-connection between New Zealand and the south-eastern parts of Asia by way of Australia or Austro-Malaya. But if, on the other hand, the connecting land was composed merely of islands, the intervening seas would probably have afforded an effectual barrier to their migration.

With regard to the hypothesis of extermination, it may be said that, so far as is known, there is nothing in the physical conditions of New Zealand antagonistic to Scorpion life, and it must be left to the

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With regard to the hypothesis of extermination, it may be said that, so far as is known, there is nothing in the physical conditions of New Zealand antagonistic to Scorpion life, and it must be left to the

geologists to decide whether there is any evidence in favour of the glaciation of this country which would account for the extermination of Scorpions, supposing they had ever succeeded in establishing themselves there. For myself, I am disposed to think that none of the recent groups of Scorpions have ever been represented in New Zealand; in which case it seems certain that the existing Scorpion population of Australia made its way, like the Mammalia, into the latter country after the isolation of New Zealand.

So, too, with Madagascar. The absence of nearly all the large Ethiopian forms from this island shows that the severance from the mainland took place before the typical African genera had appeared in the country. And the presence in Madagascar of genera of Buthidæ and Ischnuridæ, peculiar but with marked African affinities, points to the conclusion that, at the time of the connection with Africa, species of these two families were the principal if not the sole representatives of Scorpions in the Ethiopian Region.

A discussion of the fauna of Madagascar naturally leads us to inquire whether a study of the distribution of Scorpions affords any support to the hypothesis of a former direct connection between this island and the Oriental Region. But since there is no similarity between the species of the two areas, we may dismiss the subject by saying that the Scorpions do not furnish a particle of evidence that the union has ever been more complete than it is at present.

The absence of evidence in favour of Lemuria obviously suggests an examination of the question of the existence of Antarctica, to which many other groups bear witness. But here again all the evidence is on the negative side. For, firstly, there is the absence of Scorpions from New Zealand; secondly, total dissimilarity between the Scorpions of Madagascar and Australia, and of South Africa and South America; and, lastly, the almost complete want of resemblance between the Scorpions of Australia and those of South America. The one point of resemblance between these two countries is the presence in Australia of a genus of the Bothriuridæ, a family which is almost exclusively confined to the Neotropical area. This case has been cited by Mr. Forbes, in his interesting paper on the Chatham Islands, as an item of evidence in favour of Antarctica. But, since furnishing Mr. Forbes with the information that the Australian *Cercophonius squama* occurs also in Chili, I have found grounds for doubting the truth of the evidence upon which the information was based. If, however, it is so, the fact of its being the *same* species that occurs on these two shores of the South Pacific favours the view of artificial or fortuitous introduction; for apart from this one there is not a single species or even genus which is common to the Old and New Worlds. But whether or not the genus *Cercophonius* occurs in Australia and South America, we still have to account for the presence in the former country of a Scorpion belonging to a family which is typically South American. It is possible, of course, that this may be explained, as Mr. Forbes

supposed, on the hypothesis of a southern land-connection between the two countries. But this is not the only theory that will account for the circumstance; for it is possible that the family to which the genera belong had once a wide extension in the Northern Hemisphere, and that although exterminated, for the most part, in the keener competition of more northern areas, some of the genera, like the Marsupial Mammals, have found places of refuge in the countries to which they are now restricted; and this supposition is borne out by the fact that a solitary representative of the family has been recently discovered in Sumatra.

It is necessary to bear in mind that the general absence of similarity between the Scorpions of the three southern extensions of land is not of the same value in disproving the former existence of Antarctica as is the absence of similarity between Madagascar and the Oriental Region in disproving the existence of Lemuria; for while it is clear that the Scorpions could have freely migrated across a land lying in the centre of the tropical Indian Ocean, it is equally clear that, unless there was a long-continued genial climate in the Antarctic Seas, they could never have succeeded in making their way even along the northern coast-line of the southern continent that Mr. Forbes has delineated.

A further modification of Sclater and Wallace's Zoö-geographical Map that has been suggested, and often adopted, is the union of the so-called Nearctic and Palæarctic into one region, for which the term Holarctic has been proposed; but a study of the distribution of Scorpions affords no support to this change, inasmuch as it shows that none of the genera extend from one region to the other, and even when the families are taken into consideration, it is found that the similarity between the Palæarctic and Nearctic is no greater than that between the Palæarctic and Oriental.

There still remain one or two apparent anomalies in the distribution of some of the families of Scorpions to be accounted for. The first and strangest of these, furnished by the Ischnuridæ, is the unquestionably close relationship that exists between the exclusively South African genus *Opisthocentrus* and the genus *Opisthacanthus*, which is restricted to Panama, Colombia, etc. A somewhat similar peculiarity is found in the case of the two known genera of the Diplocentridæ, namely, *Diplocentrus*, which is found in the Antilles and Central America, and *Nebo*, which occurs in Arabia, Egypt, and Syria. Again, coming further to the north, we find that in the Iuridæ the Levantine genus *Iurus* has one of its nearest allies in the Californian *Uroctonus*, and that the Neotropical group of the Chactidæ has a South European representative in *Euscorpius*.

These four cases, taken together, seem to me to point to the conclusion that, at one period, these families had a widely-extended range in the Northern Hemisphere, and were enabled thereby to pass from Eastern Asia into Western North America. And the one item of

information we possess regarding comparatively recent fossil Scorpions is corroborative of this supposition. I refer to the case of a Scorpion described as a *Tityus* from the amber beds of the Baltic. From this we learn that, in pre-Miocene Tertiary times, Scorpions in Europe reached as far to the north as the 55th parallel, that is, to about the same latitude as the chain of islands which now connects Kamschatka with Alaska. Moreover, although the generic determination of this Scorpion cannot be unreservedly accepted, yet it is highly important to note that, at the present day, in the Old World the only Scorpions which could reasonably be identified as *Tityus* are found to the south of the tropic of Cancer. It seems, therefore, legitimate to conclude that in pre-Glacial Tertiary times the ancestors of our existing Old World tropical Scorpion fauna extended far enough to the north in the Europæo-Asiatic continent to pass freely along any land-connection with North America. But the advent of glacial conditions in later Tertiary times, and the consequent extermination of the Scorpions in the northern parts of the Northern Hemisphere, would soon put an end to the intercourse between the Asiatic and North American species; and these, under the changed conditions, might have become differentiated into the distinct types which inhabit these countries at the present day.

This supposition, then, of the former wide distribution in the Northern Hemisphere of the ancestors of the Ischnuridæ and other tropical or sub-tropical families, furnishes, it seems to me, the most satisfactory explanation of the resemblances and differences that are observable between the Scorpions of the Old and New Worlds. Moreover, the restriction of the Diplocentridæ and Ischnuridæ at the present time in South America to the northern parts of this region points to relatively recent immigration of these groups from North America. In fact, it seems probable that they made their way into the country at a time when it was occupied by ancestors of the existing Neotropical Bothriuridæ and Buthidæ; and this last conclusion points to the further one that these latter two families belong to an older type than the two before-mentioned groups, which are nearly related to the Scorpionidæ. This hypothesis, too, is borne out by the confinement of the Bothriuridæ in the Eastern Hemisphere to Sumatra and Australia, and by the fact that, although the Buthidæ are cosmopolitan in their range, yet peculiar genera of the family are found in South Africa, Madagascar, Ceylon, Australia, and South America.

R. I. POCOCK.

V.

Further Notes upon Habits of Insects.

IN a former review contributed to NATURAL SCIENCE (vol. iii., p. 267), I referred to Herr Verhoeff's researches into the habits of the social and solitary wasps and bees. This interesting subject has lately received further attention from a French observer, M. Paul Marchal, who (1, 2) gives details of the habits of two genera of the digging or fossorial wasps—*Bembex* and *Crabro*. He observed *Bembex rostrata* on coast sand-hills, and watched the operations of digging the nest and catching the prey to serve as food for the grubs. The prey of *Bembex* consists of species of *Eristalis*—large two-winged hovering-flies. M. Marchal collected a stock of these insects and, having attached the end of a silk thread to a leg of each, conveyed them to the haunts of the *Bembex*, where he let one fly, retaining the other end of the thread. The *Bembex* seized the *Eristalis* with great rapidity, held the fly back downwards beneath her body and pierced it with her sting. On releasing the fly from its captor, it was found to be quite motionless, except for feeble movements of the jaws and abdominal viscera, and in a few days it was dead and began to dry up. Fabre established the fact that *Bembex* brings a fresh fly each day to her progeny; hence there is no necessity that the life of the victim should be prolonged, as in the case of the prey of many sand-wasps.

On some occasions M. Marchal was surprised to find the *Bembices* engaged in furious and apparently aimless digging, giving the observer "l'impression d'être de véritables maniaques." His captive flies were neglected, or one put directly in front of the *Bembex* only caused the wasp to move away a few inches and recommence her strange pastime. This is clearly an aberration of the instinct of nest-making, and M. Marchal suggests that it may overcome the insect when, by reason of a high wind or otherwise, she is prevented from hunting.

Crabro cephalotes was observed to make her nest in an old felled walnut tree, by forming a tunnel curved like an irregular **3**, from which proceeded branches, some of them forked: at the end of these were the cells where the eggs were laid and the grubs hatched. Each cell was sealed with a plug of sawdust filling up its branch of the tunnel, after the wasp had placed in it from six to ten paralysed flies —mostly *Syrphi*. In carrying these into the nest, she was observed

to walk on five or four legs, holding the prey beneath her by one or both legs of the intermediate pair. All her maternal care did not ensure the safety of her offspring from the attacks of a large ichneumon fly, *Ephialtes albicinctus*, which was found in the tunnel, and whose grub and pupa were found in the cocoons of the *Crabro*. The pupa (the limbs of which are, as is usual in hymenoptera, free) can turn itself over when disturbed.

In the paper in NATURAL SCIENCE already mentioned, I also referred to Dr. Sharp's researches upon stridulation in ants. M. Ch. Janet has (3) been working at the same subject, but, like the former observers mentioned by Dr. Sharp, he does not distinguish between the true stridulating organs and the ordinary sculpture of the segments. He mentions that a number of red ants (*Myrmica*) imprisoned between two pieces of glass fixed by cement, made a noise like the gentle boiling of water in a closed vessel, when their prison was held to the ear. The noise was specially audible when the ants were irritated by the observer blowing between the two pieces of glass.

In NATURAL SCIENCE for November last (vol. iii., p. 353), Mr. Rothera gave us an interesting paper on gall-producing insects, in which he brought forward arguments against the view that the formation of a gall is due to poison deposited with the egg in the plant by the parent insect. Dr. F. Heim, however, in a recent memoir (4), defends this view. His observations were not made upon any of the gall-flies (Cynipidæ), but upon a species of saw-fly (*Nematus salicis*, Jur.[1]), whose grubs pass their early stage within galls produced upon the leaves of willows. The female fly appears in May and makes a triangular cut close to one of the veins of the leaf; in this the eggs and poison are laid, and the gall is formed before the grub is hatched. It is evident, therefore, that the malformation cannot be due to any mechanical irritation by the larva, but from Mr. Rothera's article it appears that in such a case he would attribute it to some chemical irritant in the egg. The gall takes three or four weeks to attain its full size, by which time it appears as an ellipsoidal swelling, 1 cm. long, 5 mm. wide, and 5 mm. thick, of a paler green than the normal hue of the leaf, and with a somewhat purplish tint. Transverse sections showed the gall to have an epidermis with thickened cuticle, almost destitute of stomata; beneath this were cells containing a red colouring matter, shown by chemical tests to be a derivative of tannin, like the pigments found in fruits; the internal layer consisted of small cells, very rich in chlorophyll. The grub, when hatched, is of the usual saw-fly type and colourless, but becomes greenish by feeding. It undergoes no moult within the gall. After two or three weeks it makes a round opening in its habitation, by

[1] This insect is not *Nematus salicis*, L., whose grubs do not live in galls, but spend all their lives free on the leaves. I have not been able to trace the synonymy.

which, a few days later, it emerges, and passes the rest of its life, which lasts several weeks more, feeding freely upon the leaves of the tree. This is an observation of much interest, giving us a link between the habits of grubs like those of *N. salicis*, L., which feed openly all their lives, and those like *N. salicis-cinereæ* and *N. gallicola*, which do not come out of their galls till nearly ready to pupate. The pupal stage of the present species is passed in a cocoon buried in the ground. The brood of flies, consisting entirely of females, appears in August, and a parthenogenetic egg-laying ensues. The grubs of this second generation pass through a similar experience to those of the summer brood, and are full-fed by the time the leaves of their food-tree are ready to fall. The winter is passed by the insects underground as pupæ, and among the flies produced from these in the following spring a few males are found. Thus, as in so many other cases among insects, sexual and virgin reproduction alternate.

Dr. Heim concludes his memoir with some general considerations on galls, in which, as already mentioned, he defends the view that those structures are due to the action of poison injected by the parent insect. He compares the dense parenchyma of the gall of *Nematus* with the cell-proliferation which occurs in cancerous tissues in man, criticises M. Beyerinck's comparison of the structure of galls to that of shoots, and calls attention to the analogy between galls and fruits, suggesting that the action of the insect-poison on the leaf-tissues is comparable to that of a secretion of the pollen-tube upon the carpels of a fertilised flower.

Though the hymenoptera surpass all other insects in the interest of their habits, much is to be learnt from the other orders. Dr. A. Seitz has recently (5) collected a large number of noteworthy facts regarding the butterflies and moths, with special reference to their feeding in the larval and perfect stages. The first meal of a young caterpillar is well-known to be often its empty egg-shell; from this it turns to feed upon the leaves whereon its provident parent had laid her eggs. But in a few cases hatching takes place in winter or early spring, and the young larvæ have then to find a temporary food until their own special plant is available. For example, the caterpillars of some species of *Xanthia* and other noctuid moths feed at first upon willow-catkins. All who have tried to rear caterpillars know that, while those of some species will feed only on one particular species of plant, others will eat several species of the same genus or family, while others again are still less particular, some being able to feed on almost any green herb. That caterpillars, in this respect, have often very nice tastes there can be no doubt, and Dr. Seitz suggests further that the jaws of certain species are unable to bite leaves of different textures. It is curious to note how certain species change their food in different localities, a caterpillar confined to one plant in one place being less particular elsewhere. Individual aberrations in food are of special interest, in suggesting the starting

point for a change in the race. When we consider the vast numbers of the lepidoptera, and the structural modifications which they have undergone, their generally faithful adherence to a vegetable diet (mostly leaves, rarely wood) is remarkable. The clothes-moths (Tineids) have invaded our dwellings and found a congenial food-stuff for their larvæ in our garments. A few small species of the same group (*Ephestia*) are reared in meal and other human food-stores, while the caterpillars of some pyralid moths (*Asopia*, *Aglossa*) feed upon kitchen refuse. Two species of crambid moths (*Aphomia sociella* and *Galleria melonella*) find a home in bee-hives, where their caterpillars feed upon the wax, while the waxy secretion from the body of the great American lantern-fly (*Fulgora candelaria*) serves both as shelter and food for the caterpillar of the moth *Epipyrops anomala*. Very few caterpillars have developed a thoroughly carnivorous habit. *Cosmia trapezina* feeds on oak and other leaves, but devours smaller caterpillars which happen to get in its way, and if shaken from the tree, eats other larvæ while climbing the trunk. *Xylina ornithopus* and a few other species are said to be always carnivorous when opportunity offers; the small looping caterpillar of a "pug"-moth (*Eupithecia coronata*) has been observed to eat a larva three times as big as itself. The caterpillars of *Orthosia pistacina* live together of a peace while their food is moist, but devour each other when it g the up; this is true cannibalism—a term which should not be applied ds the habit of preying on another species. Dr. Seitz remarks that il- vertebrate animals also, thirst is more maddening than hunger. Canni, balism has been observed in a few other cases. Patagonia yields severed examples of carnivorous caterpillars, as might be expected from thd poverty of its flora. A few carnivorous caterpillars do not attacf; other caterpillars, but prey upon insects of different kinds; among these *Fenescia tarquinius*, which eats aphides, and *Erastria scitula*, which feeds upon scale insects, must be reckoned as benefactors to mankind.

The life-history of the latter moth has lately been worked out in detail by Dr. H. Rouzaud, and is described (6) by Professor V. Riley. It inhabits the shores of the Mediterranean, and its caterpillar devours the coccids upon various fruit-trees, specially the black-scale (*Lecanium oleæ*) of the olive. The moth, which is a small noctuid, the white markings on whose wings give it the appearance of a bird-dropping when at rest in the daytime, appears in May, and lays her eggs, singly and far apart, upon the trees infested by the coccids. When hatched, the young caterpillar selects a large female coccid, eats its way through the scale, and devours the insect beneath; having done this it makes its way to a fresh victim. As it increases in size it forms a case for itself made of the scales of its victims, excrement, etc., bound together by silk which it spins, and, protected by this covering, which closely resembles the smut-covered bark of the tree, it roams about during its later stages, devouring several

coccids every day. The caterpillar is short and stumpy in shape, tapering towards the head, and recalling in appearance the maggots of some flies. Only the three hinder pairs of pro-legs are functional; those on the fifth and sixth abdominal segments are used for walking, the hindmost pair being attached to the case, which they keep in position. When full-fed, the caterpillar chooses a crevice in the bark or a fork between two branches, where it fixes its case, spins a cocoon, and pupates, having first taken the precaution to gnaw an aperture through which the moth can come out. So rapid is the transformation, that four or five broods follow each other during the summer. The execution upon the coccids must, therefore, be very great, and Professor Riley states that it is intended to import the moth into California, where it is hoped that it may prove as serviceable as the other predatory insects which have been introduced there to make war upon the destroyers of the fruit-trees.

After discussing carnivorous caterpillars, Dr. Seitz gives instances of the habit of nest-making which some caterpillars practise, living in colonies, with the web spun by their united labour as a protection. The influence of the food of the larva upon the colour and size of the imago is next considered. Some observers have thought that the [illegible] of the moth is affected by the food of the caterpillar, poverty of [illegible]ment producing males; but Dr. Seitz points out that the fact on [illegible]n this rests—that some neglected larvæ of a batch pupating [illegible]ly produced males, and the rest, fed afterwards, females—is better [illegible]plained by supposing a tendency to an earlier development of [illegible]ales, or proterandry (if a botanical term may be borrowed). There [illegible]n be little doubt that the sex is irrevocably fixed in the egg. The [illegible]ference of feeding by which bees can rear a worker or a queen at [illegible]l is not, of course, an instance of determination of sex by food, as the worker is only an infertile female.

Dr. Seitz discusses also the causes which govern the time of development of a species, pointing out the advantage of rapid growth, when climatic conditions permit, in giving rise to several broods, and thus enormously increasing the number of individuals. The relation between caterpillar and imago, as regards size, is of considerable interest, the body of the latter being in some cases much smaller than that of the former, while in others the difference is less marked. In some moths we find a complete division of labour between the larval and perfect stages, the former being entirely devoted to feeding, the latter to reproduction. In such the mouth-organs of the moth, which lives but a few days, are vestigial. Dr. Seitz suggests that, as this condition exists in some of the most primitive families (Cossidæ, Hepialidæ), these may have come down to us unchanged from a period anterior to that of honey-bearing flowers. In some of the highest moth-families, however (Saturniidæ, Arctiidæ), we find the same state of things, and it seems more likely that in all cases it is a degradation and not a survival. A review of the habits of the butter-

flies and moths which do feed concludes Dr. Seitz's paper. He mentions various observations of these insects drinking water, or dew from leaves, and gives a list of the very unsavoury substances for which they sometimes forsake their normal food—honey. Blood from the wound of an animal is one of these not generally known. The advantages taken by collectors of such aberrant tastes are sufficiently familiar; it is sad to remember that gorgeous butterflies can be lured to capture by a bait of putrid meat, and moths by a mixture of treacle and intoxicating drink.

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GEO. H. CARPENTER.

SOME NEW BOOKS.

Mr. Bateson on Variation.

Materials for the Study of Variation, Treated with Especial Regard to Discontinuity in the Origin of Species. By William Bateson, M.A., Fellow of St. John's College, Cambridge. London: Macmillan & Co., 1894. Price 21s. nett.

Those who have been following the progress of the science of morphology in recent years must have noticed a change in its attitude and methods, a change to which Natural Science has repeatedly called attention. The details of Comparative Anatomy and Embryology are being investigated with continued enthusiasm, but there is a chastened spirit in the matter of basing huge generalisations upon particular facts. In the study of the cell and nucleus, and of the physical nature of protoplasm, results so novel and so interesting are being obtained, that no doubt we over-estimate greatly their theoretical importance. But the other results of microscopy certainly we do not exaggerate. The centre of gravity of morphological investigation is changing. Many are working at what Professor Lankester aptly called Bionomics, the study of organisms as they actually live; others are making experimental enquiries in teratology, in cross-breeding, on the ovum and spermatozoon, and Mr. Bateson, Professor Weldon, and others are studying variation in new ways. All these are matters which, in the first enthusiasm for studying anatomy and embryology in the new light of Darwinism, morphologists considered questions for the curious rather than for the scientific. But as Mr. Bateson insists in his preface, the problem of species has not been solved by anatomy and embryology, and we must turn to new ways for new lights. In discussing Mr. Bateson's book, I shall endeavour rather to give an account of it than to present in detail many objections to his method of argument. For whether or no it is on Mr. Bateson's lines that the problem of species will find solution, undoubtedly his ingenious and careful work should serve to stimulate many to a side of biological inquiry too long neglected. Let me, then, endeavour to present the main lines of his argument. The salient feature of life is that the forms of living things are diverse. Those who study groups may differ as to which characters are specific, but the naturalist and the man in the street are agreed that species are different kinds of animals not to be confounded. Species are what Mr. Bateson calls "discontinuous" in the main, although occasionally linking forms and doubtful cases occur. Next, it is obvious that specific forms fit, on the whole, the places in which they have to live. The third simple fact is that in the succession from parent to offspring variation occurs. Mr. Bateson assumes the doctrine of descent, or, as he puts it, that the specific differences between species and species have come about through, and are compounded of, the individual differences between parent and offspring. He assumes it because there is a balance of evidence in its favour,

and because if it is not true we can get no further with the problem. According to Lamarck's solution of the problem of species, organisms make an appropriate structural and physiological response to the environment, and by the inheritance and accumulation of these, species arise. According to Darwin, in the struggle for existence, those of occurring variations which are in the direction of adaptation survive, the others do not. Cases of adaptation, as Lamarck understood it, are, says Mr. Bateson, significantly few, while Natural Selection is a true cause.

On the other hand, upon both theories specific diversity of form is dependent on diversity of environment, and apparently identical species live in most different environments, while apparently closely-allied but distinct species live under apparently identical conditions. In this Mr. Bateson sees an ultimate difficulty, insoluble by previously-existing facts and methods. His new method is to study variation. Mr. Bateson then dismisses the Recapitulation Theory as corroborating only the general theory of descent, and as inapplicable to the history of individual cases. He dismisses also the study of adaptation until such time as there be with it exact information about the actual amount of preservation caused by particular adaptations. He lays great stress on the familiar difficulty of accounting by Natural Selection for the incipient stages of adaptation, and finally suggests that a study of variation may reveal that the discontinuity of species arises from a discontinuity of variation. As he regards discontinuity in species as the chief difficulty, he proposes for the present to consider evidence only of discontinuity in variation. Next, Mr. Bateson proposes to consider chiefly variation with regard to symmetry. In almost every living thing pattern and symmetry are a striking feature, so much so that, if we understood pattern and symmetry, we should not be far from understanding organism. A great part of symmetry depends on repetition of parts, and for this Mr. Bateson employs the new term *merism*. Numerical and geometrical variation is of frequent occurrence. Mr. Bateson calls it meristic, and the evidence in this book deals almost entirely with it. It is of importance, first because differences in number and symmetry are frequently specific differences between organisms, and, therefore, because meristic change has occurred in evolution. It is found that meristic variation is very often discontinuous, in some cases of repetition always so. This, of course, suggests that the magnitude of the integral steps by which variation proceeds is often greater than most have supposed. *Substantive* variation, or variation in the actual quality of the elements of an organic structure, is in most cases to be distinguished carefully from *meristic* variation, although the two may occur in conjunction.

He sees in merism a general law of the organic world, and therefore dismisses all attempts to make metameric repetition an indication of community of descent—as, for instance, in seeking segmented invertebrates as the necessary ancestors of segmented vertebrates. Still more he repudiates the attempt to assign individual values to the members of a meristic series, as, for instance, the attempt to definitely compare and homologise the nerves in all the groups of vertebrates.

He draws an analogy between asexual reproduction and the production of a meristic series, and suggests that variation among the members of a meristic series may occur just as variation, although comparatively rarely, may occur among asexually-produced individuals. Similarly, he compares the relations between a paired metameric series to the relations between the first two segmentation spheres of an ovum, separation of which, as Driesch and E. B. Wilson

have shown, causes, when complete, two individuals, when incomplete, a double monster.

To make plain what he means by discontinuity in variation, Mr. Bateson gives a number of instances of discontinuity in substantive variation. In the case of 583 mature male earwigs collected at the same time, in the same locality, it was found that the length of the forceps varied from two and a half to nine millimetres. In the most common cases of variation of this kind, it is found that, when a curve is drawn of which the ordinates represent the numbers of individuals, and the abscissæ the lengths, that the curve is a simple one, corresponding to what is called a curve of error. In other words, individuals with a mean length are the most common, individuals with extreme lengths, above and below the average, are most rare. In Mr. Bateson's earwigs there were two curves of error, two common kinds of earwigs. About 120 had a length of three and a half mm., about 90 of seven mm., and the numbers tapered off to the extremes and to the mean.

Sudden variations of colour are very common, and as these are generally from one colour directly to another colour without intermediate shades, it is clear that colour-variation may be discontinuous. Other common discontinuous variations are hairless mice or rats, bulldog headed fish, and so forth. As typical examples of what he means by discontinuity in meristic variation, Mr. Bateson takes two cases. The first is where a flower—for instance a tulip—has all the parts in multiples of four, instead of in multiples of three. The change is complete, the division into four and the resulting symmetry is as perfect as the normal division into three. The second case is that of the tarsus of the leg of a blackbeetle. Usually this consists of five joints, but as a common variation the tarsus of one side may consist only of four. But in this case, as in the tulip, the new symmetry is complete. The four-jointed tarsus is of the usual length, and it is as perfectly shaped, and apparently as workmanlike a production, as the normal form. These two cases show what Mr. Bateson's extended observations clearly establish. Variations so massive as to be clearly discontinuous with the normal form, show for the most part no trace of monstrosity or irregularity, but are so clearly co-ordinated to their function, and to the general symmetry of the body, that only by actual knowledge can the variation be detected. If a number of examples of the variation were placed in the hands of an observer, who, though otherwise competent, were ignorant of the particular animals or plants, he would detect nothing unusual in the variations given him. Speaking generally, Mr. Bateson finds that what Galton called positions of organic stability occur in variations. Just as in species there is a mean form, on either side of which slight irregularities taper off, so in most variations there is a normal or mean form, round which slight irregularities of the variation cluster. This suggests to him that the discontinuity in species may be an expression of the discontinuity of variations. Of course he puts forward this view, not so much as an actual solution, as a direction in which the study of variation may profitably proceed. He suggests, in fact, that it is worth while enquiring whether the definiteness of animal and plant types be not due to the physical limitations of variation rather than to adaptation to environment. In connection with this, he draws attention to the obvious fact that as growth depends upon cell-division, many of the problems of organic structure may resolve themselves into problems of the physiology of division. Similarly, in questions of qualitative change, questions of what he calls substantive variation, many of the problems will resolve themselves ultimately

into chemical problems. Chemical compounds are themselves discontinuous, and if the chemical nature of a substance be altered at all, it will be changed either inappreciably or completely.

After this introductory matter, which alone can be treated of at any length in a short account of Mr. Bateson's book, the author passes directly to the exposition of the large number of cases of meristic variation he has examined. These are arranged under various natural headings, and are described under consecutive numbers, so that future additions and references can be made very simply. It is impossible to exaggerate the skill and care with which this has been done, and there can be no question that a most excellent introduction to the study of variation has been written.

Although it is quite certain that a just appreciation of the value of the general conclusions Mr. Bateson foreshadows can be reached only after a long study, not only of Mr. Bateson's careful descriptions, but of actual specimens, a few of the many considerations suggested by the book may be set down. The more one studies his book, the more one is struck by the always present and occasionally avowed placing in juxtaposition of the discontinuity in species and the discontinuity in variation. In more than one place Mr. Bateson suggests definitely that the former finds its explanation in the latter. Most will agree that the author over-estimates the value of his suggestion. Take first the facts upon which the so-called discontinuity in species depend. Although, in many cases, it is merely an assumption of palæontologists that those animals which can be arranged in a structural series necessarily constitute a series in descent, yet in many cases—as, for instance, in Ammonites —series of descent upon almost undoubted evidence have been shown to exist; and these series show that the evolution of species took place, not *per saltum*, but by continuous modification. We do not expect to find complete records of continuous descent; but the imperfection of the record is necessarily so great, that unless descent by simply-graded modifications were the ordinary history, it would be almost inconceivable that so close simulation of such an occurrence as has been shown to exist could exist. Moreover, the palæontological record explains so many cases of geographical distribution, that it seems quite unnecessary to make so much difficulty about the present occurrence of most nearly-allied species in the same localities. Nothing has been established more certainly than that existing distribution depends upon so many slight changes in the immediate and in the remote past, that the present occurrence of species is absolutely no guide to their original habitats. Temporary geographical isolations of parts of species repeatedly occur, and as repeatedly disappear. We have much evidence that goes to show that the great land areas of the world have remained comparatively constant in position, but that there have been repeated changes from continents to archipelagoes and back again to continents. We have direct evidence as to the effect of geographical separation in producing divergence. Putting the two together, we have good reason to suppose that groups that have become divergent when separated, have been thrown together again. Anyone who is familiar with the physical changes of a country side in the very few years that cover the observations of one individual, must know of changes in the distribution of the most common animals and plants. This knowledge may not be, and probably is not, enough to solve the problem of the co-existence of different forms closely allied; but it is enough to give pause before finding in the co-existence of such species a problem that can be solved only by the assumption of a discontinuous origin.

But in drawing his contrast between the continuity of the environment and the discontinuity of species, Mr. Bateson attaches a curiously limited meaning to the term environment. It is quite true that in the same spring weather, in the same fields, and on the same flowers, lady-birds differing in striking but apparently unimportant details may be found, and side by side under the same stones dimorphic earwigs occur. No doubt such instances could be multiplied almost indefinitely wherever observers so careful as Mr. Bateson are at work. But can we deduce from such facts as these the necessity of abandoning continuity in the modification of species? If there is a struggle and a selection of the more favoured individuals, there are a thousand elusive factors in the environment that may be determining the existence of the animals. A slight difference in the power of resisting bacteria, a change in physiological habit that would write no mark upon the visible form of the creatures, and many another factor overlooked by us, might well be the determining cause of the animal's existence. These, no doubt, are all hypotheses, but it is not a hypothesis to say that we are too ignorant of the relations of any animal or plant to its environment to conclude that discontinuous forms of life arise or exist in a continuous environment, and therefore to seek explanation in discontinuous factors. When, moreover, one remembers that each animal or plant is an organic whole, and that selection of an invisible character must bring with it selection of many visible characters, it is plain enough that, although the characters which arrest our attention bear no intelligible relation to what we know of the environment, we need not abandon old methods and known causes to seek new and unknown causes.

Turning now to discontinuity in variation, it is to be noted that Mr. Bateson himself explains again and again that what he calls discontinuity is merely a necessary result of growth, or of chemical constitution. An embryonic cell divides, or it does not divide, and there results in the adult two organs or one organ, but not one and a half. A pigment is chemically altered, or it is not chemically altered, and there results a new colour, or the old colour, but not a series of intermediate shade. Such instances, however valuable as part of existing nature, do not, however, so much as touch the every-day instances of slight gradation and undoubtedly continuous variation that are to be observed in the shape and size of various parts. Moreover, as Mr. Bateson himself is careful to note, no adequate evidence is yet forthcoming with respect to the inheritance of discontinuous variations. In short, what Mr. Bateson has really done in his most interesting book is to collect and formulate a large amount of information about the laws of growth. He has shown clearly what naturalists are accustomed to neglect. The body of any animal or plant forms an organic whole. The various organs and structures are correlated together just as physiologists have shown that the various functions of the body are correlated together. In the case of physiological facts, there is as yet little guidance for us in attempting to understand how the correlations are affected. Mr. Bateson has shown how, in many cases, the structures that build up the symmetrical organism are correlated; he has brought out clearly the most striking fact that variations are not irregularities, but that they are as regular, and as harmonious, as the ordinary conditions of organisation. Indirectly, this will no doubt serve to explain the apparent discontinuity in species by showing that, as Mr. Bateson says, the integral stages by which

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variation proceeds are larger than we supposed. But it will also serve to make the apparent discontinuity less striking by directing our attention to the immense importance of the laws of growth in normal structures, as well as in abnormal ones. I do not think that his book so much suggests, as he supposes, that the discontinuity of species may be a consequence and an expression of the discontinuity of variation, but that the apparent discontinuity found both in variation and in species will seem less and less as we understand more the physical limitations by which the growth of organisms are determined.

P. C. M.

AN ENGLISH BOTANY TEXT-BOOK.

A STUDENTS' TEXT-BOOK OF BOTANY. By S. H. Vines, M.A., D.Sc., F.R.S. (First half.) 8vo. Pp. x., 430, with 279 illustrations. London: Swan Sonnenschein & Co., 1894. Price 7s. 6d.

WE have waited a long time for Dr. Vines' text-book, and, while welcoming the first portion, fervently echo the hope expressed by the publishers on the back of the cover, to wit, "that the remainder of the work, together with the Index, etc., will be ready for issue in the course of the year." The book has grown out of the author's English edition of Prantl's *Lehrbuch*, but growth has implied a change in form as well as increase in bulk, and to such an extent that "the present is essentially a new and distinct work," and, consequently, bears only Dr. Vines' name on its title-page. The older work, issued in 1880, contained 344 pages and 275 woodcuts; the first half of the new one contains nearly 100 pages more and 279 cuts; moreover, the scope of the book has been so extended that, while retaining all that has made it of value to beginners, it may also be useful to those engaged in the advanced study of the science.

A text-book is generally regarded as nourishment for the student who is feeding up for examination, or has a worthy desire to get a good general knowledge of botany before specialising in some one of its many branches. But it should have a wider use, as a tonic for specialists, and a corrective for the evils arising from work in a too confined space. Workers in different fields would be more in touch with each other if their knowledge of the general principles in the great divisions of morphology, physiology, and classification was kept fairly up-to-date; besides which, they would take a broader view of things, and make fewer blunders, while the value of their work would be enhanced accordingly. This puts a heavy responsibility on the shoulders of the text-book writer, and exposes him to universal criticism, but might also have the salutary effect of suppressing, if he is suppressible, the ever-recurring individual who, having realised a long-felt want, hastens to supply it with a compilation badly planned and feebly executed.

Dr. Vines' text-book is the first that modern botany has produced in this country. We have text-books, and good ones, like Sachs and Goebel, but they have been only translations of German works, native talent exhausting itself in the addition of a few foot-notes and a glossary at the end. It is matter for congratulation that originality in this line has not been utterly quenched. No botanist is so well fitted for the task as Dr. Vines, and the portion of the text-book already produced is admirable. It includes Part I., on Morphology; Part II., on the Intimate Structure of Plants (Anatomy and Histology); and that portion of Part III., the Classification of Plants, comprised under the old term Cryptogams including the

groups Thallophyta, Bryophyta, and Pteridophyta. We notice at once a departure from the plan of the edition of Prantl, where the chapters on physiology followed, we think more naturally, those dealing with anatomy and histology.

Speaking of physiology we are reminded of the author's text-book on the subject, with which in one respect his more recent effort compares unfavourably. We mean in mode of expression. The "Physiology" was pleasant and comparatively easy reading, but the students' text-book is stiff, and bristles with technical terms, many of which will be new and strange even to the student who left off text-books less than seven years ago. This is most noticeable in the anatomical part. Thus, the plant unit is an *energid*, which becomes a cell only when invested with a cell-wall. An aggregate of energids, *e.g.*, the body of a *Vaucheria*, is a *coenocyte*, and *coenocytic* structure is thus contrasted with cellular; but we have also to distinguish the multinucleate cell, which from being uninucleate has become multinucleate by direct nuclear division, from the *syncyte*, a structure developed from already-formed cells by a more or less complete absorption of the separating walls, such as occurs in the development of vessels. The employment of the term *stele* for the tissues derived from the plerome entails a number of derivatives such as *polystely*, *schizostely*, *gamostelic*, *meristele*, etc., which will make the botanist to whom we have referred feel quite ancient.

We like the part on morphology best, and note especially the able way in which the more highly and more lowly organised plants are considered from one broad point of view. The extension of *root* and *fruit* into cellular plants, and a more general use of some other terms helps to break down the exclusive barrier which has too severely separated the larger groups. In treating of the leaf, the author adopts Bower's view of differentiation along a longitudinal axis, or *phyllopodium*, into *hypo-*, *meso-*, and *epi*-podium, the first forming the leaf-base, including the sheath, stipules, and pulvinus, the second the stalk, while the third is generally winged, forming the blade. When we consider the extremely close relation between the parts, and the frequent impossibility of accurate distinction, as well as the continuity of structure, these divisions seem somewhat arbitrary. A very admirable chapter is that on the cell, which forms a splendid introduction to the anatomical section. We notice, however, a slip on p. 112, where it is stated that "the proteid grain always contains a mass of mineral matter," a statement disproved by some work done in Dr. Vines' own laboratory at Cambridge.

The most noticeable features in the portion dealing with classification are the separation of *Isoëtes* from Salviniaceæ as a distinct order of Heterosporous Eusporangiate Filicinæ, and the well illustrated account of the embryogeny of the lycopods.

We sincerely hope that the "Students' Text-Book," by the production of which Dr. Vines has increased the obligation the botanical world already owes him, will not long remain incomplete.

British Glaciation.

Papers and Notes on the Glacial Geology of Great Britain and Ireland. By the late Henry Carvill Lewis, M.A., F.G.S. Edited from his unpublished MSS., with an introduction, by Henry W. Crosskey. Pp. lxxxi. and 469. 82 figures and 10 maps. London: Longmans, 1894. Price 21s.

Among all the vexed questions in British geology none has been the subject of more divergent theories, faddy speculations, and prolific

literature of the type which Haeckel would call "phaulographic," than the interpretation of the series of sands and clays which are generally regarded as due to the action of some form of ice. The chaos has resulted from various reasons, among the chief of which are the facts that the sections are usually open temporarily so that only local workers can follow them, that comparatively few have the opportunity of studying ice in action at the present day, and that the subject has suffered from an extra crop of faddists and theorists. There is probably no branch of stratigraphical geology which requires that its students should have examined the deposits over such wide stretches of country, for they vary so greatly within very narrow limits. It was therefore highly desirable that some well-trained geologist should go over the whole of the ground, in order to collect data for an authoritative expression of opinion; English students were accordingly glad to welcome Professor H. Carvill Lewis when he entered upon this task. He brought to it very special qualifications; he was a man of untiring energy and remarkable keenness of observation; he was a petrographer as well as a physical geologist, so that he was able to recognise the erratics he had to study; he held, moreover, the great advantage of a training in the glaciation of America, where he had been able to examine enormous extents of glacial deposits in a region where the agency of floating marine ice could not be invoked. It was objected that he was a theorist, but this, after all, only meant that he was a man of ideas, and had the imagination necessary to enable him to interpret the descriptions of others, and allow for the misrepresentations due to inexperience, carelessness, and fads. The readiness which he always showed to abandon his old positions as soon as facts rendered it untenable testified to his fairness and showed that he was not a man of blind prejudices. Hence, when the news spread that he had fallen a victim to the fell disease of typhoid fever at Manchester, just after he had landed for a fourth campaign in July, 1888, it was universally admitted that glacial geology had suffered an irreparable loss. And this was felt the more keenly by all who had come within the range of his personal influence, and knew the manly frankness, the contagious enthusiasm and energy, and the manifest sincerity which gave his character a singular fascination.

How much we have lost by the death of Professor Lewis, this volume helps us to realise; for it shows us what an amount of material he had collected, which now has to be published in a somewhat scrappy form. The work, however, is much better than some had expected, and for this we have to thank the editor. Much anxiety was felt when it was first announced that Professor Lewis's manuscripts had been left to Dr. Crosskey for him to edit, as this seemed like asking the Pope to edit Luther's tracts; for Dr. Crosskey had generally been regarded as one of the most conservative of the older school of glacial geologists and one of those most opposed to the views of Professor Lewis. Dr. Crosskey, however, did his task in a manner that deserves the highest admiration. He refused to "criticise," and confined his labours to those of arrangement, elucidation, and editing. This he has done, not only with a wide knowledge of the subject and a sound literary judgment, but with feelings of affectionate sympathy with his heretical young friend. He has contributed an admirable introduction of 49 pages, giving a sketch of the general results of Lewis's work, which shows how deep an influence the latter's views had had in England in a quarter where it was least expected.

When Professor Lewis came first to England the generally-accepted theory was that this country had twice been covered by a vast ice-sheet, and in the interval between these had been submerged to the depth of over 1,400 feet at the very least. One school occupied the extreme position of denying the influence of land-ice in any form, and explained the whole thing as due to icebergs drifting southward during a period of submergence; this view, however, was very restricted in its distribution, and there was an overwhelming majority for the orthodox side. A few men had advocated other views, notably J. G. Goodchild and T. Belt; they believed in one ice-age, denied any submergence, except locally to a very small amount, maintained the power of ice-sheets to raise boulders and frozen masses of shell-beds from lower to higher levels, and realised the effects of the differential movements that must occur in ice-sheets flowing over uneven surfaces. But these teachers had exerted barely any perceptible influence; their views was based on the complexity of the conditions of glaciation, whereas geologists preferred to regard these as very simple. Goodchild's remarkable paper on the "Glacial Geology of Edenside" appeared in 1875, but it was before its time and was practically ignored.

In opposition to the orthodox theory, Professor Lewis advocated the unity of the ice-age, at least until his discovery at Frankley Hill, and strenuously opposed the great submergence. He admitted that the north of England and Scotland had subsided for about 150 feet, and attributed all the deposits formed by water above this level to construction of either lakes or subglacial streams. The shells, he thought, *fruit*ked their way up through the ice in frozen masses, just as boulders he[illegible] through the Swiss glaciers. These views are expressed in the [illegible]volume in five short papers. These are entitled, "Comparative [illegible]studies upon the Glaciation of North America, Great Britain, and [illegible]reland"; "The Terminal Moraines of the Great Glaciers of [illegible]ngland"; "On some Important Extra-Morainic Lakes in Central [illegible]England, North America, and elsewhere, during the period of Maximum Glaciation, and on the Origin of Extra-Morainic Boulder Clay"; [illegible] "The Supposed Three-fold Division of the Drift"; and "The Direction of Glaciation as ascertained by the form of the Striæ." These essays, however, he left only in abstract. Most of the volume consists of his field notes, and contains descriptions of sections and country in many parts of Ireland, England, and Wales. The districts that he knew best, and where he has added most to our knowledge, are the south-west of Ireland and northern Donegal, Shropshire, and South Lancashire; his description of the sections in these localities must prove of great service to English geologists.

One of the principal difficulties in using this part of the work is the varying meaning attached to the same term, and the frequent changes of position which were necessitated by increasing knowledge, and no one can read the volume without feeling what we have lost by the fact that Professor Lewis did not live to state his case finally and fully in the light of his latest conclusions. Many of the views expressed we know he modified the year before his death, and he had come to England to re-investigate the changes that were inevitable after his abandonment of his view of there being but a single glaciation.

But in spite of his early death he had lived to do much. He had unquestionably cleared the ground of many stumbling blocks.

He showed, for example—and Kendall's recent study of the sections exposed by the Manchester Ship Canal support it—that the old view of the three-fold classification of the drift into an upper and a lower boulder clay, separated by sands, is absolutely untenable; that even within very short distances this supposed sequence is often reversed, and we may get the boulder clay in the middle, with sands above and below. A second point of great importance was his insistence on the influence of water in depositing and arranging the glacial materials: it may act in the form of subglacial streams or lakes, which are either upon or in front of the glacier. Probably, however, Professor Lewis's most important contribution to British geology has been the practical annihilation of the theory of the submergence of England to the depth of 1,400 feet in an interglacial period. This theory he once characterised as "the most pernicious one ever propounded in geology" (p. 375), and against it he never ceased to wage war. That probably not one of the younger English geologists now believes this theory is, practically, entirely owing to the work of Professor Lewis. To have laid this bogey was itself a most brilliant achievement.

There are a few points, however, in which Professor Lewis seems to have overstated his case. Thus his law that "every glacier is bounded by a terminal moraine," is not likely to be generally accepted; it was founded at the time when it was confidently expected that the moraines of the Eastern States of America would be found to extend right across the Central Plain and unite with the old moraines of the Rockies. But now that it has been proved that this is not the case, and that the extra-morainic fringe is more extensive that was once thought, this view cannot be accepted as Professor Lewis stated it. A still more important point was his former denial of the existence of more than one glaciation, and his refusal to admit the action of land-ice for the Essex Boulder Clay, which was a natural corollary from the former. His examination, however, of the Frankley Hill sections convinced him that there have been two glaciations, and that his "terminal moraines" were all formed in the later. It is, therefore, highly probable that if he had examined some of the Essex sections after this, he would have accepted the Boulder Clay there as a true till belonging to the earlier glaciation; he would not then have attributed it to an extra-morainic lake, though the glacial gravels of this district are probably formed by some such agency. It is impossible to regard the scattered denuded remnants of the South Essex Boulder Clay as synchronous with the comparatively unweathered moraines of the Lake District and Wales. The acceptance of a double glaciation removes the most serious of the objections to Professor Lewis's hypothesis.

The volume is well printed, and has been most carefully edited. The explanations and notes of Dr. Crosskey and Mrs. Lewis, and a valuable appendix by Mr. Kendal, strengthen and remove doubts from Professor Lewis's position. The maps and sketches are clear and instructive, though some improvements might easily have been made in some of the former that would have made them more in accordance both with the facts and Professor Lewis's theories. Had a few such alterations been made, they would have been of more service. The only two serious regrets we feel are, that there is no index—the list of contents is, however, a full one—and that the volume has not been adorned with a portrait of its author.

J. W. G.

Science and Sport.

Big Game Shooting (Badminton Library). By Clive Phillipps-Wolley. With Contributions by Sir Samuel W. Baker, W. C. Oswell, F. J. Jackson, Warburton Pike, F. C. Selous, Lieut.-Colonel R. Heber Percy, Arnold Pike, Major Algernon C. Heber Percy, W. A. Baillie-Grohman, Sir Henry Pottinger, Bart., Earl of Kilmorey, Abel Chapman, Walter J. Buck, and St. George Littledale. 8vo. 2 vols., illustrated. London: Longmans & Co., 1893. Price 10/6 each vol.

That a book on Big Game Shooting, written professedly for the sportsman, should contain such a large amount of natural history, for the most part excellently written and well up-to-date, as the one before us, is a sign that sportsmen are taking a genuine interest in zoology, and that their sole aim is no longer the making of the biggest bag, or the securing the finest trophies. How heartily we welcome this change from the old state of affairs it is unnecessary to mention, but we hope the appearance of this most excellent work will aid in stimulating all the slayers of Big Game to use their unrivalled opportunities of adding to our knowledge of the habits and distribution of the animals they pursue. If we might add an additional plea, it would be that they would incur a further obligation on the part of the working naturalist if they would occasionally give us some notes on the life-histories of other little-known creatures, not coming under the title of Big Game, which they must frequently come across in the course of their wanderings. Such observations take but little time to record, and will often serve to fill up an idle hour in camp.

The large field covered by the work before us will be seen from the titles of the chapters, which are as follows:—Big Game Shooting generally, by C. P. Wolley; South Africa Fifty Years Ago, Second Expedition to South Africa, Later Visits to South Africa, and With Livingstone in South Africa, all by W. C. Oswell; East Africa—Battery, Dress, Camp-Gear, and Stores, Game Districts and Routes, The Caravan, the Headman, Gun-bearers, etc., Hints on East Africa, Stalking, Driving, etc., the Elephant, the African Buffalo, the Lion, the Rhinoceros, the Hippopotamus, Ostriches and Giraffes, Antelopes, all by F. J. Jackson; the Lion in South Africa, by F. C. Selous; the Big Game of North America, the Caucasus, the Mountain Game of the Caucasus, all by C. P. Wolley; the Musk-Ox, by W. Pike; Arctic Hunting, by Arnold Pike; the Caucasian Aurochs, the *Ovis argali* of Mongolia, and the *Ovis poli* of the Pamir, by St. G. Littledale; the Chamois, and the Stag of the Alps, by W. A. Baillie-Grohman; the Scandinavian Elk, by Sir Henry Pottinger; European Big Game, by Major A. H. Percy and the Earl of Kilmorey; the Large Game of Spain and Portugal, by Abel Chapman and W. J. Buck; and Indian Shooting, by Lieut.-Col. R. H. Percy.

Among these chapters, especial interest attaches to the one by the late Mr. Oswell, on the condition of Africa, and the swarms of its big game half-a-century ago; although we can but recall with regret that such an excellent observer should have been one of those who assisted to no inconsiderable extent in the extermination of the so-called white rhinoceros. And here we take the opportunity of calling attention to the unrivalled excellence of the illustrations by Mr. Charles Whymper, with which the volume is embellished, some of these giving a better idea of what South Africa used to be in its palmy days as a big game country, than any others that have come under our notice. What more particularly strikes us is the fidelity to nature with

which such small-sized photographs have been executed, the species of every animal portrayed being recognisable at the first glance.

Among the other chapters it is almost invidious to make a selection; but Mr. Jackson's chapter on the antelopes of East Africa, from a naturalist's point of view, strikes us as particularly good; while the one on the big game of India, by Colonel Percy, must certainly come in for a share of commendation. In a work like the present it is not likely that a naturalist would find much that is absolutely new to him; but there are many points that are worthy of his attention. In the first place, we believe it is not generally known that leopards inhabit the Caucasus, but if we are to credit the evidence recorded on page 43 of the second volume, it appears that we must admit this to be the case. Another point of interest is the opinion that the African elephant was never tamed by the Carthaginians, the coins with the effigy of this animal upon them having been struck long after Carthaginian times, and the animal even on these being a kind of mongrel. More probably the elephants which the Carthaginians undoubtedly possessed were imported Indian animals. The maximum height of the African elephant recorded in this work is 12 feet 2 inches, and the extraordinary numbers in which these giants formerly occurred in South Africa is graphically described on page 129 of the first volume. The subject of height reminds us that a maximum of 18 feet is assigned in this work to the giraffes although Mr. Bryden has assured us by letter that he has measured, a specimen of 19 feet. We confess that we should like further information on this subject; more especially as the apparently gigantic stuffed male from South Africa in the British Museum measures only 17 feet 4 inches.

In another work Mr. Selous has rather thrown cold water on the idea that the African buffalo is a ferocious animal, but in the volume before us Mr. Jackson fully supports the view that the creature is decidedly an ugly customer to encounter. That the African rhinoceroses attack, as might have been supposed from the absence of front teeth, with their tremendous horns, is fully demonstrated; but it is perhaps less generally known that the Asiatic species use the lower tusks as their weapons of offence, in the same manner as a pig. The recorded occurrence of the Sitatunga antelope from Uganda is, if we mistake not, a new fact in distribution.

The foregoing references are sufficient to indicate the highly interesting nature of this most excellent treatise on Big Game, which we can heartily commend, not only to the attention of the sportsmen for whom it was primarily intended, but likewise to everyone interested in the habits and mode of life of these largest efforts of creation.

If we were in the mood for fault-finding, which we are not, we should discover but little to blame in the work as a whole. May we, however, suggest to the editor that in the next edition he should exercise his editorial powers a little more energetically, in order to prevent discrepancies. For instance, we find on page 76 of the second volume Mr. St. George Littledale lamenting that naturalists have not decided on the synonymy and number of species of the big Asiatic wild sheep; while on page 291 of the same volume Colonel Percy quotes at length Mr. Blanford's well-known and decisive observations on this interesting subject. Again, it would be well if the editor were to take into his council some zoologist of repute who would aid him in the thorny subject of nomenclature; and if this were

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These are, however, but trifling blemishes in a most excellent work, which will long be the standard on the subject of which it treats, and reflects credit alike on its editor and its contributors, as well as on the artists and publishers.

The Chemistry of Animals.

The Essentials of Chemical Physiology for the use of Students. By W. D. Halliburton, M.D., F.R.S., F.R.C.P., Professor of Physiology in King's College, London, etc., etc. Pp. 166. London: Longmans, Green & Co., 1893. Price 5s.

In the records of research and in advanced treatises the most certain and the newest discoveries are so held in solution that only an expert can detect them. But when a brilliant investigator and competent teacher like Dr. Halliburton writes a text-book for students, we expect to find that his mind has acted on the confused details of his science so as to precipitate and render obvious the most important truths. It requires small effort to see that Dr. Halliburton regards it as of the first importance to impress on students that in chemical physiology they are concerned with living organisms. Natural the[illegible]ence from time to time has called attention to the growth of what wo[illegible]alled "Vitalism" in physiology, and here in that citadel of the d[illegible]echanical school—the chemistry of the animal body—one finds the new doctrine entrenched. Consider what one was taught even a few years ago. The body was a collection of specialised organs, each organ with its own function or functions. The blood was a mechanical channel of connection between the organs. The food was taken into the alimentary canal and the various digestive juices acted upon it so that insoluble substances were turned into soluble. These latter passed by osmosis into the blood directly, or through the lacteals. By the blood they were handed on to the different organs, in each of which certain chemical processes went on. In the lungs oxygen diffused in, carbonic acid diffused out, and the reverse process took place in the tissues. Wherever a direct mechanical or chemical comparison could be made it was adopted as an explanation. Elaborate tables abounded, showing the transformation of so much food material into so many grammes of fat and glycogen and proteid; calculations were made showing the work to be expected from the oxidation of so much proteid and carbohydrate and the corresponding production of waste products.

Here all these matters are in the background, being replaced by the microscopic appearances of the cells and tissues; and the idea that all the processes are vital processes occurring in living protoplasm is insisted on. In digestion, no doubt, insoluble starches are turned into soluble sugars, insoluble proteids into soluble peptones; fats are emulsified and saponified. But these do not diffuse into the blood; they are "eaten" by the living cells of the alimentary canal, and these, neglecting diffusibility or indiffusibility, select what they want not even systematically but with varying caprices. Indeed if soluble peptone be injected into the blood, into which it used to be thought to soak, poisonous effects

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are produced and the animal may die. The epithelial cells of the alimentary canal protect us from these poisonous effects by turning back into insoluble proteids the peptones they absorb. Again, sugar injected into the blood passes into the lymph, but soon the percentage in the lymph is higher than the percentage in the blood—a result which cannot be explained by filtration or diffusion, and must be due to the "secretory activity" of the cells of the vessel walls. Even the vital action of the cells of the intestine does not alone perform the work of digestion. The process is aided by multitudes of bacteria that thrive where the antiseptic gastric juice has deen neutralised by the fluid from the pancreas.

Another striking "vitalistic" occurrence is the excretion of carbonic acid by the lungs. The tension of carbonic acid in the air in the alveoli of the lungs is much lower than the tension of carbonic acid in the veins. Hence it seems a simple physical process that carbonic acid should pass from the veins to the alveolar air. But the process goes on so far that when the venous blood has become arterial, the tension of the carbonic acid is lower in the blood than in the alveolar air. Here again secretory activity of the epithelium lining the pulmonary alveoli is called into account.

Some of the other changes from the physiological chemistry of our youth are equally striking. Thus, a large number of the so-called chemical processes depend on unorganised ferments; that is to say, on ferments possessing the characters of living things, except that they cannot be identified as separate organisms having shape, size, and so forth. Of these, some we know, others are more recent discoveries. Ptyalin of the saliva, pepsin and rennet of the stomach, trypsin, amylopsin, and steapsin of the pancreas, and the fibrin ferment in the white blood corpuscles, all are appeals from chemistry to vital action.

The organs and tissues of the body are bound together in much more than a mechanical fashion. Experimental evidence has shown that, in addition to its own special functions, each organ contributes in a vague way to the "general metabolism of the body." The expression of the fact is vague, but the fact is real and striking. Extirpation of the testes causes many well-known general changes in addition to the special change of impotency. And so extirpation of other organs brings about many general changes over the whole body in addition to loss of the special.

The special functions of organs are not the simple occurrences formerly believed in. For instance, urea is not formed in the kidneys, not even in the muscles, but in the liver; while, on the other hand, bile is not a digestive fluid but probably purely excretory.

We advise strongly those who are working in other branches of Biology to read Dr. Halliburton's clear and interesting little book. Many problems await their solution by the joint action of morphological and physiological considerations, and we recommend morphologists in particular to notice the paths along which the students of function are travelling.

It remains to be said that from the point of view of students Dr. Halliburton's book is admirable. The directions for practical work at the beginning of each section are explicit and simple; even dull students should require little further direction. The descriptions of apparatus are very clear, and if they are studied not directly from the figures in the book but with the apparatus in the laboratory, much time will be saved from the teaching of physics for the teaching of physiology.

THE FAUNA OF THE DEEP SEA. By S. J. Hickson, M.A., D.Sc. Modern Science Series. Pp. xvi., 169. London: Kegan Paul, Trench, Trubner & Co., 1894. Price 2s. 6d.

IN this interesting little book, which we are prevented by its title from calling superficial, Dr. Hickson deals in a readable and commendably brief way with recent enquiries into the nature and habits of animals living at great depths. The subject is alluring to everybody, and the "general reader" will, we imagine, not be disappointed with what Dr. Hickson has to offer him. The more seriously minded and expert reader will not of course expect a detailed discussion of the problems arising out of the facts. Nor would they indeed be entirely in place in a short epitome of this kind. Dr. Hickson begins his preface by saying, quite melodramatically, that "a time may come" when the bottom of the sea will be thoroughly known. At present it is as he justly, though hibernically, intimates a large *terra incognita*. Still a fair amount is known, and mainly, as everybody admits, through the results of the "Challenger" voyage. The uninstructed devourer of Mr. Hickson's pages will be struck with the satanic aspect of many of the deep-sea fishes. The piscine countenance is not formed for the presentation of beauty, but it is rarely that such malevolence is to be read there as in the case of these particular fishes. Moreover, the reader will also learn that the features are an index to the dispositions of their possessors. So ferociously carnivorous are some of these fish that they will without the least hesitation swallow a neighbour ever so much larger than themselves. Kindly nature, who in this quarter of the world is particularly "red in tooth and claw," has aided and abetted them in this course of action by endowing them with unusually extensile stomachs. Their voracity, however, is not always unpunished; for many other animals, which would be liable to fall victims to these cannibals, have put on an extensive armature of spines; anyone who consults the sumptuous plates of the "Challenger Monographs" will soon meet with figures of crustacea which are only to be compared with hedgehogs. Luckily there is a limit to this indiscriminate feeding off one another in the deep sea, or its fauna would by this time have arrived at the condition of the crew of the "Nancy Bell." Some years ago Dr. Moebius wrote a paper entitled "Wo kommt dann die Nährung der Tiefseefauna aus?" in which it was sought to prove that the animals of the abysses chiefly depended upon the surface waters for their food-supply. From them there falls a constant and abundant rain of the dead bodies of minute surface-creatures such as Diatoms and Foraminifera, which are snapped up by the animals at the bottom. In reviewing the facts concerning the deep-sea fauna with an eye to generalisations, one is struck by the contradictory nature of the evidence. The environment seems to have acted differently in various cases. For instance, the gills of the deep-sea fishes are shrunken and comparatively rudimentary. On the other hand, the very same cause has produced in other animals the reverse condition. Milne Edwards remarked upon the enormous development of accessory gills in his gigantic Isopod *Bathynomus*; and another Isopod belonging to the same family, *Anuropus*, has the last pair of abdominal appendages converted into gills. In some groups of animals the deep-sea representatives are shrunken and dwindled in size as compared with their shallow-water allies; in other groups the very largest members known hail from the deep-sea; *Bathynomus* is an instance of the latter statement. There is thus a good deal to do in the way of collating the abundant facts at

our disposal, which will furnish in all probability good material for deciding on the merits of Natural Selection as opposed to environmental influence.

AN INDEX TO THE GENERA AND SPECIES OF THE FORAMINIFERA. By Charles Davies Sherborn. Part I. (A to Non). From Smithsonian Miscellaneous Collections, vol. xxxvii. (No. 856). 8vo. Pp. ii., 240. Washington: Smithsonian Institution. 1893.

THE Smithsonian Institution has often deserved well of the working naturalist by the publication of works troublesome to prepare and costly to print, appealing to no wide public, but invaluable to the few that are able to appreciate them. In no case will gratitude be more profound or more deserved than that which will come to the Institution in return for the issue of this Index. The labour involved in the production of such a work is sufficiently clear to anyone who casts his eye down one of its closely-printed pages, and is also evidenced by the fact that this, the first half, has taken nearly as long to print as the whole index took to prepare. The work is practically a systematic index to all the literature published on Foraminifera up to the end of 1889, and refers not merely to every published name, but to many names not properly published, to many published instances either not named or not referred to Foraminifera at all, and to other instances that have been erroneously placed in this group. The author does not profess to determine the synonymy of the different species, since this would introduce the element of personal opinion, which, even in the case of so approved an authority, might occasionally be erroneous. Almost the same end is, however, attained by the citation of previous opinions, and by the extraordinarily complete system of cross-references. The value of a work like the present is almost entirely dependent on the two qualities of accuracy and completeness. Those conversant with Mr. Sherborn's "Bibliography of the Foraminifera," a book of constant service to the librarian, the bibliographer, and the scientific writer, know that no one has a fuller acquaintance with the literature of his subject; while Woodward and Sherborn's "Catalogue of British Fossil Vertebrata" is sufficient testimony to the accuracy of the compiler.

We cannot dilate on this monument of enthusiastic industry, since it appeals to the specialist rather than to the general reader. But we would urge all who contemplate similar publications to take this as their model; and we hope that this example will lead many to imitate it for other classes of animals. Were all groups indexed in this manner, the labours of zoologists would be lightened of a grievous burden, and the wail of the synonym need no longer be heard in the land.

OTHER NEW PUBLICATIONS ON THE FORAMINIFERA.

THE samples of sea-bottom obtained by the German exploring ship "Gazelle" have been worked out by Dr. J. G. Egger, who has paid especial attention to the Foraminifera. Dr. Egger's monograph has recently been published in the Abhandlungen of the Bavarian Academy (vol. xviii.), and consists of 266 pages, illustrated by 21 quarto zincographs. These zincographs are sketchy, but serve their purpose admirably, because most of the forms have been described before. They are crowded with figures, and therefore present, for perhaps the first time, a valuable grouping of many allied forms in

one picture. This is especially well seen in pl. x., devoted to the Lagenæ, and pl. xiii., devoted to the Globigerinæ, and helps the student to realise how intimately related all the so-called "species" are, and how difficult it is to attempt to draw specific distinctions when whole series of these lowly organisms are obtained. Fornasini has just published a paper on the Costa and Seguenza collections in the Naples Museum (*Mem. R. Accad. Sci. Ist. Bologna*, ser. 5, vol. iv.), which were collected from the Tertiary marls of Messina. His paper deals chiefly with the Nodosariæ and Cristellariæ, and the three beautiful plates of outline drawings leave nothing to be desired. Dervieux has also published in the *Boll. Soc. Geol. Ital.*, vol. xii., part 4, a paper on the Tertiary Nodosariæ of Piedmont, in which 64 forms are carefully described, and illustrated in one plate. There should be no difficulty now in identifying the numerous Tertiary forms of the genera, but students cannot be too strongly cautioned against describing supposed new varieties without the most patient study of existing literature, a study which, we think, is often neglected because of the trouble and time required.

The Study of Molluscs.

Introduction à l'Étude des Mollusques. Par Paul Pelseneer. 8vo. Pp. 216, figures. Reprinted [in advance] from the "Mémoires de la Société Royale Malacologique de Belgique, tom xxvii." (1892). Bruxelles: H. Lamertin, 1894. Price 6 francs.

Most works on elementary zoology are, as the author points out in his preface, behind the time, because it is not in the power of any single man to keep simultaneously abreast with the progress made in all the various branches of zoological science. Pending the time that such a work shall be produced, as the more advanced treatises often are, by the co-operation of specialists in the several groups, the student can frequently find much that he needs to know in monographs such as Graber's "Die Insekten," or Günther's "Introduction to the Study of Fishes," but no such work exists on the Mollusca, if we except Coupin's that appeared in 1892. This last is, however, more of a "cram book" than anything else, and was very much behind the time even when it was issued, so that it is hardly to be seriously reckoned with. Dr. Pelseneer's "Introduction," on the other hand, like all he attempts, is thorough, and though there are points on which we venture to differ from him, which are open to objection or criticism, there can be no doubt of its great value to all malacologists as a reference book, and as such it is cordially to be recommended. Indeed, we only wish that the systematic conchologists (the variety-mongers and mere collectors of shells may be left out of account as past praying for) could be induced to master some of the principles here so clearly set forth, since it could not fail to have a beneficial effect in widening their mental vision, and so rendering their labours more useful by imparting, to them at least, a tinge of true science.

This Introduction is framed, of course, on a zoological basis, and in some respects too exclusively so. The method of treatment is to give a succinct account, first of the Mollusca as a whole, and then of each class, or sub-class when that seems desirable, the sub-headings being:—(1) Morphology; (2) "Éthologie," which the author defines as the principal particulars concerning the conditions of existence, in other words, the habits and mode of life, for which "Bionomics" is a more appropriate term; (3) Bibliography; and (4) Systematic

OBITUARY.

WILLIAM PENGELLY.

Born January 12, 1813. Died March 16, 1894.

ALTHOUGH Mr. Pengelly was for some sixty years a resident in Torquay, he was not a Devonshire man, his birthplace having been in the quiet little fishing town of East Looe, in Cornwall. It is not surprising that, with the surroundings of such a place, he should have acquired, as a lad, a strong attachment to the sea. Fortunately, however, the bent of his mind towards scientific studies led him to adopt the profession of a teacher, and while still a young man he established himself at Torquay. Here his enthusiasm for the acquisition of knowledge soon manifested itself in the activity which he displayed in developing the Mechanics' Institute; and a few years afterwards he suggested the formation of a local society for the study of Natural Science. It was, indeed, mainly due to Mr. Pengelly's energy that the autumn of 1844 witnessed the foundation of the Torquay Natural History Society. Over its early fortunes he exercised the most watchful care, and in 1851 he was induced to accept the office of Honorary Secretary, an office which he continued uninterruptedly to hold, to the unspeakable advantage of the Society, for not less than nine-and-thirty years. Under his guidance it became a scientific power in the county; year after year he lectured there, tincturing the locality with his own enthusiasm, and from the Society there ultimately sprang the Museum in Babbacombe Road, with its admirable local collections. In the reading-room attached to the Museum there fitly hangs an oil-painting of the man whose individuality is unmistakably stamped upon the entire institution—William Pengelly.

Everyone identifies Mr. Pengelly with the exploration of Kent's Cavern. To this cave he had been attracted soon after his settlement in Torquay; and as far back as 1846 he had assisted in its partial examination, as a member of a small and poorly-equipped Committee, appointed by the local Natural History Society. The work of this Committee tended to confirm the conclusions formed by MacEnery and afterwards by Godwin-Austen; but the age was far from being ripe for the reception of their views.

Twelve years passed, and a cavern in Devonian limestone was discovered at Windmill Hill, near Brixham. By this time scientific opinion was veering round to a more favourable quarter, and the

Royal and Geological Societies took immediate advantage of the discovery, by appointing and subsidising a Committee to explore exhaustively the untouched cave. On the shoulders of Mr. Pengelly fell practically all the work of supervision, but he had his reward in finding ample corroboration of his previous conclusions; for beneath the unbroken stalagmitic floor were brought to light relics of human handiwork in such intimate association with the remains of extinct mammalia as to place their contemporaneity beyond all cavil.

A change of attitude towards the great question of the geological range of Man having been thus effected, the eyes of many were again turned towards the depths of Kent's Cave, if haply they might yield fresh evidence bearing upon this subject. At the Bath meeting of the British Association in 1864, mainly by the influence of Sir Charles Lyell, who was then President, a powerful committee was appointed for the systematic exploration of the cavern; and of this committee—to borrow Professor Bonney's words—Mr. Pengelly was "the hands and the eyes, and, at least, a fair proportion of the compound brain." In fact, from the commencement of the work, on March 28, 1865, until its close, on June 19, 1880, Mr. Pengelly laboured with an untiring enthusiasm, which is attested by his long series of annual reports to the Association. One of the most important incidents in the course of this exploration was the confirmation—though not until the work had been continued for more than seven years—of MacEnery's discovery of the teeth of *Machærodus* in Kent's Cavern.

At the meetings of the British Association, Mr. Pengelly was always a welcome figure, whether in the Geological section or in the Anthropological department. Over the former he presided at the Plymouth meeting, in 1877, and over the latter at the Southport meeting, in 1883. On each occasion he discoursed, in his presidential address, on the Bone Caverns of Devonshire.

After the model of the British Association, Mr. Pengelly suggested the foundation of a Devonshire Association for the Advancement of Science, Literature, and Art. This was formed in 1862, and at the Barnstaple meeting, five years afterwards, Pengelly fitly occupied the presidential chair.

In order to settle the vexed question of the age of the Bovey beds, he undertook a systematic examination of these deposits, and in 1861 read before the Royal Society a valuable paper on "The Lignite and Clays of Bovey Tracey." At the same meeting, a communication was received from Dr. Oswald Heer, to whom the plant-remains had been submitted, "On the Fossil Flora of Bovey Tracey." These essays were reprinted, with a preface, as a monograph, in 1863; and although Mr. Starkie Gardner's subsequent studies of the Bovey flora have modified our opinion as to the age and origin of the deposits, Mr. Pengelly's work remains of great permanent value.

A fine collection of Devonian fossils from Devonshire and Cornwall, formed by Mr. Pengelly, was presented in 1860 by the Baroness

Burdett-Coutts to the Museum of the University of Oxford, in connection with the foundation of a geological scholarship, and is still known as "The Pengelly Collection." In 1863, Mr. Pengelly was elected a Fellow of the Royal Society, mainly in recognition of his work at Bovey and at Brixham; and in 1886 he received the Lyell Medal from the Geological Society—an award of singular fitness in so far as it commemorated the author of the "Antiquity of Man," who had been largely instrumental in securing the exploration of Kent's Cave. It is hoped that the memory of Mr. Pengelly may be suitably perpetuated, and to this end an influential Committee has been formed, including the President of the Royal Society, Professor Huxley, Sir John Lubbock, and Sir Douglas Galton, to consider a scheme for the provision of more Museum accommodation at Torquay.

Mr. Pengelly's scientific work was characterised by extreme honesty of purpose and thoroughness of execution; no amount of labour was considered too great for the elucidation of a detail which, to others, might seem insignificant. The work he undertook demanded accurate observation and sagacious inference rather than the gift of originality. Outside scientific circles Mr. Pengelly was at one time well known as a popular lecturer: his lectures were marked by much freedom of utterance and freshness of language, while they were certainly not lacking in one of the prime elements of success in public speaking—a touch of humour. To most people his geniality of presence was magnetic. In fact, many of us feel that, by Mr. Pengelly's death, we have lost one of the most clear-headed, quick-witted, and warm-hearted of friends.

F. W. R.

VERNON LOVETT CAMERON.

BORN 1844. DIED MARCH 26, 1894.

BY the death of Commander Cameron, owing to a fall from his horse at Leighton Buzzard on 26th March, we have lost another of the leaders of the "heroic age" of African exploration. He was born in 1844, and early entered the Royal Navy, and had his interest in African exploration roused while engaged in dhow-chasing off the East Coast of Africa. He was in Zanzibar when the Royal Geographical Society's Livingstone Relief Expedition broke down by the resignations of its successive leaders, Lieutenant Dawson, Lieutenant Henn, and Mr. New. He then proposed to organise another, and was commissioned by the Geographical Society to start to the relief of Livingstone, and then to solve the problem as to whether the Lualaba was really the head river of the Congo or the Nile. He left Bagamoyo early in 1873 with three other Europeans and a caravan of over 230 men. Though he succeeded in performing what was then the great feat of crossing Africa, the expedition was not a complete success. The caravan met Livingstone's body as it was being carried back to the coast, and thus the first object

of the expedition was frustrated; but Cameron resolved to push on and accomplish the second. This he had to do single-handed, for Murphy resolved to return with Livingstone's men, while Moffat had died near the coast, and Dillon had shot himself in the delirium of fever. Cameron reached Tanganyika, and there made his principal contribution to African geography—the discovery that, *at the time of his visit*, the Lukuga was an outlet from the Lake to the west. He then reached the Lualaba and descended this to Nyangwe, a point previously reached by Livingstone. Here the natives forbade his advance, they refused him boats, and his men protested: so Cameron gave it up, and struck away to the south-west to the Lomani and across the head streams of the Kassabe to Benguela, which he reached in November, 1875. During the latter part of his march his astronomical observations enabled him to fix the positions with greater precision, but the great question as to whether the Lualaba belonged to the Nile or the Congo he left exactly where he found it. Stanley arrived a little later and had just the same opposition, and was in less advantageous circumstances; but he defied the natives, seized boats when he could not buy them, coerced his men, and accomplished his mission. A comparison has often been instituted between the methods of the two men, and Cameron's peaceful tact has often been commended in contrast with Stanley's forceful ways. But it must be remembered that they were both sent out to do a certain piece of work, and that while Cameron failed Stanley succeeded. In later years Cameron explored the "hinterland" of the Gold Coast and the Niger in 1882, and also the proposed railway route to India down the Euphrates Valley, in which scheme he was a devout believer. For some years he had settled down in England, his time being mainly occupied with his work as promoter and director of several commercial companies.

JOHN JENNER WEIR.

BORN AUGUST 9, 1822. DIED MARCH 23, 1894.

ANOTHER veteran entomologist has passed away. Mr. Weir died on 23rd March at the age of 71. He was principally known as a student of the Lepidoptera, numerous papers on which have appeared by him during the last thirty years. He was one of the earliest observers to remark the now well-known connection between the colours of caterpillars and their edibility by birds, a paper on this subject having been published by him in the *Trans. Ent. Soc.* for 1869 and 1870. Students of the British fauna will recall his work on the moths of Shetland, published in the *Entomologist*, on the editorial staff of which magazine he served up to his death. Melanism and other colour problems have received elucidation by his researches, which included vertebrates—specially birds—as well as insects. In the April number of the *Entomologist* is published also his last paper—

a discussion on some genera of Limnaine butterflies, with a description of a new species from Borneo, illustrated by a boldly-drawn figure. An autobiography, with portrait, appeared in the *British Naturalist* for July, 1893.

CHARLES EDOUARD BROWN-SÉQUARD.

BORN APRIL 8, 1817. DIED APRIL 1, 1894.

THIS eminent physiologist was the son of Edward Brown, of Philadelphia, and was born at Port Louis, in the Mauritius. His mother, from whom he derived his name, was a Frenchwoman. In 1838 he went to Paris to study medicine, received his doctorate in 1840, and proceeded at once to make those researches which have rendered his name famous. He held a post for a short while in London at the Hospital for Paralytics, and from 1864–1868 he held the Professorship of Pathology and Physiology at Harvard College, United States. In 1878, he succeeded Charles Bernard in the chair of experimental physiology at the School of Medicine at Paris. Among his best-known works may be cited his researches into the composition of the blood, animal heat, the spinal cord and its diseases, and the characters of the brain. He was the founder of the *Journal de la physiologie de l'homme et des animaux*, and was its editor from 1858–1863; he directed the *Archives de Physiologie Normale et Pathologique*, with Charcot and Valpian from 1868 onwards, and in 1873 he established a practice in New York, and, with Dr. Segrim, founded and edited the American Journal *Archives of Scientific and Practical Medicine and Surgery*. He was president of the Biological Society of France in 1887.

GEORGES POUCHET.

BORN 1833. DIED MARCH, 1894.

GEORGES POUCHET was the son of Félix Archimede Pouchet, and was born at Rouen in 1833. He studied medicine at Paris, receiving his Doctorate in 1864. The year following he was appointed assistant-naturalist and chief of the anatomical department of the Musée d'Histoire Naturelle at Paris, a post he continued to hold until his death. In October, 1870, he became general secretary to the Prefecture of the Police, but soon returned to his natural history.

Pouchet published "De la pluralité des races humaines" (1858); "Précis d'histiologie humaine" (1863); "Colorations de l'épiderme" (1864); and numerous memoirs on the great ant-eater and other edentates.

THE deaths are also announced of:—Dr. WILLIAM F. POOLE, the well-known librarian of Chicago. Dr. Poole was in his seventy-third year, and had been librarian at Boston and Cincinnati before he went to Chicago to take charge of the Public Library. Since the fire

of 1871 he has organised the Newberry Library in that city. Dr. Poole was President of the American Library Association for several years. His great work, "An Index to Periodical Literature," laid a debt upon readers difficult to estimate, and it is unquestionably one of the greatest bibliographic labours of any time.

Mr. HENRY BEAN MACKESON, who died at Hythe in March last, was in his eighty-third year. He was well known as an archæologist and geologist, and was the one to discover the great Saurian in the lower greensand of Hythe, which was described by Owen as *Dinodocus mackesoni*.

We also regret to record the death of JOHN BICKERTON MORGAN, of Welshpool, one of the most promising geologists of the younger school. Mr. Morgan died of phthisis in March.

Among others who have passed away we notice the name of GEORGE PYCROFT, of Richmond, Surrey, whose death occurred at Torquay on Good Friday. Mr. Pycroft was one of the original founders of the Devonshire Association for the advancement of science and art, his chief interests being centred in archæology and art. Mrs. MARY HEMENWAY, the Boston millionaire, who employed throughout life her knowledge, experience, and great wealth in inaugurating and fostering all matters relating to social and educational progress, scientific and historical research. She is best known as a friend to American archæology, and promoted and carried through the "Hemenway Exploring Expedition" in Arizona. DR. F. ULRICH, Professor of Zoology in Hanover Polytechnic; DR. S. A. WEISS, the well-known Professor of Botany at Prague; and DR. ARTHUR HILL HASSALL, the microscopist and analyst, who died on April 9 at his home at San Remo; also swell the heavy list which we regret to have to record this month.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

MR. JOHN C. WILLIS has left Cambridge for Glasgow, where he takes up the appointments of "Senior Assistant in Botany" at the University, and Lecturer in Botany at Queen Margaret's College. Mr. J. Elfreth Watkins, for nine years Curator of the Department of Transportation at the National Museum, Washington, has lately left for Chicago to take charge of the Department of Industrial Arts at the New Columbian Museum. It was Mr. Watkins who arranged the remarkable exhibit of the Pennsylvania Railroad at the World's Fair. Dr. O. Mattirolo has been appointed Extraordinary Professor of Botany and Director of the Botanic Garden at Bologna. Dr. F. Dahl has been appointed Professor of Zoology, and Dr. F. Shütt Professor of Botany, at Kiel University. Mr. Walter Garstang, M.A., who was recently elected to a Research Fellowship at Lincoln College, Oxford, has resigned his post on the Plymouth Staff of the Marine Biological Association, and will leave Plymouth towards the end of May. We understand that Mr. Garstang intends to devote himself to his Fellowship researches and to the early completion of an illustrated descriptive work upon the littoral zoology of the British Isles.

A SHORT time ago the Berlin Aquarium built a Zoological Station at Rovigno, of which Dr. Hermes is Director. The internal arrangements are now so far completed that there is room for six students. Two tables are at the disposal of the German Government, two at that of the Prussian "Kultusministerium," and two at that of the Berlin Aquarium. Students may be of any nationality. We regret to learn, through the *American Naturalist*, that the Biological Laboratory founded at Milwaukee by Mr. Allis, has had to close in consequence of the hard times. The surplus copies of the five Annual Reports upon the Liverpool Marine Biological Station formerly on Puffin Island (1888 to 1892, the complete set) have been collated and bound up to form an 8vo volume of about 180 pages, illustrated with cuts and plates, and containing the original lithographed covers. Thirty copies of this volume are now offered at 3s. each nett (post free), and may be obtained from Mr. I. C. Thompson, 4 Lord Street, or Professor Herdman, University College, Liverpool.

THE Director-General of the Geological Survey is arranging at the Museum of Practical Geology a series illustrative of the submarine deposits now forming off the coasts of the British Isles. This should prove of great value and interest to all naturalists.

UNDER the auspices of that useful body, the A. W. P. L. (for which see NATURAL SCIENCE, vol. iv., p. 254) some short courses of lectures on Natural History are being given in the Natural History Museum in the Cromwell Road. These courses are specially intended for children, and are followed by rambles in the Museum. The fee for a course of six lectures is ten shillings. As the Trustees of the British Museum provide one of their public galleries for a lecture-room, as well as screens,

specimens, chairs, and the services of an attendant, it is to be hoped that the general public will benefit by this innovation, and that the A.W.P.L. will not be the only body to secure a commission on the receipts.

THE Museums Association meet this year in Dublin during the last week in June, under the presidency of Dr. Valentine Ball, C.B., F.R.S. There is every promise of a successful meeting, as a strong local committee has been formed in Dublin, and we note that the council invite the attendance of members of Museum committees as well as curators. Further information may be obtained from H. M. Platnauer, Museum, York, or E. Howarth, Museum, Sheffield.

THE Rutherford College, Newcastle-on-Tyne, which may be described, in the words of Lord Armstrong, as "a cheap working-man's college, designed chiefly for useful technical education, but intended, also, for the advancement of all pupils of excessive ability who aspire to professional life or to the attainment of academical honours," was formally opened by the Duke of York on April 5. This College, which is raised as a monument to the late Dr. J. H. Rutherford, who did so much for education among the masses, provides accommodation for 2,000 students. The Trustees have established some 30 scholarships, tenable for three years, ranging from £9 to £15 a year, and also an exhibition of £50 a year to the University of Cambridge. The building contains fine art, physical, chemical, biological, metallurgical, and engineering departments, with workshops, lecture theatre, library, and reading-room. The College is affiliated to London University, and associated with the Science and Art Department at South Kensington. A tower at one end of the building will be utilised as an astronomical observatory.

THE date of the Robert Boyle lecture at Oxford is now definitely fix May 8. Professor Alexander Macalister's subject, "Some Points of M logical Interest in Human Variations," is much "on the moment," as the new journalism phrases it. At the conversazione of the Junior Scientific Club, May 22, Captain Lugard will speak. We thought, by the way, the Robert Bo lecture had been founded to remove lectures from the conversazione. Profes August Weismann, who is to deliver this year's Romanes lecture, arrived in Engl on April 10.

THE Summer Educational Plans of Toynbee Hall, Whitechapel, contain the following alterations in the lecture list that we published last February (p. 156). Dr. Fison's subject is now "Light Waves and the Ether," while Mr. Parkyn talks on "The Tissues of the Body." On April 27, Mr. C. G. Moor begins a short course on "The Chemistry of Common Life," with practical laboratory classes. Dr. A. S. Gubb now takes a Sunday class in Physiology. The Natural History Society announces several excursions; its evening meetings are on the first Monday of each month.

THE London Amateur Scientific Society appears to have ceased publishing, but in the form of meetings at the Memorial Hall, Farringdon Street, its activity still continues. The secretary is Mr. S. Pace, 252 Fulham Road. Papers have lately been read, "On the Ascent of Water in Trees," by L. A Boodle; "On the Occurrence of Gold in Eruptive Rocks from South Africa," by G. Holbrook; and "Some facts about Parasitism and kindred Phenomena in Plants," by Jesse Reeves. On April 27, F. A. Bather is announced to discuss the vexed question, "What is a Genus?"

THE chief features of the report of the Zoological Society of London in the accounts for 1893 are an increase of £300 in cost of provisions, of £200 in menagerie expenses, and of £400 in the cost and carriage of animals. There is, fortunately, an increase of £1,600 in admissions to the Gardens, which assists in

balancing the above-mentioned and other out-payments. The mortgage debt, which is steadily reduced £1,000 a year, now stands at only £2,000. The Society hold no stocks beyond the £2,000 Davis Fund Trust.

THE report of the Epsom College Natural History Society for the year ending Christmas, 1893, reaches 86 pages, and shows evidence of considerable activity. Among the lectures delivered last year we note one by Mr. J. J. H. Teall, on Silica, one on the Puss moth, by Mr. O. H. Latter, and a third, on Captures of a Tow-net, by Dr. Hudson. Numerous excursions were made in the district round Epsom, a method of instruction upon which it is impossible to lay too much stress. Lists are given of the birds, plants, and lepidoptera of the district, with notes on their earliest observed occurrence. The system of giving a prize for the best lecture or essay, delivered by a member of the school during the year, is an excellent one, and tends to stimulate research and accuracy. Last year the prize was won by Mr. H. Mayo, for his paper on "Evolution." This year the subject selected for competition is "The Life and Work of Darwin." Might we suggest that subjects of more local and less cosmic interest would better bring out the scientific capabilities of our intelligent youth?

FROM the Sixty-ninth Annual Report of the Norfolk and Norwich Museum we learn that the glass cases are almost completed. The departments of Ornithology, Mammalia, and Ethnology have been enriched by important additions, among which may be mentioned: a series of specimens from the bank of the Amazon; an adult skull of *Hippopotamus amphibius*; twenty-two specimens of birds of prey from Mr. J. H. Ha-ney; and numerous animals of local interest and rarity, including the adult tion, e of the great Bustard (*Otis tarda*) referred to in Stevenson's "Birds of Norfolk" Mr. (iii., p. 401), which now permits the exhibition in the Museum of an un- the e-alled group of these birds. A full account of the Norfolk and Norwich Museum of f-m the pen of Dr. Henry Woodward appeared in this Journal for November, 1892. ie twenty-fifth annual meeting of the Norfolk and Norwich Naturalists' Society is held in the Museum, Norwich, on 27th March, the president (Mr. T. Southwell, of Z.S.) in the chair. In reading his address, Mr. Southwell remarked upon the pub-lic-ation in the Society's *Transactions* of very valuable records of the local fauna and flora, and of the biographical memoirs of Norfolk naturalists, among the more interesting Ge-which we recall those of C. B. Rose and Samuel Woodward. The series of th-ters from Gilbert White to Robert Marsham, of Stretton, were also referred to, as well as the endeavours of the Society to prevent the wanton destruction of rare and interesting birds that occasionally visited the country. The full strength of the Society is 226, and despite the heavy cost of printing the *Transactions*, it is now in a fairly prosperous condition. Mr. Southwell then gave a sketch of the avifauna of Norfolk, and the pastoral value of its soil. Dr. Charles Plowright was elected president in succession to Mr. Southwell, and Professor Collett and Mr. E. T. Newton were elected honorary members.

WE note the following papers in Publications of British Societies. The December number of *Timehri*, the journal of the Royal Agricultural and Commercial Society of British Guiana, contains a paper by the editor, Mr. Rodway, on the history of the Society for the first two years of its existence. The writer notes that the 18th March, 1894, was the fiftieth anniversary of the foundation of the Society. The excellent work of the Society in furthering the agricultural and commercial interests of the colony is touched upon, and attention is called to the importance of the Museum and Library, and the large collection of newspapers and magazines to be found in the reading room. No more interesting work can be done by the editor of a fifty years' old Colonial journal than to give a sketch of this kind, for the present generation are apt to forget the names of those who founded, and of those who carried successfully on, a Society which has stimulated such good work and provided so much entertainment for those whose interests lie in the development and

advancement of their working field. Another paper of value in this number is that of the Rev. W. B. Ritchie, on "The First Thirty Years of Schools and Schoolmasters in British Guiana." The *Proceedings of the Dorset Natural History and Antiquarian Field Club*, for 1893, has been issued. Mr. Mansel-Pleydell's Presidential Address deals chiefly with the geological distribution of plants, and forms a useful synopsis of the subject. Mr. S. S. Buckman discusses the Inferior Oolite and its subdivision into zones; while the President adds a further note on the Dewlish "Elephant Bed," with photographic illustrations. The Rev. O. P. Cambridge continues his valuable reports on new and rare British spiders, and the Rev. R. P. Murray gives a coloured illustration of his new bramble, *Rubus durotrigum*, described two years ago in the *Journal of Botany*. The other papers are of archæological interest. The Royal Society of South Australia has issued the second part of vol. xvii. of its *Transactions*. The papers are mostly of a systematic nature, dealing with Galls, Rhopalocera, Heterocera, Coleoptera, and Tertiary Gasteropoda; but two papers by W. G. Stretton and Rev. D. Mackillop give a most interesting account of certain aboriginal tribes in the Northern Territory of S. Australia. One of the most useful branches of this flourishing Society is a committee for the protection of the native fauna and flora, many species of which are in great danger of extermination. Among the many interesting papers in the second number of the *Proceedings of the Malacological Society of London*, mention must be made of the useful paper on British Chitons, by Mr. E. R. Sykes; an excellent plate is given, and the student should now have little difficulty in identifying his finds. A similarly valuable paper has just been published by the Geologists' Association (*Proceedings*, vol. xviii., p. 190), by Mr. Smith Woodward, on "Sharks' Teeth from British Cretaceous Deposits." This paper is also well illustrated, and will enable many an amateur geologist to identify his Chalk finds without worrying a Museum curator. Dr. H. B. Guppy has a paper on the Temperature of Rivers in the last number of the *Proceedings of the Royal Physical Society of Edinburgh*. Only part i. has yet appeared, and deals with the daily changes and method of observation.

AT the twelfth annual meeting of the American Society of Naturalists, held at Yale University, December 27 and 28, 1893, a committee was appointed to appeal to Congress for the repeal of that tax upon knowledge which is embodied in the Customs duties on instruments of research. It was pointed out that these duties were not needed for the protection of the American manufacturer; for at least one American firm was ready to sell its goods at a price a little below that of the foreign manufacturers to those institutions that could already obtain duty-free prices, while for all others it added the extortionate 65 per cent. of the present tariff. In the interests of science we wish the committee every success. The American Society of Morphologists held its annual meeting at New Haven on December 28 and 29, 1893, while the American Physiological Society was holding its sixth annual meeting at the same time and place. These two Societies have entered a movement of closer co-operation among the leading American scientific Societies, headed by the Society of Naturalists. It is hoped that the Psychologists and Geologists will also join. Co-operation is admirable; but the immediate result appears to be the formation of something like a second American Association for the Advancement of Science.

IN the March issue of the *Botanical Gazette*, Mr. F. C. Coville, botanist to the U. S. Department of Agriculture, states that the Department has inaugurated a seed collection in connection with the National Herbarium, which is intended to include seeds of all the species of plants obtainable, especially weeds and forage plants. The seeds, when not too large, will be placed in flat-bottomed specimen tubes, which will be labelled, and arranged systematically in covered trays made of binder's board. Fleshy fruits of native American plants will be put into similar bottles filled with a preserving liquid. Authentic herbarium specimens of plants raised from the seeds represented, or of plants from which the seeds were obtained, will accompany the

collection whenever possible. The Division of Botany also proposes to undertake the testing of various seeds as to purity and germinative powers, for which purpose a laboratory will be fitted up and equipped after the most approved methods of American and European seed control stations. Physiological experiments connected with germination and development will be conducted both in the laboratory and the open air. Histological studies may ultimately be made of the structure of the seeds of American weeds and forage plants, partly with the view to elicit facts of taxonomic value. The entire work will be carried on with special regard to its economic importance, while the collection will be useful for reference. Mr. G. H. Hicks, recent instructor in Botany at the Michigan Agricultural College, has been placed in charge of the matter.

It will be remembered that, some time ago, the appropriation voted by Congress towards the United States Geological Survey was materially reduced. From Major Powell's annual report we glean the following results of this reduction. During the early part of the fiscal year the field parties engaged in geological work were entirely reorganised. A large reduction of the force employed in palæontological work was made necessary, and several former divisions were consolidated into one division, in charge of the chief palæontologist. The division of illustrations was dissolved at the opening of the year, and then re-organised, with a force of seven persons. The total expenditures of the survey for the year amounted to $423,022, against an appropriation of $430,073. It is interesting to note that, despite the reduction, much work, of great economic value to the nation, has been done on the running waters of the country. Steps are already being taken, it is stated, to use at least the smaller streams for water power and irrigation. A rough computation, based on the stream measurements and census figures, indicates, it is cited, that the running streams of the arid region, when all are used, will have a value of at least $6oo,oo,ooo. Those who know the secret of the reduction of the grant will find food here for sweetly ironical reflections.

In November last we announced that Mr. W. F. E. Gurley had been appointed State Geologist of Illinois, and we feared that the palæontological work of the State would be entrusted to the well-known and prolific author of new specific names, Mr. S. A. Miller. Our worst anticipations have been realised. The Illinois State Museum of Natural History has published as its *Bulletin* no. 3 a "Description of the New Species of Invertebrates from the Palæozoic Rocks of Illinois and adjacent States" by those two writers. This appears equal to their previous work, and therefore needs no criticism. We merely wish to point out that fifty-eight new species are here described in a State publication, under the name of a State official; but that only four of the type specimens are in the State Museum, the remaining fifty-four being in the private cabinets of the two authors. "It requires neither information nor study to propose specific names" (p. 55); but the type specimens gain in pecuniary value even when the species are worthless.

Kansas University appears to have undertaken the work of a Geological Survey during 1893. Professor Erasmus Haworth organised a class of three who studied the stratigraphy of the south-eastern part of the State. Nearly 600 miles of sections were made and much other work accomplished, a report of which will be found in the *Kansas University Quarterly* for January, 1894. Enthusiastic students are always to be found among the young; this method supplies them with what they chiefly lack—organisation and experience.

The United States Government are fitting out an expedition to explore the region of the north magnetic pole. This spot, which lies on the western point of the Isle of Boothia Felix, near Cape Adelaide, has only been described once, when Ross's expedition discovered it, and a great deal of curiosity is felt as to whether it will have shifted at all during the last sixty-five years.

CORRESPONDENCE.

A Whale Fight in 1240.

In reference to Mr. R. Lydekker's account of a whale fight in the South Atlantic, printed at p. 173, it may be of interest to quote the account of a whale fight which occurred in 1240, probably near the mouth of the Thames, as given by Matthew Paris, the chronicler of St. Albans, in his *English History*.

A.D. 1240.—"*A Remarkable Battle amongst the Fishes of the Sea.*—Although other great and unheard-of wonders happened in this year, we have thought it worth our while to mention in this work one more remarkable than the rest. As it is the nature of the sea to vomit up on dry land the dead bodies thrown into it, about eleven Whales, besides other marine monsters, were cast up on the sea coast of England dead, as if they had been injured in some kind of struggle, not, however, by the attacks or skill of man. The sailors and old people, dwelling near the coast, who had seen the wonders of the deep when following their vocation in the vast waters, and trafficing to distant countries, declared that there had been an unusual ba[illegible] amongst the fishes, beasts and monsters of the deep, which by wounding and gr[illegible] each other, had caused death to several; and those which had been killed h[illegible] cast ashore. One of the fishes, a monster of prodigious size, made its way i[illegible] Thames, and with difficulty passed uninjured between the pillars of the bridg[illegible] was carried as far as a manor of the king's called Mortlake, where it was foll[illegible] by a number of sailors, and at length killed, after a great deal of trouble by innu[illegible] able blows of spears."

Dunstable. Worthington G. Smith

An Ammonite Problem Solved (?)

With reference to our note with this title (p. 245), Mr. S. S. Buckman points out to us that a distinction should be drawn between "beds which only yield worn and imperfect specimens," and "beds which afford specimens in a complete state of preservation, the shell-sculpture remarkably sharp, and the delicate mouth-processes complete." In the former cases "distribution is readily granted"; but in the latter "it is reasonable to conclude that there has not been much distribution after death."

While admitting some such caution, we may, nevertheless, suggest that the shell of *Spirula* is a sufficiently delicate structure, and that distribution by flotation would not really expose the shells to many casualties.

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NOTES AND COMMENTS.

Composite Generic Fundamenta.

THE *American Geologist* for April contains a note by Mr. J. M. Clarke, which is of such interest in connection with subjects lately discussed in our own pages, that we take the liberty of quoting it at length :—

"Some remarks on 'Mathematical Biology' in the February number of Natural Science, and the suggestion made by Bather in his recent work on the 'Crinoidea of Gotland,' that Galton's method of composite portraiture might be employed to ascertain a specific type from which variations in various degrees could be reckoned, recalled to me some germane experiments in which I was interested some eight years ago, and which indicate that there is even a possibility among narrowly restricted, up-to-date genera, of educing, by a series of comparative measurements and successive overlays, a sort of generic *fundamentum*. I take the liberty of citing and illustrating a single instance of this among the fossils, the case being a simple one and involving the search for the standard in one element of variation only, namely, *outline*; but whatever may be done with a single specific character, in this manner, is equally possible of all the others.

"In volume v., pt. 1, of the Palæontology of New York are described 57 species of the lamellibranch genus *Leptodesma*, two of which are ascribed to the horizon of the Hamilton group, and 55 to the fauna of the overlying Chemung beds. *Leptodesma* is an aviculoid genus with smooth and convex valves, and the shells upon which these observations are based are preserved plump and without disturbance of the marginal outline. The genus in some of the higher beds of the Chemung group is enormously prolific, so that the perception and identification of these 55 specific values is a puzzle to which the identification of Jurassic Ammonites or recent Unios is but play. Nevertheless, no more instructive series of variations of a single generic type has ever been described among

Palæozoic fossils. The first onset at such a group is certain to lay the student low with acute brain-fag, and leave upon his mind a hazy conviction that all the alleged species are pretty much one and the same thing. The beauty of the series and the persistence of its variations or species dawns upon the mind gradually but invincibly. Having attained this point in the study of the original specimens of these fossils, the attempt was made by the process of superposition to ascertain a standard or radicle outline for the entire generic group. In the making of this composite figure, which is here reproduced, it was, of course, necessary to fix upon two points or one dimension as a base to which all the elements could be reduced or elevated: just as in composite portraiture, the eyes, or some other indices, of all faces in the composite must be coincident. In this case the standard to which all have been made to conform has been the distance between the anterior cardinal extremity and the deepest posterior incurvature of the valve; a purely arbitrary dimension; probably any other would serve as well.





"The figure represents superposed outlines of the left valve of 36 of these species of *Leptodesma*. It will be observed that notable differences of outline in the components of this figure are obscured by the coincidence of the lines for a greater or less portion of their length, while the ruling outline or type stands out conspicuously like a coarsely shaded drawing of the valve. Of all the component outlines this interwoven valve approaches nearest to that of *L. rogersi*, as shown in the adjoining figure. It will naturally be understood that *L. rogersi* enters into this combination no more than the other 35 species, nor are there other shells whose outline is so nearly similar to that of *L. rogersi* as to produce a succession of parallel lines, which thus become intensified. The depth and predominance of the *L. rogersi* outline is wholly due to the coincidence of parts of outlines of species which vary greatly in this respect, as witness the four silhouettes of elements in this composition, shown on page 403.

"As already remarked, what is here done for the outline of the valves can be done with equal facility for convexity or other specific variations, either by this method of superposition or by the expression of such differences by a series of co-ordinates or mathematical curves.

"One interesting fact in regard to the type or *fundamentum* expressed in this composite is that *L. rogersi* (the silhouette of

which, here given, is of exaggerated size) is the temporal precursor of the cometary outburst of *Leptodesma* in the Chemung faunas, and is one of the two species occurring in the Hamilton faunas beneath.



"I am disposed to believe that whenever this method is practicable it will be found of real usefulness not only to biologists in general, but to the palæontologist in particular, especially in the determination of specific standards or *fundamenta*, as suggested by Bather. Its successful application implies, first of all, abundant data. With that prerequisite provided for, profitable comparisons could be instituted between species of distant faunas supposed to be identical, or which, though perhaps identical, may be masquerading under different [illegible]; and between such species in successive faunas of the same region. As a case in point, the brachiopod species *Athyris* or *Seminula subtilita* Hall would serve; an abundant and remarkably variable shell from the upper Carboniferous. In the lower Carboniferous limestones occurs an allied form, *A. subquadrata*, and it has been confidently asserted that neither species passes into the geological horizon of the other. Yet the two are greatly alike and each appears under many variations of outline and contour. Assuming that the two are alike in internal structure, which I believe to be essentially true, a series of composites might determine whether or not the variations of the two so-called species reduce to similar *fundamenta*, and if they do I should be inclined to regard this as rational evidence of specific identity. Again, the geological value of this method might be tested in such a case as that recently adduced by Professor Prosser, who argues that an extensive series of rocks in central and eastern New York, which appear to be permeated with Hamilton fossils, are not of the age of the Hamilton group, but represent a later geological stage, and that the fossils have, in a considerable degree, undergone modifications from their typical expression in the true Hamilton fauna. As the quality of these variations is hardly palpable under ordinary circumstances, and has not been satisfactorily expressed in words, it ought to appear in composites based upon sufficient data; and should there prove to be differences in the *fundamenta* of specific variations, from the true Hamilton fauna, and

of those from the alleged but contested Hamilton fauna above, the argument from such evidence would be entitled to serious consideration."

ORGANIC EQUILIBRIUM.

IN our last number, the review of Mr. Bateson's book on Variation gave occasion for some remarks on continuity and discontinuity in species. It will be remembered that the guiding idea of that book is that a species may change from one specific type to another by a sudden jump, or, as Mr. Bateson phrases it, by a discontinuous variation. In the words of Francis Galton, the species passes from one position of organic stability to another. It may therefore be interesting to remind our readers that similar ideas were long ago put forward by two American biologists, who, however, went even further in supposing a sudden passage of more than one species from one genus to another. E. D. Cope in 1868 discussed the Origin of Genera (*Proc. Acad. Nat. Sci. Philadelphia*), and showed, as he believed, that genera were grades of organisation, entered upon by more than one species at a time, and often entered upon with some suddenness. He explained these metamorphoses of species by a reference to metamorphoses at periods of rapid development in the life of individuals. If such periods corresponded with the period of reproduction, the offspring would, he considered, be more liable to variation. When the metamorphosis of the individual was subsequent to reproduction, the offspring, and therefore the species, would remain unchanged; but when the metamorphosis occurred before the period of reproduction, the characters would be transmitted to the offspring and the species would enter on a new phase. Cope compared these points of change to those critical points of temperature at which sudden changes take place in the molecular constitution of matter, and he applied to them the term "expression-points." Carefully examined, however, Cope's principle of "Acceleration" is seen to be essentially the same as what is now called the "Law of Earlier Inheritance"; and those who believe that the latter "Law" expresses the process of evolution of species and genera, certainly do not believe in any sudden change.

W. H. Dall, writing in the *American Naturalist* for March, 1877, similarly ascribes such supposed sudden changes "to the action of a law of development which finds expression in the paradox that the same species may belong to different genera." That sudden leaps may be due to the gradual accumulation of minute differences, he exemplifies as follows:—"In a sloping gutter of a paved street not too cleanly swept every one will have noticed on a sudden shower how small particles of earth and other materials will sometimes act as a dam, producing a puddle, which, relieved by partial draining, may for a time remain in *statu quo*. A time comes, however, when the gradually-accumulated pressure suddenly sweeps the dam before it

for a short distance, until another similar one is formed, the pool again appears for a time to remain unchanged, and so on indefinitely. Now the modern idea of a species may be stated to be a greater or lesser number of similar individual organisms in which for the time being the majority of characters are in a condition of more or less stable equilibrium, and which have the power to transmit these characters to their progeny with a tendency to maintain this equilibrium. This tendency may be, in some cases, sufficiently strong to resist for a considerable period the changes which a gradual modification of the environment may tend to bring about. When the latter has reached a pitch which renders the resistance no longer effectual, it is conceivable that a sudden change may take place in the arrangement of the constitution of the organism, adapting it once more to its surroundings, when the tendency to equilibrium may reassert itself in the minor characteristics, and they may, as it were, crystallise once more in a form not dissimilar in its specific results to that which was recognisable in the earlier generic type. If among a certain assemblage of individuals constituting a species, the tendency to maintain the specific equilibrium be (as it should be *à priori*) transmitted to the progeny in different degrees of intensity, a gradual separation might take place between those with a stronger tendency to equilibrium and those with less. Here natural selection would come in. Those yielding as above to the pressure of the environment would necessarily become better adapted to it (or perish) and with their changed generic structure might be able to persist. On the other hand, those with the broader base, so to speak, with the inherited tendency to remain unshaken by the modifications of the environment, may be conceived as through this tendency to be and to remain less injuriously affected by adverse circumstances, and consequently might still endure. In short, natural selection in the one case would find its fulcrum in the tendency to easy adjustment of characters; and in the other case in the inherited persistency in equilibrium rendering its possessor more or less indifferent to the injurious elements of the environment. The intermediate individuals by the hypothesis would be those least-fitted to persist in any case and hence liable to be rapidly eliminated. Then we should have parallel series of species in two or even more genera existing simultaneously."

Induced Variation.

How far the variation of organisms can or does take place in response to change of environment is one of those questions that the biologist is bound to answer before he can solve the problem of species. A determined attempt to answer this question has, we all know, been made for many years by Professor E. B. Poulton, in his experiments on caterpillars. At the Royal Society's conversazione, on

May 2, that investigator exhibited some living specimens of the larva of the Lappet-moth (*Gastropacha quercifolia*). Under usual conditions these caterpillars are of a rather dark brown colour, with a few whitish spots down the sides; some may be darker, and some lighter than the others. The specimens exhibited, however, which had all been reared from the same batch of eggs, had been divided into two lots, and, during the early stages of growth, one lot had been surrounded by black twigs, and the other lot by lichen-covered twigs, the food being the same in both cases. So impressionable were the youthful caterpillars, that all the former lot had become nearly black, with the white spots very few and small, while the latter lot were lighter than usual in colour and had the white spots large and numerous. Subsequent transference of a light caterpillar to the black twigs, or *vice versâ*, had had no effect upon its colouring; the response to environment took place only during infancy. In the course of a lantern demonstration, Mr. Poulton described many other experiments having similar results, and expressed his opinion that the young caterpillar did actually possess considerable sensibility and power of adaptation to the colour of its surroundings. This sounded like flat Lamarckism, but only for a moment. There was a limitation to be noted; there was no response to such colours as the caterpillar did not ordinarily come in contact with in a state of nature; or, if any effect was produced, it was only in the direction of some variation natural to the animal, and useful to it under certain natural circumstances. For this reason, and because all variations that do occur are clearly protective, the lecturer concluded that the marvellous power of individual adaptation to environment had been acquired through the ordinary processes of natural selection. At least that is at all events sound Darwinism. Whether the experiments are yet sufficiently conclusive, will however be doubted by many entomologists.

A most interesting example of this individual adaptation, regularly occurring amid natural surroundings, has recently been added to the beautiful series exhibited in the central hall at the British Museum (Natural History). In the forest of Maramanga, which is in Madagascar, about 65 miles east of Antananarivo, there lives a Homopterous insect, which men (scientific men) call *Flatoides dealbatus*. When this insect settles, as is its habit, on the bark of the forest trees, it becomes almost imperceptible; for each specimen, though differing slightly from its neighbour, bears, when its wings are closed, a most striking resemblance either to plain bark, or to bark covered with green moss or grey lichen. This, as the Museum label with true scientific caution expresses it, is "a fact which seems well calculated to afford them protection." Madagascar has also furnished the Museum with the curious beetle, *Lithinus nigrocristatus*, whose black and white or yellowish colour and black hairs render it almost indistinguishable from the lichen-covered twigs among which it lives.

Some sceptics say that birds must be very foolish creatures to be deceived by certain so-called protective resemblance; but the birds in Madagascar must be very old birds indeed if they often get a meal off either of these marvellous examples of mimicry.

The Ways of the White Ant.

An example of individual adaptation of somewhat different nature was also to be seen at the *soirée* of the Royal Society, in the case of Termite ants exhibited by Dr. D. Sharp. We have already alluded to the recent work of Grassi and Sandias on the two European species of this family (Natural Science, vol. iv., p. 312), a work that has stimulated afresh our curiosity as to the social economy and the physiology of the white ants. As has been partly explained by Dr. Cunningham in our own pages (vol. iv., p. 287), Grassi has arrived at the conclusion that the various forms making up the species of a Termes are produced at will by the community from similar individuals of an "undifferentiated" nature. His statements, in fact, amount to this, that the continuation of the species being confided by the community to a single pair, the produce of this base supple is made into the various castes required by the community by runnings of processes of special feeding. Should the community require new royal pair, they apply to a certain number of individuals a food l arse of feeding by which the reproductive organs are brought into ir development and functional activity, while the external parts of the body remain permanently in the pre-adult state. To such individuals the name "neoteinic,"[1] or "permanently adolescent," has been applied. Their number is afterwards reduced by the community to a single pair.

Dr. Sharp's exhibit included some 10 or 12 species of *Termites* that had been collected at Singapore by Mr. G. D. Haviland, and each species was represented by many of its various forms and most of the stages of individual development. In the nest of one species Mr. Haviland had found about 10 neoteinic pairs, leading to the belief that in this species also an economy prevails similar to that in *Termes lucifugus*, as described by Grassi. This Singapore collection, which contains a large number of specimens, all in a perfect state of preservation, has been presented by Mr. Haviland to the Museum of Cambridge University.

Botany; British and Foreign.

The May issue of the *Journal of Botany* contains descriptions of some of the new plants recently collected by Dr. J. W. Gregory in

[1] For explanation of term "neoteinic," see Kollmann, *Verh. Ges. Basel*, vii., 1883, p. 391; also Camerano, *Bull. Soc. Ent. Ital.*, 1885, p. 89, and *Atti Acc. Torino*, xix., p. 84

East Tropical Africa. Mr. Spencer Moore describes a number of Acanthaceæ, including a new genus, and Mr. E. G. Baker a new tree *Senecio*. There is considerable difference in size between our common groundsel (*Senecio vulgaris*) and the new giant which Dr. Gregory discovered growing on the rocky sides of valleys at an elevation of 13,000 to 14,000 feet on the south-west side of Mount Kenia. The woolly leaves of the latter are over a foot in length, and the stem grows to a height of 25 to 30 feet, attaining a diameter of a foot or more. This interesting plant is closely allied to one found by Mr. H. H. Johnston on Mount Kilima-njaro, and described by Professor Oliver a few years ago. Another tree *Senecio* occurs on the Abyssinian Mountains, and one in the Cameroons.

British botanists are not neglected, for in the same number several new native brambles are described. First, one by the Rev. W. H. Purchas, who admits that "It is certainly very undesirable to add to the already lengthened list of British brambles, unless very good cause can be shown for doing so; but" his plant "is so readily distinguishable from other forms, and is so plentiful and characteristic" in its locality "that it would be inconsistent with the treatment accorded to the rest of the genus to leave it without recognition as a well-marked variety." The plant in question was referred by Profess Babington and Dr. Focke, to a known species, *Rubus rosaceus*, but the Rev. W. R. Linton, who has enjoyed the best opportunities studying it, from living in the same neighbourhood, "has made increasingly evident" that "its strongest affinities are with the Caesia group." "Accordingly, the Rev. W. Moyle Rogers, who last yea saw it growing," "considers that it should be placed under *Rubu dumetorum* as a named variety." Finally, however, the two othe reverend gentlemen put their heads together and make a new *species Rubus rubicundus*.

In order to let us down gently and relieve the monotony, the editor thoughtfully inserts the note on the new *Senecio* between Mr. Purchas's description and a paper entitled "Three New Bramble Forms," by the Rev. Augustin Ley, in which a new species from Herefordshire and two new varieties are described.

The genus *Rubus* is a fruitful one for investigation, the species of the British Isles having already supplied matter for no less than five papers in the journal for the present year. It is, however, interesting to note the apologetic tone in which new species are generally introduced. Thus, the Rev. W. Moyle Rogers, before proceeding to perpetrate three in the February number, remarks: "We must, I think, be anxious, all of us, to keep down and, where it may be possible, reduce the number of our *Rubi* 'species'." One cannot help thinking that these worthy clergymen have either not realised the fact of individual variation, or are so impressed with the origin of species by evolution, that in an excess of zeal they name them before they have actually arisen.

Death Valley.

Under the title "Botany of the Death Valley Expedition," the U.S. Department of Agriculture have recently issued a bulky report, prepared by Mr. F. V. Coville, botanist to the expedition, and now head of the division of Botany. Death Valley, bounded on the east by the Funeral Mountains and westward by the Panamints, lies on the Nevada side of California, and forms part of the vast more or less desert region broken by short mountain ranges, which extends between the Rocky Mountains and the Sierra Nevada. In and about this cheerful region, Mr. Coville and his assistants spent more than six months of 1891, collecting plants and making observations on their habitat and environment. An idea of the nature of the country may be gathered from the following description:—"January 19 we broke camp, and set out for Death Valley by the old borax road. The excessive dryness of the region was evidenced by the fact that the pencil marks on a roadside grave-board, which had been twelve years exposed to atmospheric effects, still appeared clear and fresh, the surface of the wood retaining its natural appearance. We passed the night in Long Valley, and in the morning continued down the cañon, emerging into Death Valley near the south end of its alkali-
base. On either side were high mountains and between them the
running w valley, not more than ten miles wide. In the bottom of the
$600,00 was the snow-white stretch of salt and alkali, and to the north-
food l d, perhaps fifty miles away, mountains, valleys, and salt-flat
nished in haze. Creosote bush had been characteristic of all our
Stat ite until we neared the salt-flat; but here, under the influence of
wou y and alkali, it gave way to greasewood (*Atriplex polycarpa*), that in
S. rn to salt grass (*Distichlis spicata*), and the last to a shrub related to
Mu e pickle-weed." Beyond this there was no vegetation whatever.
son

The report, which occupies nearly 300 pages, is an example of what such reports should be, and an illustration of what can be done by a collector who is at the same time a proficient botanist. Besides an exhaustive catalogue of species, including descriptions of 42 novelties, and a tabulated list of specimens arranged in the order of collecting numbers, with date of collection, locality, and altitude, the author gives a valuable account of the distribution of plants in South-eastern California, and of the relationships of the mountain flora of the Sierra Nevada, and of that of Death Valley, and also discusses at some length the characteristics and adaptations of the desert flora. In a humid climate dew is an important source of moisture, but the weather-observer recorded no dew in Death Valley, and the several months' observations of desert conditions and phenomena have led to the conclusion that a very important proportion, perhaps in some cases all, of the summer water-supply of certain desert-shrubs is conserved in the soil as capillary moisture derived from local rainfall. The tree Yucca (*Y. arborescens*) is the nearest approach to the real tree that the desert affords, often attain-

ing 18 feet in height, but this is confined to the high altitudes. It is in the shrubby vegetation that the arid character is most reflected. The largest of the shrubs, the creosote-bush (*Larrea tridentata*) commonly grows to from three to five feet, while the height of most desert species ranges from this down to a foot. The struggle for continued existence is not with each other, as in a humid climate, since from scarcity of moisture each individual is separated by several yards from its neighbours, but against physical forces alone. A rounded form is the result of this spacing out, as, surrounded on all sides by light, the plants develop equally in every direction. No creeping or cone-shaped shrub is known in the desert.

An important principle in plant-life is the absorption of moisture from the soil by the roots, its conduction along the stem, and its transpiration from the leaf-surface; hence the study of desert plants reveals various adaptations for striking a living balance between absorption and loss of moisture. One device is great root development; thus the roots of the mesquite (*Prosopis juliflora*), a tree characteristic of desert areas with a moist subsoil, grow to enormous lengths; one was seen which reached more than 16 yards. As, however, during the season of drought the largest amount of water that can be absorbed is comparatively small, the greatest modifications are found in the stem and foliage. The size, form, and thickness of the leaves are most concerned. Thus, of forty-one shrubs examined, only four had leaves whose single-surface area exceeded a square centimetre. In some cases the leaves fall soon after the cessation of the spring rains, but in most they remain for the greater part of the summer, and to reduce loss of water to a minimum, have their surfaces protected by a resinous excretion or a covering of dry hairs. The creosote bush, for instance, owes its name to the resinous covering, in appearance like shellac, of its leaves and small twigs.

The twenty-one excellent plates of new species will serve to illustrate some of these points, and the large map at the end of the volume on which the route is indicated is also a valuable addition.

THE HATCHING OF THE OCTOPUS.

THAT an *Octopus* should be given to sit upon its eggs for the purpose of hatching them, like any barn-door hen, savours at first of the ludicrous, even though the instance of the female Argonaut which carries, if it does not incubate its eggs in its "boat," be borne in mind. More curious still is it that the habit was first observed and recorded by that astute old-time philosopher Aristotle, but subsequently forgotten till the discovery the other day in Lower California, by M. Diguet, of a new species of *Octopus*, which sets to work in a very systematic fashion, brought the circumstance once more to light. Aristotle remarked that the *Octopus* sits as soon as all its eggs are

laid, and in the same spot in which they are deposited—the crack of a rock or the interior of an empty shell—and he further notes that they do not feed while sitting, so that they become quite thin.

The marked peculiarity concerning the new species, which is described by MM. Perrier and Rochebrune (*Comptes Rendus*, vol. cxviii., p. 770) under the name of *Octopus digueti*, appears to be that it habitually spawns in empty bivalve shells (*Cytherea*, *Pecten*, etc.), lining the interior of the two valves with its eggs, and itself occupying the intervening space. Whether it merely remains in this situation during incubation, or whether, hermit-crab-wise, it makes its abode in empty shells a matter of permanent policy, does not transpire; but MM. Perrier and Rochebrune, judging from their concluding remarks, evidently incline to the latter idea. In fact, this habit is merely a specialisation of the ordinary habit possessed by these shell-less Cephalopoda of protecting their soft bodies by inserting them in foreign shells, rock crevices, old tins, or whatever first presents itself.

Nerves of Molluscs.

A long and apparently most careful paper on the histology and organology of the nervous centres of Gastropoda as exemplified in the genera *Helix*, *Arion*, *Zonites*, and *Limax*, from the pen of M. B. de Nabias, has just appeared in the *Actes de la Société Linnéenne de Bordeaux* (vol. xlvii., pp. 11-202, 5 pls.).

As the author justly remarks, the microscopic structure of the nervous system of the Gastropoda—indeed he might have said of Mollusca generally—is almost totally neglected, attention being devoted solely to the external topography. Mr. Nabias' researches show that there exist two sorts of nervous cells in the nervous centres. First, ganglionic cells properly so called, of variable size, that occur in the subœsophagian centres, in the visceral ganglia, and in the posterior portion of the brain, where they are disposed radially round the central fibrillary mass. The larger cells are near the periphery, the medium-sized and smaller ones in closer proximity to the central mass. Secondly, small spherical cells of uniform size, only met with in the anterior region of the brain (Protocerebron), in the terminal ganglia of the tentacles (both upper and lower) and the ganglion of the external labial nerve, which must be considered as the gustatory nerve.

The number of cerebral nerves, amounting to nine pairs, are constant for the five genera. M. Nabias distinguishes three regions in the cerebral ganglion—protocerebron, mesocerebron and post-cerebron—for the last of which the term metacerebron would have been preferable.

Nullipores.

It is already well-known that the small calcareous marine Algæ, commonly termed "Nullipores" sometimes form great deposits of

massive limestone among rocks of the Tertiary and perhaps the latest Cretaceous periods. Hitherto, however, only insignificant remains of these organisms seem to have been recognised in strata of earlier date. Geologists will, therefore, peruse with much interest the paper by Dr. Alexander Brown on *Solenopora*, published in the April and May numbers of the *Geological Magazine*. *Solenopora* is a problematical calcareous organism of varying size, ranging from the dimensions of a pea to those of an orange, and of irregular shape; and it occurs abundantly in the Ordovician (Lower Silurian) rocks, where it sometimes forms great beds of limestone. Dr. Brown attempts to prove that its structure is cellular, not tubular as previously supposed, and that there are occasionally appearances which can only be compared with the "tetrasporangia" and "conceptacles" of recent corallines. It is thus extremely likely that the problematic fossil in question will prove to be a nullipore, and in that case the range of these minute organisms as rock-builders is extended backwards to early Palæozoic times.

An International Catalogue of Scientific Publications.

In the *Bulletin de l'Académie Royale de Belgique* for April 7, M. Mourlon has a paper on the formation of an International bureau for Bibliography. He refers to the scheme read before the Academy on December 4 last by M. F. Van der Haeghen, for the compilation of a General Catalogue of the contents of public libraries, and proceeds to quote a letter received from the Royal Society of London, dated March 22, 1894, and signed by the secretaries, Professor M. Foster, Lord Rayleigh, and Sir J. Lister.

This letter, which we presume has been sent to other Societies besides the Royal Academy of Belgium, announces the proposed preparation of a Catalogue of all "scientific publications, whether appearing in periodicals or independently," and goes on to say that, as such a "Catalogue is far beyond the power and means of any single Society," the "Royal Society have appointed a Committee to inquire into and report upon *the feasibility of such a catalogue being compiled through international co-operation*." "The Catalogue should commence with papers published on or after January 1, 1900."

This excellent suggestion has been made many times before, and we are glad to hear that the Royal Society of London is taking some practical steps in the matter. But we are curious to learn the names of those forming the committee, that there may be some guarantee of special knowledge in the decisions arrived at. That the Royal Society failed to attain reasonable completeness in their "Catalogue of Scientific Papers" is generally felt, and in their letter referred to this is definitely acknowledged. But the reasons therein given for this failure are lamentably feeble, for they say that this incompleteness is "owing to the omission of the titles of papers published in

periodicals of little importance, or not easy of access." Now, in the first place, it is not for compilers of catalogues to judge whether a periodical is, or is not, of "little importance." The importance of an article, or of a note for that matter, is decided by its importance to the individual who wishes to consult it; it cannot be valued by the clerk who transcribes. In the second place it is the experience of most of those who have to consult periodicals, that they can find in one or other of the great libraries in London practically everything they wish to consult. Judging by our own experience of the "Catalogue of Scientific Papers," published by the Royal Society, the rich collections of scientific serials at Bloomsbury, and the still richer collections stored in the Natural History Museum at South Kensington, could not have been consulted very carefully by the compilers.

Turning to foreign publications of the same nature, we find that the more convenient "Bibliotheca Historico-Naturalis" of Engelmann (1846), the "Bibliotheca Zoologica" of Carus and Engelmann (1860), with the continuation now publishing under the editorship of Taschenberg, reaches a far higher level of completeness in the special subject with which these volumes are concerned. The reason for this is seen in the fact that the German compilers have a special knowledge of the subject upon which they work, and take infinitely more pains to render their work perfect. Only a year or so ago Dr. Taschenberg himself paid a special visit to this country to consult the rarer English publications in order to minimise, as far as possible, the number of omissions.

These catalogues, if they are to be compiled at all, should not suffer from the neglect of periodicals "of little importance or not easy of access," and we sincerely hope that the Royal Society of London, if it attempts the publication of a Catalogue such as it proposes, will use the experience gained by their acknowledged failure, will model their work on Engelmann, Carus, and Taschenberg, both in completeness and size, and make their part of the proposed Catalogue worthy of the subject, and of the money which must be spent upon it.

Records of Scientific Literature.

We have often referred in these pages to those books known as Records of Scientific Literature, which have for object the assistance of the systematic scientist, by cataloguing and arranging the vast flood of papers and books published during the year on scientific subjects. It is our intention, in an early number of Natural Science, to bring together a list of the more important of these valuable time-savers, to show, in the first place, what has been and is being done in this direction, and, in the second place, to bring before the notice of would-be compilers, the importance of some international action as proposed by the Royal Society in

the letter referred to above, in order to save the duplication of work, and the extravagant expenditure of energy and money, which result from the numerous independent publications all tending to the same end. The most striking illustration of the waste in this direction is perhaps seen in the fact that no less than three recognised publications are issued year by year dealing with zoological literature, viz., the *Zoological Record*; the *Zoologischer Jahresbericht*; and the *Zoologischer Anzeiger*. In Geology the case is not at present so bad, for D'Agincourt's *Annuaire* is the only publication embracing, or rather attempting to embrace, the geological literature of the whole world. There are, however, numerous other geological records of local interest, as Blake's *Annals of British Geology*, Nikitin's *Russkaya Gheologicheskaya Biblioteka*, and the *Bibliografia Geologica Italiana* of the Italian Survey. As it has been proved more than once that the scientific public will not support these works, despite their enormous value, it behoves all those interested in the cataloguing of scientific literature, to consider any scheme whereby the necessary catalogue can be produced without the doleful interruptions due to non-support, incompleteness, unpunctuality, or neglect, which have characterised these publications in the past.

BOULDERS AS SOURCES OF ERROR.

IN *Nature* for May 3, Professor McKenny Hughes has an interesting letter on the above subject. He points out that a quantity of rubbish of all descriptions is taken by barges down the lower Thames and spread on the adjoining lands. Thence it is carried on "with road scrapings, fragments of every kind of road metal; with soil turned out in digging foundations, specimens of all the materials used for building; with the contents of middens, and every variety of object of domestic use and ornament." He himself has seen pieces of Napoleonite on the surface in North Kent! Then, with regard to the soils washed out of a cliff of boulder clay, although in the majority of cases there is no doubt as to their origin, Professor Hughes points out that there is another serious source of error, of which he gives a good example.

"A Norwegian vessel, carrying timber from Christiansund to Boston, in Lincolnshire, ran aground and became a total wreck off Old Hunstanton last winter. I saw her in January. The vessel looked sound enough to a landsman's eye; but she was dismasted and gutted, and the salvage was on the sand dunes close by. About her a pool of varying breadth had been formed by the swirl of the water round the hull. The currents had been deflected by various circumstances here and there, as especially where a quantity of ballast had been thrown out. This consisted of large boulders of various kinds of gneiss and porphyry, and the weighty pile looked as if it were little affected by the currents of the incoming and receding tides.

"In April, I visited the spot again, excepting to find that the boulders had been driven along the shore by the fierce storms which had raged along that coast since my previous visit, and intending to make note of their dispersal and the distance to which they had travelled. I found, however, that the keel and a portion of the lower part of the wreck remained, and that the surrounding pool was greatly deepened and extended. Through the deep clear water I saw the heap of ballast, which had been undermined and was settling down into the depths, being already far below the level of the surrounding sand. When the last of the timber shall have yielded to the axe and the waves, the sand will soon level up the hole caused by the scour round the obstructing mass, and this heap of Scandinavian boulders will lie buried in the sand till some exceptional storm shall shift the banks, and expose them again, and perhaps transport them along the shore.

"Had this vessel been thrown on a hard rocky shore instead, the ballast would have started at once with the other boulders on the shore, and been scattered, according to size and form, along the coast. As it was, however, these have got buried deep in sand, and preserved till, perhaps, the habit of using such boulders for ballast shall have been given up, and then, washed out by the accidents of weather, of coast destruction, and of shifting sand, they will appear among the fallen fragments of a boulder-clay cliff, and be appealed to in proof of its origin.

"How many ships with Scandinavian ballast have been wrecked along our eastern coast ever since the time of the Vikings? How many hundred tons of such boulders are still travelling round our shores?"

At a meeting of the Geologists' Association of London, on May 4, Mr. W. W. Watts read a paper on behalf of Mr. A. C. G. Cameron, "On some boulders of chalk in the Boulder-clay of Huntingdonshire." These masses were of large size, and were evidently torn from the original mass, and were in no way "reconstructed" chalk. It was pointed out that one of these masses was considerably more than half-a-mile long, about a quarter of a mile wide, and of unascertained thickness. Upon it, the greater part of the village of Catworth was built, the inhabitants of which derived an abundant supply of water from wells sunk into the mass, this commodity being difficult to obtain by those living on the surrounding surface of boulder-clay. Such a block of chalk as this raises similar interesting questions to the travelled blocks of Professor Hughes. But in this case we cannot invoke the Vikings. As was pointed out at the meeting of the Association, it is difficult to imagine the ice-force necessary to tear away from the parent rock so prodigious a mass of chalk, leaving out of the question the quantity of ice necessary to lift and transport such a mass bodily over the country.

THE SUTHERLAND GOLD-FIELDS.

IT seems likely that the Sutherland gold-diggings will be re-opened in a few months after having been abandoned for twenty-five years.

The facts known about the mode of occurrence and source of this gold appear to be few and elementary. All the gold obtained was alluvial, and the diggings were along three streams—the Kildonan, Suisgill, and Kinbrace burns. These are tributaries, on the eastern side, of the Helmsdale River, which drains a considerable area in N.E. Sutherland. Gold was also found in small rills on the western side, and, as might be expected, in the main river.

The diggings were in terraces of coarse, sandy gravel which skirt the streams, and in the present detritus of their beds, the rock-pockets being the richest. Whether gold was ever found in the parent rock seems uncertain. It may exist finely disseminated in it, and does not appear to be derived from vein-quartz. The country drained by the three burns consists of quartz- and mica-schists traversed by granites, and belonging to that Eastern Highland series over the age and origin of which so much mystery still hangs.

It is not likely that speculation concerning the source of the gold will be very fruitful until many more localities have been searched, and until much more is known of the distribution of the rock-groups in East Sutherland. This we may look for before very long, as the Geological Survey is now at work there. Then, perhaps, some connection between the occurrence of the gold and the nature of the rocks may be found, and light may be thrown on a subject which, though not very important (at least at present) from an economic point of view, has always a certain scientific interest.

WE are pleased to note that the Government of Western Australia has decided to reserve a large tract of public land in the colony as a "National Park" for the preservation of the native fauna and flora. At the suggestion of Mr. Bernard H. Woodward, curator of the Government Museum, an area of 160,000 acres has been selected on the Darling Range, between the Albany and Bunbury roads, midway between Bannister and Pinjarrah, and within a reasonable distance of Perth. It is one of the most picturesque parts of the range, having many serrated mountain peaks, and being exceedingly wild and rugged. It is said to be exactly the site required for the preservation of the kangaroo, with other marsupials, and the emu, of which there are still a certain number left upon it.

I.

Cell-Division.

PART II.

IN the number of NATURAL SCIENCE for January of this year an attempt was made to give a more or less up-to-date account of the karyokinetic method of cell-division. The present article proposes to discuss the phenomenon briefly in its bearing upon reproduction and the segmentation of the ovum, and also to pay some attention to points of theoretical interest in the same connection. Before doing so, however, it will be advisable to consider that other form of nuclear division, viz., Amitosis, or direct division, to which reference was made at the conclusion of the former article. This course is almost necessitated by the fact that recent observations tend to show that amitosis sometimes occurs where theoretical considerations would naturally lead one to expect karyokinesis. If the accuracy of these observations be allowed, it would seem that much that has been written on the significance of karyokinetic division in the transmission of hereditary substance must be reconsidered, if not laid aside as no longer compatible with attested facts.

Amitosis, and its Relation to Mitosis or Karyokinesis.

Amitosis has been stated by many observers to occur normally both in unicellular organisms and in the tissues of higher animals and plants.

Firstly, as regards its occurrence in the Protozoa. The student of nuclear division in these animals must draw a sharp line of demarcation between those forms which possess both a macro- and micro-nucleus, *e.g.*, *Paramæcium*, and those which, like the common amœba, possess only one. In the former instances, the macro-nucleus invariably divides amitotically. Can the same be said of the single nucleus of the latter? Flemming, Ziegler (18), and those who seek to show that amitotic division is invariably a sign of the degeneration of the nucleus that exhibits it, maintain that in those forms which have only one nucleus the division is always accompanied by karyokinesis. Hence, if it can be proved undoubtedly that amitotic division may take place in such forms as *Amœba*, it follows that the view of those authors must be disregarded, at any rate in so far as

it applies to the Protozoa. It is somewhat surprising that little careful investigation of this latter point has as yet been made.

Secondly, as regards the occurrence of amitosis in the Metazoa. Although a few authorities still doubt its existence, it seems clear that, if we are not to disregard a large number of observations, amitotic division of nuclei does actually take place. This fact was first insisted on by Flemming, and the subject has been carefully worked out by Ziegler and others. These observers have shown, in the first place, that nuclei which divide directly are always of relatively enormous size, a condition probably the result of unusually intense secreting or assimilating processes going on within them. For instance, the ovarian cells of many insects act as conveyers of nutriment to the growing ovum. These cells possess large nuclei, which have been observed to break up directly into two or more pieces, although there is no corresponding division of the body of the cell as a whole. Again, the brood-pouches of certain molluscs are lined by cells which pour into them a nutritive fluid. These cells have large, irregularly-shaped nuclei, which undergo rapid amitotic division preparatory to disintegration. Further, it has been shown that the cells lining the mid-gut of insects have the function of gland-cells. They are continually being given off from the inner surface of the gut, and new cells are formed from underneath to supply the place of the old ones. These young cells are formed, in the first instance, by karyokinetic division, but as soon as they become functionally active, their nuclei swell up and divide directly into several pieces. Soon afterwards the cell dies and is thrown off. To take one more instance, this time from the Mammalia, it has been noticed that amitotic division characterises the nuclei of the epithelial cells in the uterine wall preparatory to parturition. Lastly, amitosis is of frequent occurrence in the cells of pathological growths of all kinds. In all these examples it must be noticed that, although nuclear division takes place, there is no corresponding division of the cell-body, and, further, that amitosis is a characteristic of nuclei that have taken on a special function, leading to the speedy death and disintegration of the cell.

It is, however, by no means true that amitosis is never accompanied by cell-division, or that it is always followed by nuclear degeneration and a stopping of cell multiplication. In the spermatogenesis of many animals, it appears that the sperm-mother-cells divide amitotically up to a certain point, after which karyokinesis marks their further division into spermatoblasts. In the development of the spermatozoa of *Branchipus*, Moore (14) has shown that some of the primitive genital cells break down to form a fluid, in which lie the spermatozoa produced by the remainder. The former are the product of repeated amitotic division, the latter of karyokinetic. But the more striking instances of fragmentation of the nucleus are met with in the fertilised ova of many animals. Henking, among others, has made careful investigations on insect

ova (10), and in a large number of cases has shown that the first segmentation nucleus may entirely disappear, and all trace of chromatic or achromatic substance vanish. The chromosomes appear to break up into minute granules, and to become scattered throughout the protoplasm of the ovum, where, by fusion, they apparently, later on, form the "free" or "yolk" nuclei. It must, however, be noticed that no trace of division into blastomeres is to be found till normal karyokinetic division sets in. The chromatin of the first nuclear spindle appears to be the result of a fusion of some of these nuclear granules. There appears, moreover, to be, in insects at any rate, every condition between a regular karyokinetic division of the first segmentation nucleus and an irregular fragmentation of the same. The limits of this article forbid further examination of the evidence in favour of the fragmentation of the nucleus, but it should be mentioned that in the spore-formation of many Protozoa a similar breaking-up of the nucleus takes place, without any trace of karyokinesis. Hickson has, in a recent paper (11), reviewed the literature on the subject, besides giving details of his own observations, and the conclusions he draws appear to be the most probable at the present state of our knowledge. They are:—

1. Fragmentation of the nucleus is a normal method of nuclear division, and is not always a sign of pathological change, or, indeed, of anything abnormal.

2. In many of the instances in which the nucleus is supposed to disappear, there is, as a matter of fact, minute fragmentation of its component parts.

3. Fragmentation of the nucleus is never followed by any kind of corresponding division in the body of the ovum.

It is certain that no hard and fast line can be drawn between mitosis and amitosis, since many recorded observations prove how the one passes almost imperceptibly into the other. There remains, however, the question as to which of the two is the more primitive. To give an answer in the least degree probable is a matter of considerable difficulty, if not of impossibility. In the first place, it is almost necessary to postulate the past existence of non-nucleate organisms, even if it cannot be allowed that there are any such living at the present day. It is equally essential to assume that they must have reproduced themselves in some way or other, either by dividing into equal halves, or by breaking up into irregular pieces. Hence, before the nucleus was developed, there were already forces residing in the cell-substance, tending either to pull it into equal halves or into many pieces. After the appearance of the nucleus in those individuals which had normally multiplied in the former manner, regular karyokinesis would take place, while in the latter case fragmentation of the nucleus could have been the only possible cause. The latter method appears to have become rarer and rarer in the Metazoa, probably because the nucleus took on a more definite

plan of structure, necessitating an exact division of each of its component parts. It is difficult to imagine such a complicated process as karyokinesis arising suddenly in a small nucleated mass of "Urschleim," or primordial protoplasm. It would seem more natural to imagine the nucleus dividing irregularly at first, which division gradually became more and more precise as the importance of the nucleus as an element in the cell increased. It cannot be considered, however, that amitosis, when it occurs in the Metazoa, is a reversion to a primitive condition. There can be little doubt that it is an acquired process, dependent on some special change in the nucleus or cytoplasm.

The Relations of Nuclear Division to Reproduction and Segmentation.

It is well known that the essential process of fertilisation is the conjugation or "fusion" of the head of the spermatozoon, with the nucleus of the ovum. In order to understand the nature of the phenomenon, it is necessary to study the part played by each of the two elements concerned. The head of the spermatozoon, or sperm-nucleus, is practically a mass of chromatin, with nothing resembling the achromatic network of a resting nucleus. The body, or middle piece, is the centrosome, the nature of which was fully discussed in the first part of this article. The tail consists merely of vibratile protoplasm, and either drops off while the spermatozoon is still in the periphery of the ovum, or may persist for a while in the substance of the egg, eventually becoming detached from the head and finally absorbed. The head itself, after penetrating into the egg, may either (*a*) be transformed immediately into a resting nucleus with membrane and achromatic network, a condition it maintains throughout, or (*b*) having first passed into a resting condition, later on its chromatic elements may become collected to form one or more chromosomes, but always a definite number for every species, while the achromatic elements and the membrane simultaneously disappear.

In *Ascaris megalocephala*, the classical object for research on nuclear structures, the sperm-nucleus always possesses a certain number of large chromosomes in the form of threads or rods. The whole process of fertilisation has been most minutely studied by Van Beneden, Boveri, Carnoy, and others. The egg-nucleus is precisely similar in appearance, and in the number of chromosomes it contains, to the sperm-nucleus. To reach this condition, it has had to undergo important changes and losses, caused by the so-called "reducing division," or extrusion of the polar bodies. The significance of these structures has been interpreted in many different ways, some of which will be briefly touched upon later. It is enough at this point to call attention to the fact that, by a reduction in the amount of chromatin it originally possessed, the egg-nucleus has come to exactly resemble in all outward features the metamorphosed head of the spermatozoon. Passing on now to the actual process of fertilisation, Van Beneden

has observed that, in one species of *Ascaris*, both the egg- and sperm-nuclei (male and female pronuclei), contain a single varicose chromatin thread. As they approach one another, the thread in each divides transversely into two halves, both of which become looped and again divide, this time by longitudinal splitting, into four apparently identical pieces. The two nuclei then come into close contact, and the eight threads, from male and from female, arrange themselves in the equatorial plane of a spindle, which has by this time been formed by the activity of the centrosome brought into the ovum by the spermatozoon. The threads gradually travel towards the poles of the spindle, and a separation is effected between them in such a way that each of the daughter-nuclei is formed out of four chromosomes, two derived from the female and two from the male. Constrictions now appear, dividing the ovum into two blastomeres, the nuclei of which have by this time passed into a resting condition. It will be seen from the foregoing account that there is no true fusion of the chromosomes of the two pronuclei, but only a mingling together of their substance. Indeed, the notion that in the fertilisation of any ovum the two nuclei actually fuse to form one, or that any part of them do so, is gradually being given up.

There still remain to be considered those cases in which the two nuclei copulate while in a typically resting state. The process of fertilisation in the leech *Nephelis*, which the present writer has had the opportunity of studying, affords a good case in point. Both pronuclei are in a state of perfect rest during the whole time; all that appears to happen is the breaking down of the two membranes of the adjacent nuclei at the point of contact, allowing a mingling of the chromatic and achromatic elements, which results in the formation of a single large nucleus. This also remains some time in a state of rest before it proceeds to karyokinetic division.

It would seem, therefore, necessary to distinguish those cases in which the two pronuclei fuse, so as to give rise immediately to a nuclear figure, from those in which copulation takes place between two resting pronuclei to form a single nucleus in the same condition. What the significance of these two modifications may be is not clearly understood.

Nuclear Division in Relation to Points of Theoretical Importance.

Since the nuclear matter is so obviously concerned in the process of fertilisation, it is legitimate to hold that the nucleus is the seat of the hereditary substance which the parent transmits to its offspring. Strasburger and O. Hertwig were among the first to advocate this view, and it has now come to be generally accepted that only the chromatin of the nucleus is the bearer of the hereditary tendencies. This view is supported by the fact that paternal characters can be inherited just as well as maternal, although the amount of protoplasm in the two germ-cells is greatly disproportionate.

Further, the persistent part of the spermatozoon is practically nothing more than a mass of chromatin and a centrosome. The latter is, without doubt, merely an organ for division, although one author has recently tried to show that the centrosome is the bearer of parental qualities to the entire exclusion of the chromatin substance. If it be granted, therefore, that the chromatin is the bearer of the hereditary tendencies, it is of the greatest importance to trace out the changes which the chromatin of the egg- and sperm-nuclei undergo preparatory to copulation.

Mention has already been made of the "reducing division" of the nuclear matter of the egg-cell by means of the polar bodies. These structures have been noticed for some considerable time, and there has been much speculation as to their real nature. The following are some of the more important suggestions which have been made in order to explain their presence:—

1. The earlier authors considered them to be excretory products which the ovum had to get rid of.

2. Kölliker regarded them as a means of rendering the egg-nucleus the same size as the sperm-nucleus.

3. Flemming thought they represented a parthenogenetic mode of development possessed by the ova of earlier ancestors.

4. Hertwig and Boveri look upon the formation of the polar bodies as nothing more than unequal cell-division, strictly comparable to what happens in the formation of the spermatozoon. Boveri has constructed two diagrams, reproduced here, to illustrate this point (3), and has invented a new nomenclature for the germ-cells in the various stages of their development. From an inspection of these diagrams it will be seen that the first polar body corresponds to a sperm-mother-cell, while the second corresponds to a spermatozoon. While the diagrams illustrate strictly the number of divisions which the ovocyte I. and spermatocyte I. undergo, viz., two, there are more series of cell-divisions than represented in the upper part of the diagram for the zone of germination. These diagrams will be subsequently referred to in considering the fate of the chromosomes in the reducing division.

5. Minot and Balfour, as is well-known, regarded the polar bodies as representing outcast male elements, and postulated a somewhat similar process taking place in the formation of the spermatozoon, by which corresponding female elements were got rid of. Thus, these two authors considered the primary germ-cells to be essentially hermaphrodite.

6. Weismann sees quite a different significance in the polar bodies. According to his earlier view, nuclear matter presents two modifications, viz., germ-plasm and histogenic nucleoplasm. The former is only found in the germ-cell, and is the hereditary substance passed on from parent to offspring. The latter is the element which determines the nature and directs the growth of the cell which contains it.

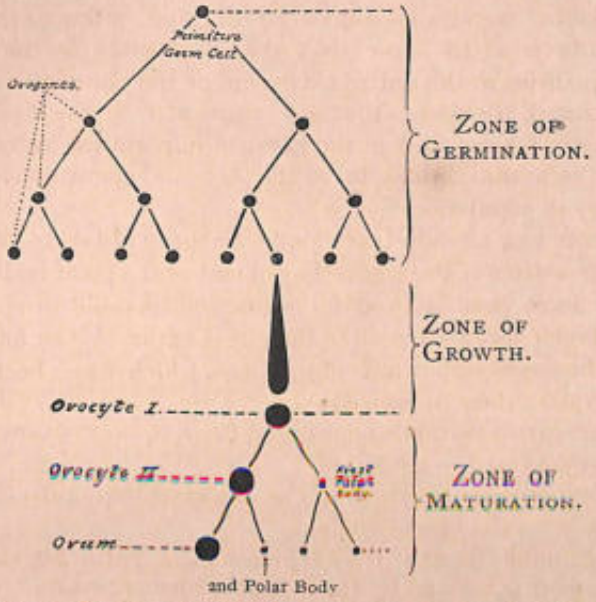
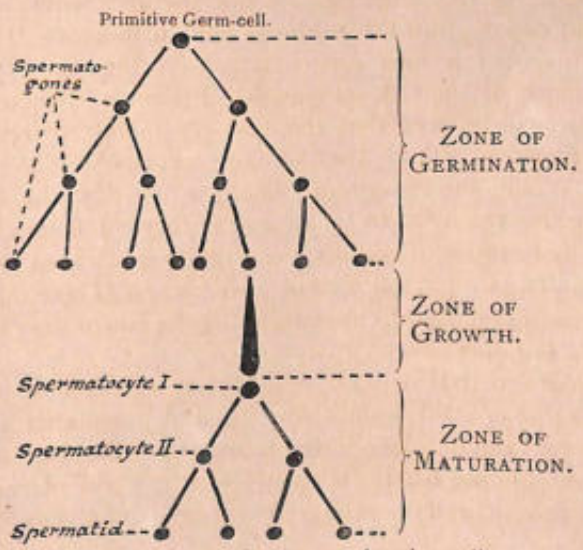


Diagram Showing the Development of the Egg-Cells in Ascaris megalocephala (*after Boveri*).



Spermatocyte I. = Sperm-grandmother-cell.
Spermatocyte II. = Sperm-mother-cell.
Spermatid = Spermatozoon.

Diagram Showing the Development of the Sperm-Cells in Ascaris megalocephala (*after Boveri*).

Thus, the nucleus of a nerve-cell would contain a different kind of histogenic plasm from that of a gland- or muscle-cell. As soon as the ovum has arrived at maturity, its own histogenic plasm has become superfluous, and is therefore got rid of in the shape of polar bodies. When, however, it became known that, whereas in sexually reproducing ova two polar bodies were extruded, in those which developed parthenogenetically there was never more than one,[1] Weismann was forced to attach another meaning to the second polar body. This he did by suggesting that it was a means of keeping the number of ancestral units in germ-plasm from increasing; were there not some such method the germ-plasm would be doubled at every fertilisation, and soon grow too large to be contained in the nucleus. It is possible that this idea was based on certain erroneous observations as to the exact method of nuclear division in the formation of the second polar body. The point is far too complex to be entered upon here, but it has been adequately discussed, and criticised by Boveri (3) and others.

After the publication of Hertwig's (10) admirable researches on the spermatogenesis of *Ascaris*, Weismann considerably modified his views. It was shown, or at any rate Weismann has gathered from Hertwig's work, that the youngest sperm-mother-cells possess nuclei which are characterised in the active stage by the presence of four chromosomes. These cells (spermatogones, to use Boveri's nomenclature) increase by ordinary karyokinetic division, *i.e.*, longitudinal splitting of their chromosomes up to a certain point. When this is reached, the cells cease to multiply, but grow considerably in size, and their nuclei pass into the resting condition. On again becoming active, the nuclei are seen to possess not four rods as heretofore but eight. At this point the reducing division sets in. Each cell (spermatocyte I.) divides into two (spermatocyte II.), each of which again divides, forming two daughter-cells, which are the true spermatozoa (Spermatids) (*see* diagram). It must be noticed, however, that in both these latter divisions there is no longitudinal splitting of the chromosomes, but half the number of rods pass into the daughter-nuclei. Since there are two reducing divisions, it will be seen that each spermatozoon eventually comes to possess two rods, *i.e.*, half the number contained by the original spermatogone. The process in the formation of the ovum is exactly similar to the above, the sole difference being that, whereas in the male cells the reducing division of the spermatocyte I. gives rise to four equal spermatozoa, that of the ovocyte I. gives rise to three polar bodies[2] and the ovum, of which only the last is functionally active. Therefore, the ovum before the extrusion of the polar bodies is the homologue of the sperm-grandmother-cell or spermatocyte I.

[1] Blochmann, however, has described two polar bodies in the parthenogenetic ova of *Apis* and *Liparis*, and Brauer has shown that the same is the case in the parthenogenetic eggs of *Branchipus* and *Artemia*.

[2] The first polar body divides into two, making three in all.

This reducing division has excited great attention among biologists, and scarcely a week passes without some new contribution to the already extensive literature on the subject. It is only possible here to give the briefest mention of the view that Weismann now holds. This author considers the nuclear chromatin rods, or "idants," to be composed of individual masses of ancestral plasms, or "ids," each of which could, if it alone dominated the ovum, control the whole course of development, and give rise to a complete individual of the species. The doubling of the idants before the reducing division is a means of increasing the number of possible combinations among the ids, and of thereby ensuring that as many spermatozoa as possible shall possess the hereditary tendencies balanced in different proportions. By this means, which equally well applies to the ovum, the organism resulting from the union of two individuals may differ markedly from its parents, or, in other words, may exhibit considerable variation from the specific type.

Before leaving the subject of the reducing division, it should be noticed that a similar process occurs in the Protozoa. Maupas has shown that the micro-nucleus of the Ciliata, which alone takes any part in conjugation, undergoes as many as three reducing divisions before it is ready for action. In some lowly-organised plants (Desmidiacea) reducing division takes place after two individuals have fused to form a zygote.

[1] Lastly, Boveri has carefully traced the method of nuclear-division in the segmenting ovum of *Ascaris*, and finds that large masses of chromatin may be extruded bodily from the cells. Further, he has discovered that the ovum divides, at its first segmentation, into a somatic-cell and a germ-cell. The difference between them is marked by the way their nuclei divide. Whereas the former cell and its descendants divide in a more or less rough and ready manner (though always karyokinetically), and masses of chromatin are extruded bodily from the cell, the latter divides, with the exactest halving of its rods, into two, one of which halves repeats the division of its parent, while the other segments in a manner similar to the somatic cells. Thus, up to a certain point in ontogeny, there is in the embryo, at any given period, one cell which is a product of perfect karyokinetic division. Beyond this stage all the descendants of this cell divide similarly to it, and give rise to the germ-cells. Thus there is a perfect continuity of the nuclear matter from the two-celled stage in development up to the ripe sexual elements of the adult. Boveri's results are, therefore, a striking proof of Weismann's theory of the continuity of the germ-plasm, as far as outward form goes.

The above account deals with some of the main points in which cell-, and more particularly nuclear, division are of importance, as regards both the actual process of fertilisation and development. It is naturally impossible to give any clear idea of Weismann's elaborate theories based on nuclear structure and the changes it under-

goes; but it was felt that it would be equally undesirable to entirely pass over all theoretical speculations. Without some connecting idea, the facts themselves lose much of their interest, and biologists must of necessity pay close attention to such work as Weismann's, even if they have to find subsequently that some new discovery renders that work no longer tenable as affording an adequate explanation of the facts as they stand. Though to some minds Weismann's doctrines may extend into the regions of transcendental science, yet his works, and those of the Hertwigs, Boveri, and other eminent cytologists, have brought home to all an immense and varied field of possible research, the results of which may in the future, as they have done in the past, profoundly modify the position of Biological Science.

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M. D. HILL.

II.

Can the Sexes in Ammonites be Distinguished?

ALL living Cephalopoda known to us have the sexes separate; no hermaphrodite species has been discovered. It is therefore natural to suppose that the extinct Ammonites were similarly unisexual, some specimens being male, others female. If this be admitted, it becomes of some importance to ascertain whether the difference of sex affected the hard as well as the soft parts of the animal, and, conversely, whether certain differences of form that distinguish individuals may not be of sexual rather than of varietal or specific nature. Clearly it would be wrong to give separate specific names to individuals whose only difference was that of sex.

The opinion that such secondary sexual characters are manifested in Ammonite shells has long been maintained by many eminent authorities, notably among French palæontologists. The first to suggest it seems to have been Ducrotay de Blainville, in his "Prodrome d'une monographie d'Ammonites," Paris, 1840. "Unknown," he said, "though the Ammonite animal be, yet analogy drawn from what we know of the Nautilus does not allow us to doubt that the sexes were separate in different individuals. The females, then, must have had the ventral side more swollen by the ovaries replete with eggs at a certain period. Hence the shell and its aperture must have betrayed these differences by a size and swelling in the former and a relative width in the latter, both greater in female individuals than in those of the male sex, since the semen of the males could never reach such a development as the fertilised ova of the females." De Blainville was followed by d'Orbigny and many other palæontologists. Pierre Reynès, in the Monograph cut short by his untimely death (1879), strongly upholds the distinction of the sexes, saying—"The difference in the dimensions of shells equally ornamented seems to be one of the most convincing proofs. Among individuals, some are slight and of compressed shape, while the whorls of others are broad; we think that the first are the males and the second the females. . . . It results from the comparison of a certain number of individuals of the same species that, without any

doubt, among Ammonites, the compressed forms are almost always accompanied by swollen forms, and that these two forms are constantly associated. The ornamentation and septa are identical; the individuals only differ in the involution and width of their whorls. Anyone making a general study of Ammonites can easily convince himself that the majority of species have two distinct forms, and can prove this whenever the material is sufficiently plentiful. To what cause can this difference of thickness be attributed if not to sex? Ornament is often more pronounced in one sex than in the other, although identical in general arrangement. The ribs and tubercles are then more marked and their number is less; in this case the furrows have a greater depth (*A. bifrons*, *Murchisonæ*, *Martiusii* [*sic*], *Truellei*, *Duncani*, etc.). In other species the ornament is in no way changed, the whorls preserve their ordinary proportions, and are not modified in any of their parts."

That differences, not of specific importance, do exist between individual shells, will not be denied by anyone who has studied large collections of Ammonoidea; and these differences may be due to sex, with at least as much probability as to any other cause. The smaller size of the male is certainly paralleled among recent Cephalopoda, *e.g.*, *Sepia*, *Sepiola*, *Loligo*, *Argonauta*. For the views enunciated by Reynès and the other palæontologists mentioned above, something might perhaps be said within small limits; but modern research has done much to disprove them. First, the supposed males and females are found to be not always contemporaneous when the strata are critically examined. Next, the thick and thin forms of a "species" are replaced in modern collections by a great number of forms in all degrees of compression, and these forms do not exhibit that strict agreement in details of ornamentation, etc., which we should expect to find after reading the accounts of these authors. Finally, it has yet to be proved that the thicker forms did actually possess body-chambers of larger capacity than their thinner relatives. It would be so, of course, if the length remained the same; but our investigations rather indicate that the length varies with the degree of compression, so as to maintain, among relatives, the same size of body-chamber in proportion to the whole shell. For instance, in *Stephanoceras* the body-chamber varies in length from about half a whorl in the thick forms (the supposed females) to very nearly two whorls in the thin forms (the supposed males). If the "female" had to enlarge her body-chamber for the accommodation of the eggs, it is obvious that her body-chamber ought to be larger in proportion to the whole shell than is the body-chamber of the "male," who was not called upon to provide this extra house-room.

Recent writers on the subject have therefore gone upon another tack. In their over-anxiety to obey the behest "cherchez la femme!" two well-known French authors have set themselves to prove that the two sexes in the Ammonites differ not only in compression, but in

ornamentation, in shape, in size, in septation, and in the processes at the shell-aperture.

First comes Munier-Chalmas, who, in the *Compte rendu de la Société géologique de France* (no. 14, p. 170, 1892), explains his views on the subject. He considers that the males among Ammonites are represented by certain "scaphitoid" forms, as he calls them, which are distinguished by the following characters:—1. The last whorl is more or less bent back (*réfracté*). 2. The edge of the shell aperture (*péristome*) is furnished with a double process—one auricle on each side —(*apophyse jugale*). 3. The size is relatively small. 4. The evolution of the septa is rapidly arrested.

As types of these *Scaphites*-like Ammonites, Munier-Chalmas indicates the following genera with which we are already acquainted:—*Œcotraustes*, Waagen; *Œcoptychius*, Neumayr; *Sutneria*, Zittel; while he himself adds, as new genera, *Cadomoceras*, of which the type-species is *Ammonites cadomensis*, d'Orb.; *Horioceras*, with type-species *A. baugieri*, d'Orb.; and *Creniceras* which has for type *A. renggeri*, d'Orb. The species belonging to these forms are considered by Munier-Chalmas to be the males of other species, which are females. Thus, *Œcotraustes genicularis*, Waagen, is assigned as male to *Oppelia subradiata* (Sow.); *Horioceras baugieri* (d'Orb.) is made to ally himself with *Distichoceras* (n.g.) *bipartitum* (Zieten); and so on. Why, if these species really are males, generic names, none too happily invented should be given to them, it is not easy to understand. Surely, among Ammonites, as among higher orders of beings, husband and wife should be known by the same name.

This author also treats of the group of Ammonites allied to *A. humphriesianus*, under the name *Cæloceras*. This genus he divides into males, distinguished by an "apophyse jugale," and called *Normannites*; and females, which have no such "apophyse" and are called *Cadomites*. Thus he links the following males and females:—

Cæloceras (*Normannites*) *bigoti* to *C.* (*Cadomites*) *bigoti*.
" " *sauzei* to " " *polyschides*.
" " *braikenridgii* to " " *subcoronatum*.
" " *linguiferum* to " " *daubenyi*.

The example set by Munier-Chalmas has recently been followed by E. Haug, in a paper on "Ammonites des Etages moyennes" (*Bull. Soc. Géol. France*, 3e ser., t. xx., p. 298). This author has applied the same criteria to the genus *Sonninia*, and has come to the conclusion (p. 300) that the normal *Sonninia* are the females, and that the group of *Sonninia sulcata*, which contains forms not unlike *Ammonites cycloides*, consists of males. He admits that the species of females far outnumber those of males, but expresses a hope that the missing husbands will eventually be found.

Let us now consider how far these somewhat extreme views, which if true would lead to great complications, are consistent with already known facts.

An objection that might strike even the casual reader of these papers is that all the Lias and much of the Inferior Oolite had been passed through before any of these so-called males appeared; for the earliest are those which Haug finds among the *Sonniniæ*. In the Ammonite fauna of the Lias we have as yet only been able to notice one species possessing the characters supposed by Munier-Chalmas and Haug to denote the male sex. That species is *Ammonites accipitris*, J. Buckman, which, as it occurs with *Ammonites oxynotus*, would be considered as the male of that species. Now *Ammonites accipitris* is, unfortunately for this hypothesis, a very rare form, which seems only to have been found in this country, and in one restricted locality. *Ammonites oxynotus*, however, is common enough on the Continent, where it must have lived in a state of single blessedness. But as for the rest of the Liassic Ammonites, where are the males? Or are we to suppose that the two sexes were exactly alike, and that secondary sexual characters were a comparatively latter-day Jurassic invention?

Objections such as these suggest the enquiry whether there may not be some other explanation of the appearances that the French authors are so ready to ascribe to sexuality. As that remarkably Heavenly Twin, Angelica Hamilton-Wells, reminds us, "so unwholesomely is the imagination of a man affected by ideas of sex."

Now the characters that Munier-Chalmas has hit upon as "masculine" are in great measure those which appear in the different Ammonite-stocks or lines of descent when they draw near the period of their extinction. They are, to a large extent, characters such as we have learned to associate with the old age of a race, when it is in a sense retrogressive. May we not then suppose that these so-called males are in reality the final expressions of the various races to which they belong? At all events, *Cadomoceras*, *Œcotraustes*, and *Œcoptychius* seem explicable as such retrogressive forms; but *Normannites* is admittedly a difficulty.

The sexual hypothesis, however, is one about which it would be foolish to dogmatise; it deserves full consideration. Munier-Chalmas and Haug say that they have many males yet unfigured; and this statement is borne out by other collections, so far as the Oolitic rocks are concerned. It must be remembered that our knowledge of the Ammonite fauna is as yet very imperfect, large though the number of described species appears to be. Some of the Jurassic rocks have been done very scant justice to, and their treasures are still unknown to Ammonite literature.

Apart, however, from any special hypothesis, there is one consideration that may profitably be brought to the notice of those who endeavour to discover signs of sex in the extinct Cephalopods. They may be recommended to enquire whether such signs are to be found in the living species of *Nautilus*, and if so, what those signs are. Of course *Nautilus* is not an Ammonite, does not even belong to the same Order of Cephalopoda; still there can be very little doubt that

the situation of the animal with regard to the shell was much the same in the Ammonites, and that any effect produced in the Ammonite shell by sexual difference would probably also be found in that of *Nautilus*.

Owing to the somewhat curious fact, which may of course be mere coincidence, that the large majority of specimens of *Nautilus* examined by eminent naturalists have been of the female sex, not much has been said upon this subject by zoologists. J. D. Macdonald, however, in his account of the anatomy of *Nautilus umbilicatus* (*Phil. Trans.*, 1855, p. 277), has a sentence which, though its logic is obscure, certainly bears on this question. He writes: "The specimen of *N. umbilicatus* examined proved to be a female, a fact which may serve to modify the views of those who, adopting the ingenious speculations of d'Orbigny with reference to the sexes of the Ammonites as indicated by the characters of their shells, apply them also to the several kinds of *Nautili* known." He appears to be alluding to the belief that the species known as *N. umbilicatus* was nothing more than the male of *N. pompilius*. Still the fact that it was really a true species, though it might upset that particular belief, would not conflict with the idea that there might be some differences between the shells of the two sexes.

An attempt to demonstrate some such difference was made in the same year by J. Van der Hoeven (*Verh. d. kon. Nederl. Akad. van Wetensch.*), who had at last obtained a male *Nautilus pompilius*. After describing various external sexual characters of the animal, he proceeds:—"It seems to me moreover as not improbable that in the shape of the hood there is a sexual distinction, and that this, when of the same average length, is about two centimetres shorter in the male individual. With this is also connected a distinction in the shape of the shell; in the male individual this is broader and more globose at the aperture, more laterally compressed in the female animal. Also the edge of the aperture of the shell in the male animal is, as it seems to me, more strongly sinuous; in the female animal more regular." It will be noted that these points of difference, assuming them to be correct, by no means agree with the suppositions of de Blainville, d'Orbigny and others with regard to Ammonites; and in such a case surely no one will venture to exclaim "So much the worse for the *Nautilus!*" Moreover, if one may judge from the drawings by A. G. Bourne which adorn Professor E. Ray Lankester's article on Mollusca in the "Encyclopædia Britannica," both the male animal and its shell are proportionately broader than the female. It is indeed by no means necessary to suppose, as did the older writers, that the swelling of the female organs involves a widening of the shell. The shell of the body-chamber must necessarily have been formed before the development of the ova, and any pressure caused by the enlargement of the soft parts must act on the soft parts themselves quite as much as on the shell. Since, however, the shell is hard

and unyielding, the obvious result of such pressure is merely to push the animal forward so that it obtains space by lengthening rather than by widening the body-chamber.

Some caution is, however, to be exercised before accepting the views which Van der Hoeven based upon the evidence of a single specimen. Differences such as he points out do certainly exist between *Nautilus* shells; but a careful examination inclines one to regard them as due to difference of age rather than of sex. As *N. pompilius* grows older, the sides of the shell-aperture begin to swell outwards, so that the width of the aperture is greater towards the inner or dorsal region of the whorl; and this produces a sinuosity in the margin, which continues to increase with age up to a certain point. May it not then be that Van der Hoeven was deceived by just the same kind of characters in the individual species as we believe Munier-Chalmas and Haug to have been deceived by in the race?

To sum up the case in language that has often been objected to in this Journal, and with which its readers cannot, therefore, be wholly unfamiliar,—we suggest that the characters in question are auxologic or bioplastic rather than sexual, being in some cases phylo-gerontic, in others merely ephebic or gerontic, and we conclude that sexual dimorphism has yet to be proved for Ammonite-shells.

S. S. BUCKMAN
F. A. BATHER

III.

The Palæarctic and Nearctic Regions compared as regards the Families and Genera of their Mammalia and Birds.

IN a paper read before the Cambridge Natural Science Club on March 12 (and printed in *Nature* of April 26, 1894), I discussed the question of the nature and uses of Zoological Regions, and arrived at the conclusion that, in order to secure the maximum of utility, it is essential to have only one set of regions for all groups of animals; and, further, that the six regions established by Dr. P. L. Sclater are the most natural and convenient, and are best adapted to facilitate the comparative study of distribution, which is the main purpose for which Zoological Regions, as distinct from the ordinary geographical divisions of the globe, have been established.

These regions were at first generally adopted; but of late years many eminent naturalists, both in America and Europe, have proposed other divisions, though hardly any two of these agree with each other. The most important modification, and that which has been adopted by several zoologists both as regards mammals and birds, is to unite the Palæarctic and Nearctic Regions so as to form one new region, coextensive with the extratropical Northern Hemisphere. This new region, which has been termed the Holarctic, is said to be more nearly equal to the other regions as regards peculiar genera and families, and, therefore, to form part of a more natural and harmonious series than if we treat the two component parts as separate regions. It is this one question only that I propose to discuss in the present paper: whether the Palæarctic and Nearctic Regions, as defined by Sclater, present so many resemblances and so few differences that they can be reasonably considered to form one region as homogeneous as are most of the other regions.

The reason why so much difference of opinion exists on this point, when the very same facts are before all the enquirers, seems to be that they treat the facts in different ways. In the first place, it seems to me that far too much stress is laid upon the comparatively small number of absolutely peculiar genera or families in the two temperate

regions. That may, or may not, be a reasonable ground for deciding that the regions in question are not of equal rank with some other regions; but it cannot justify the union of the two areas unless it can be shown that they are to a large extent homogeneous—that, in fact, the resemblances are more numerous and more important than the differences. In the areas we are discussing, however, this is decidedly not the case, since the facts clearly show that the differences very far surpass the resemblances—that the former are, in fact, fundamental, and are far greater than can be found in the separate halves of any of the other regions, unless they are so divided as to be very unequal in area or to present very great differences in climate. But the Palæarctic and Nearctic Regions are, roughly speaking, equal in area, while they both exhibit the very same range of climate. Any difference that exists between them must, therefore, be due to more fundamental causes; and the most fundamental cause is, that in each case the existing fauna is descended from an ancient, equally distinct, fauna, modified in different ways by immigration from adjacent areas.

In dealing with any question of this kind, it is very important that the genera and family groups adopted shall be the same as far as possible throughout the whole area. In order to ensure this, I have, for mammalia, adopted those of Flower and Lydekker in their "Mammals Living and Extinct." In the case of the birds, am indebted to my friend Mr. H. E. Dresser, both for completing his "List of European Birds" so as to include those of the wlole Palæarctic region, and also for so harmonising his genera with tbse of the "A. O. U. Check List of North American Birds," as to combine the whole into a single series. As Mr. Dresser has made a study of North American birds as well as of those of the Palæarctic Region with which his name is more especially associated, the resu is probably as fair and unbiased, for the comparison in question, as can be arrived at. In the case of both regions, I have rejected all those genera in which a single species only just enters the region from the adjacent region to which it properly belongs. Here, again, there is, of course, room for difference of opinion; but the doubtful cases are not sufficiently numerous to introduce any important uncertainty in the result.

For both mammals and birds I adopt the same system of presenting the facts which, after much consideration and many trials, seems to me to be the best. The families and genera are arranged in three columns—the first giving those which are Palæarctic but not Nearctic; the third those that are Nearctic but not Palæarctic; the middle column giving those which are common to the two regions—so that by combining this with either of the others we obtain the entire fauna of that region. The results are then summarised in other tables and briefly discussed. We begin with the mammals:—

Mammals of the Palæarctic and Nearctic Regions.

PALÆARCTIC.	BOTH REGIONS.	NEARCTIC.
	Bats.	
Rhinolophidæ:		
1. Rhinolophus.		
Vespertilionidæ:	*Vespertilionidæ*:	*Vespertilionidæ*:
2. Synotus.	1. Plecotus.	1. Antrozous.
3. Harpyiocephalus.	2. Vesperugo.	2. Nycticejus.
	3. Vespertilio.	3. Atalapha.
	Emballonuridæ:	*Phyllostomidæ*:
	4. Nyctinomus.	4. Macrotus.
	Insectivora.	
Erinaceidæ:		
4. Erinaceus.		
Soricidæ:	*Soricidæ*:	*Soricidæ*:
5. Crossopus.	5. Sorex.	5. Neosorex.
6. Crocidura.		6. Blarina.
7. Diplomesodon.		
8. Anurosorex.		
9. Nectogale.		
Talpidæ:	*Talpidæ*:	*Talpidæ*:
10. Myogale.	6. Urotrichus.	7. Scalops.
11. Talpa.		8. Scapanus.
		9. Condylura.
	Carnivora.	
Viverridæ:	*Felidæ*:	*Procyonidæ*:
12. Genetta.	7. Felis.	10. Procyon.
13. Herpestes.		11. Bassaris.
Hyænidæ:	*Canidæ*:	
14. Hyæna.	8. Canis.	
	Ursidæ:	
	9. Ursus.	
Mustelidæ:	*Mustelidæ*:	*Mustelidæ*:
15. Meles.	10. Lutra.	12. Latax.
	11. Mustela.	13. Mephitis.
	12. Gulo.	14. Taxidea.
	Trichechidæ:	*Otariidæ*:
	13. Trichechus.	15. Otaria.
Phocidæ:	*Phocidæ*:	*Phocidæ*:
16. Halichærus.	14. Phoca.	16. Macrorhinus.
17. Monachus.	15. Cystophora	
	Ungulata.	
Suidæ:		*Dicotylidæ*:
18. Sus.		17. Dicotyles.
Cervidæ:	*Cervidæ*:	*Cervidæ*:
19. Moschus.	16. Cervus.	18. Cariacus.
	17. Rangifer.	
	18. Alces.	
		Antilocapridæ:
		19. Antilocapra.

PALÆARCTIC.

Bovidæ:
20. Saiga.
21. Pantholops.
22. Gazella.
23. Addax.
24. Rupicapra.
25. Budorcas.
26. Capra.

Equidæ:
27. Equus.

Myoxidæ:
28. Myoxus.
29. Eliomys.
30. Muscardinus.

Muridæ:
31. Gerbillus.
32. Cricetus.
33. Ellobius.
34. Siphneus.
35. Mus.
36. Acanthomys.

Spalacidæ:
37. Spalax.
38. Rhizomys.

Dipodidæ:
39. Sminthus.
40. Dipus.
41. Alactaga.
42. Platyceromys.

Hystricidæ:
43. Hystrix.

BOTH REGIONS.

UNGULATA.—*continued.*

Bovidæ:
19. Ovis.
20. Bos.

RODENTIA.

Sciuridæ:
21. Sciurus.
22. Tamias.
23. Sciuropterus.
24. Arctomys.
25. Spermophilus.

Castoridæ:
26. Castor.

Muridæ:
27. Arvicola.
28. Myodes.
29. Cuniculus.

Lagomyidæ:
30. Lagomys.

Leporidæ:
31. Lepus.

MARSUPIALIA.

NEARCTIC.

Bovidæ:
20. Haplocerus.
21. Ovibos.

Sciuridæ:
22. Cynomys.

Haploodontidæ:
23. Haploodon.

Muridæ:
24. Hesperomys.
25. Sigmodon.
26. Ochetodon.
27. Neotoma.
28. Phenacomys.
29. Synaptomys.
30. Fiber.
31. Neofiber.

Geomyidæ:
32. Geomys.
33. Thomomys.
34. Diplodomys.
35. Perognathus.
36. Heteromys.

Dipodidæ:
37. Zapus.

Hystricidæ:
38. Erethizon.

Didelphidæ:
39. Didelphys.

Looking through the preceding lists, we find a remarkable divergence in the characteristic groups of the two regions. Out of fifteen genera of Insectivora, only two are common to both regions; an amount of difference which, if it occurred among larger and better known animals, would produce a striking effect of diversity. In the Carnivora there is a much smaller proportion of peculiar genera, but this is to some extent counterbalanced by each region possessing two families absent from the other. Ungulata, again, show considerable diversity, the Palæarctic Region possessing ten genera and the Nearctic five which are not found in the other. Only five genera are common to both, and of these five, two, Rangifer and Alces, are arctic, while two others, Ovis and Bos, have one species each in the Nearctic Region against about thirteen between them in the Palæarctic. The Rodents, again, have only eleven genera common to both regions, while the Palæarctic has sixteen and the Nearctic seventeen which are not found in the other region.

The following summary will enable us to see the total amount of similarity and difference:—

SUMMARY OF DISTRIBUTION.

Orders.		Families.			Genera.		
	No. of Genera.	*Palæarctic.*	*Both.*	*Nearctic.*	*Palæarctic.*	*Both.*	*Nearctic.*
ptera ..	11	1	2	1	3	4	4
vora ..	15	1	2	0	8	2	5
ivora	22	2	6	2	6	9	7
Vive ilata	20	2	2	2	10	5	5
ntia	44	2	7	2	16	11	17
supialia ..	1	0	0	1	0	0	1
Hy	—	—	—	—	—	—	—
		8	19	8	43	31	39
		—	—	—	—	—	—
otal Genera ..	113	—	—	—	74	—	70
Total Families ..		27		27			

Here we see that in both the regions the number of genera *not* found in the other largely exceed those common to both, the proportion of the total genera thus limited being 58 per cent. in the Palæarctic, and 56 per cent. in the Nearctic Region; while in each case out of a total of 27 families no less than 8 are so limited. If we compare this amount of diversity with that between the Ethiopian and Oriental Regions, we shall find that, while as regards genera it is somewhat less, as regards families it is considerably greater. In the Oriental Region out of a total of about 72 families of mammals only 8 are not Ethiopian; while in the Ethiopian region out of about 74 families only 10 are not Oriental. No doubt these two regions are those which most resemble each other, and they have been united by some naturalists, but they are now generally admitted to be sufficiently distinct to be classed as separate regions. Great weight has, however, always been given to the possession of peculiar families; and for the same reason the fact that nearly one-third of the families in the

Palæarctic and Nearctic regions respectively are *not* found in the other, indicates a fundamental diversity that renders their union into one region quite inadmissible.

We will now tabulate the birds, in which the amount of diversity is much larger as regards genera, and equally as large as regards families:—

LAND BIRDS OF THE PALÆARCTIC AND NEARCTIC REGIONS.

PALÆARCTIC.	BOTH.	NEARCTIC.
Turdidæ:	*Turdidæ*:	*Turdidæ*:
1. Monticola (4 sp.)	1. Turdus (21.5)	1. Merula (2)
2. Myiophoneus (1)	2. Sialia (1) (= Grandala (3)	2. Hesperocichla (1)
3. Saxicola (20)	3. Regulus (4.3)	3. Polioptila (3)
4. Pratincola (7)		4. Myadestes (1)
5. Ruticilla (15)		
6. Hodgsonius (1)		
7. Chimarrhornis (1)		
8. Cyanecula (2)		
9. Erithacus (4)		
10. Calliope (3)		
11. Cossypha (1)		
12. Nemura (1)		
13. Daulias (2)		
14. Sylvia (18)		
15. Melizophilus (2)		
16. Phylloscopus (19)		
17. Hypolais (8)		
18. Ædon (2)		
19. Acrocephalus (10)		
20. Lusciniola (10)		
21. Locustella (8)		
22. Cettia (4)		
23. Cisticola (1)		
24. Drymoeca (1)		
25. Scotocerca (2)		
26. Argya (3)		
27. Rhopophilus (3)		
28. Babax (1)		
29. Pterorhinus (1)		
30. Trochalopteron (2)		
	Cinclidæ:	
	4. Cinclus (4.1)	
Accentoridæ:		
31. Accentor (11)		
Panuridæ:		
32. Panurus (1)		
Paridæ:	*Paridæ*:	*Paridæ*:
33. Leptopœcilus (2)	5. Parus (26.6)	5. Chamæa (12) (sub. fam.)
34. Lophobasileus (1)	6. Lophophanes (3.4)	
35. Suthora (1)	7. Acredula (7) = Psaltriparus (13)	
36. Ægithalus (4)		
	Sittidæ:	
	8. Sitta (8.4)	

PALÆARCTIC.	BOTH.	NEARCTIC.
Certhiidæ:	*Certhiidæ*:	
37. Tichodroma (1)	9. Certhia (2.1)	
	Troglodytidæ:	*Troglodytidæ*:
	10. Troglodytes (6.3)	6. Cistothorus (2)
		7. Thryothorus (3)
		8. Catherpes (1)
Motacillidæ:	*Motacillidæ*:	9. Salpinctes (2)
38. Motacilla (16)	11. Anthus (14.2)	10. Harporhynchus (8)
39. Limodromus (1)		11. Galeoscoptes (1)
		12. Mimus (1)
		13. Oroscoptes (1)
Crateropodidæ ?		*Mniotiltidæ*:
40. Zosterops (2)		14. Mniotilta (1)
Oriolidæ:		15. Protonotaria (1)
41. Oriolus (2)		16. Helenaia (1)
		17. Helmitherus (1)
		18. Helminthophila (8)
		19. Compsothlypis (2)
		20. Dendroica (24)
		21. Sciurus (3)
		22. Geothlypis (6)
		23. Icteria (1)
		24. Sylvania (3)
		25. Setophaga (2)
		26. Cardellina (1)
Muscicapidæ:	*Laniidæ*:	*Vireonidæ*:
42. Muscicapa (6)	12. Lanius (22.2)	27. Vireo (12)
43. Hemichelidon (1)	*Ampelidæ*:	*Ampelidæ*:
44. Alseonax (1)	13. Ampelis (2.2)	28 Phainopepla (1)
45. Xanthopygia (3)		
Hirundinidæ:	*Hirundinidæ*:	*Hirundinidæ*:
46. Chelidon (3)	14. Hirundo (3) (= Chelidon (1)	29. Progne (1)
	15. Cotile (3) (= Clivicola (1)	30. Petrochelidon (1)
		31. Tachyniceta (2)
		32. Stelgidopteryx (1)
		Tanagridæ:
		33. Piranga (4)
Fringillidæ:	*Fringillidæ*:	*Fringillidæ*:
47. Carduelis (2)	16. Chrysomitrus (2) = Spinus (4)	34. Rhyncophanes (1)
48. Serinus (4)	17. Coccothraustes (1.1)	35. Poocetes (1)
49. Chloris (3)	18. Linota (7) = Acanthis (2)	36. Ammodramus (11)
50. Passer (8)	19. Carpodacus (10.4)	37. Chondestes (1)
51. Eophona (2)	20. Pinicola (1.1)	38. Zonotrichia (6)
52. Pycnorhamphus (1)	21. Loxia (5.2)	39. Spizella (7)
53. Petronia (2)	22. Plectrophanes (2) = Calcarius and Plectrophenax (5)	40. Junco (7)
54. Montifringilla (7)		41. Amphispiza (2)
55. Fringilla (6)		42. Peucæa (5)
56. Fringalauda (1)		43. Melospiza (4)
57. Bucanetes (4)	23. Leucosticte (5.3)	44. Passerella (1)
58. Uragus (2)		45. Pipilo (6)
59. Pyrrhula (7)		46. Cardinalis (1)
60. Urocynchramus (1)		47. Habia (2)
61. Emberiza (32)		48. Guiraca (1)

PALÆARCTIC.	BOTH.	NEARCTIC
		Fringillidæ—(continued):
		49. Passerina (4)
		50. Spiza (1)
		51. Calamospiza (1)
		Icteridæ:
		52. Dolichonyx (1)
		53. Molothrus (1)
		54. Xanthocephalus (1)
		55. Agelaius (3)
		56. Sturnella (1)
		57. Icterus (6)
		58. Scolecophagus (2)
		59. Quiscalus (3)
Alaudidæ:	*Alaudidæ*:	
62. Certhilauda (2)	24. Otocorys (4.1)	
63. Galerita (3)		
64. Alauda (3)		
65. Ammomanes (2)		
66. Calandrella (6)		
67. Melanoscorypha (6)		
68. Rhamphocorys (1)		
Sturnidæ:		
69. Sturnus (4)		
70. Pastor (1)		
71. Poliopsar (1)		
72. Sturnia (2)		
Corvidæ:	*Corvidæ*:	*Corvidæ*:
73. Podoces (4)	25. Nucifraga (1) = Picicorvus (1)	60. Cyanocitta (2)
74. Pyrrhocorax (2)	26. Perisoreus (1.2)	61. Aphelocoma (3)
75. Garrulus (9)	27. Pica (2.2)	62. Cyanocephalus (1)
76. Cyanopica (2)	28. Corvus (11.5)	
		Tyrannidæ:
		63. Milvulus (1)
		64. Tyrannus (4)
		65. Myiodynastes (1)
		66. Myiarchus (3)
		67. Sayornis (3)
		68. Contopus (4)
		69. Empidonax (7)
		70. Pyrocephalus (1)
		71. Ornithion (1)
Cypselidæ:	*Cypselidæ*:	*Cypselidæ*:
77. Acanthyllis (1)	29. Cypselus (7) = Micropus (1)	72. Cypseloides (1)
		73. Chætura (2)
Caprimulgidæ:		*Caprimulgidæ*:
78. Caprimulgus (4)		74. Antrostomus (2)
		75. Phalænoptilus (1)
		76. Chordeiles 1)

PALÆARCTIC.	BOTH.	NEARCTIC.
		Trochilidæ:
		77. Eugenes (1)
		78. Cœligena (1)
		79. Trochilus (8)
		80. Iache (1)
Picidæ:	*Picidæ*:	*Picidæ*:
79. Dryocopus (2)	30. Picus (18) = Dryobates (5)	81. Campephilus (1)
80. Gecinus (6)	31. Picoides (1.2)	82. Xenopicus (1)
81. Ingipicus (3)		83. Sphyrapicus (3)
82. Jynx (1)		84. Ceophlæus (1)
		85. Melanerpes (5)
		86. Colaptes (4)
Alcedinidæ:	*Alcedinidæ*:	
83. Alcedo (1)	32. Ceryle (2.2)	
84. Halcyon (2)		
Coraciadæ:		
85. Coracias (2)		
Meropidæ:		
86. Merops (3)		
Upupidæ:		
87. Upupa (1)		
Cuculidæ:		*Cuculidæ*:
88. Cuculus (4)		87. Coccyzus (2)
89. Hierococcyx (2)		88. Geococcyx (1)
90. Coccystes (1)		89. Crotophaga (1)
		Psittacidæ:
		90. Conurus (1)
	Strigidæ:	
	33. Strix (1.1)	
Bubonidæ:	*Bubonidæ*:	*Bubonidæ*:
91. Ninox (1)	34. Asio (1.2)	91. Speotypo (1)
92. Athene (3)	35. Syrnium (4) = Ulula (3)	92. Micrathene (1)
	36. Nyctea (1.1)	
	37. Surnia (2.1)	
	38. Nyctala (1.2)	
	39. Scops (4) = Megascops (2)	
	40. Bubo (4.1)	
	41. Glaucidium (1.1)	
Vulturidæ:		*Cathartidæ*:
93. Gyps (2)		93. Pseudogryphus (1)
94. Vultur (1)		94. Cathartes (1)
95. Neophron (1)		95. Catharista (1)
96. Gypaetus (1)		
Falconidæ:	*Falconidæ*:	*Falconidæ*:
97. Circaetus (1)	42. Circus (6.1)	96. Elanoides (1)
98. Nisaetus (2)	43. Buteo (5.7)	97. Ictinia (1)
99. Milvus (4)	44. Archibuteo (2.2)	98. Parabuteo (1)
100. Pernis (1)	45. Aquila (7.1)	99. Urubutinga (1)
	46. Haliætus (3.1)	100. Polyborus (2)

PALÆARCTIC.	BOTH.	NEARCTIC
	Falconidæ—(continued):	
	47. Astur (2.1)	
	48. Accipiter (4.2)	
	49. Elanus (1.1)	
	50. Falco (17.8)	
	51. Pandion (1.1)	
Columbidæ:	*Columbidæ*:	*Columbidæ*:
101. Turtur (8)	52. Columba (14.3)	101. Ectopistes (1)
		102. Zenaidura (1)
		103. Columbigallina (1)
		104. Scardafella (1)
Pteroclidæ:		
102. Pterocles (6)		
103. Syrrhaptes (2)		
Phasianidæ:		*Phasianidæ*:
104. Phasianus (17)		105. Meleagris (1
105. Pucrasia (2)		
106. Chrysolophus (2)		
107. Crossoptilon (5)		
108. Lophophorus (1)		
109. Ithaginis (3)		
110. Caccabis (5)		
111. Francolinus (2)		
112. Perdix (4)		
113. Coturnix (2)		
114. Tetraophasis (2)		
115. Tetraogallus (5)		
Tetraonidæ:	*Tetraonidæ*:	*Tetraonidæ*:
116. Tetrastes (3)	53. Lagopus (5.4)	106. Colinus (3)
117. Tetrao (5)	54. Canachites (2)= Dendragapus (3)	107. Oreortyx (2)
		108. Callipepla (3)
		109. Cyrtonyx (1)
		110. Bonasa (1)
		111. Tympanuchus (2)
		112. Pediocetes (1)
		113. Centrocercus (1)
Turnicidæ:		
118. Turnix (3)		

By merely glancing over these lists we see at once how greatly the genera in either the first or third column exceeds those in the middle column. This tells us that the speciality of each region in relation to the other is very much greater than their similarity. In order, however, to bring out these differences and resemblances more clearly, and to show what they imply, the facts are summarised in the following table of the families, giving the distribution of their genera and species in the same order as in the preceding lists:—

FAMILIES OF NORTH TEMPERATE LAND BIRDS WITH THEIR DISTRIBUTION AS REGARDS THE PALÆARCTIC AND NEARCTIC REGIONS.[1]

Total Genera.	Families.	PALÆARCTIC ONLY. Genera.	Species.	BOTH REGIONS. Genera.	Species.	NEARCTIC ONLY. Genera.	Species.
37	Turdidæ	30	185	3	0	4	18
1	Cinclidæ	—	4	1	0	—	1
1	Accentoridæ ..	1	11				
1	Panuridæ	1	1				
8	Paridæ	4	44	3	0	1	14
1	Sittidæ	—	8	1	0	—	4
2	Certhiidæ ..	1	3	1	0	—	1
9	Troglodytidæ ..	—	6	1	0	8	22
3	Motacillidæ ..	2	31	1	0	—	2
13	Mniotiltidæ ..					13	54
1	Vireonidæ ..					1	12
1	Crateropodidæ ? ..	1	2				
1	Oriolidæ	1	2				
1	Laniidæ	—	22	1	0	—	2
2	Ampelidæ ..	—	1	1	1	1	2
4	Muscicapidæ ..	4	11				
7	Hirundinidæ ..	1	9	2	1	4	7
1	Tanagridæ ..					1	4
41	Fringillidæ ..	15	110	8	5	18	79
8	Icteridæ ..					8	18
1	Alaudidæ ..	7	26	1	1	—	1
8	Sturnidæ ..	4	8				
1	Corvidæ ..	4	30	4	2	3	14
9	Tyrannidæ ..					9	25
4	Cypselidæ ..	1	8	1	0	2	4
4	Caprimulgidæ ..	1	4	—	—	3	4
4	Trochilidæ ..					4	11
12	Ficidæ	4	31	2	0	6	22
3	Alcedinidæ ..	2	5	1	0	—	2
1	Coraciadæ ..	1	2				
1	Meropidæ ..	1	3				
1	Upupidæ ..	1	1				
6	Cuculidæ ..	3	7	—	—	3	4
1	Psittacidæ ..					1	1
1	Strigidæ ..	—	1	1	0	—	1
12	Bubonidæ ..	2	18	8	4	2	11
4	Vulturidæ ..	4	5				
3	Cathartidæ ..					3	3
19	Falconidæ ..	4	51	10	5	5	26
6	Columbidæ ..	1	22	1	0	4	7
2	Pteroclidæ ..	2	8				
13	Phasianidæ ..	12	50	—	—	1	1
12	Tetraonidæ ..	2	14	2	1	8	20
1	Turnicidæ ..	1	3				
285	(44 Families.)	118	747	54	20	113	397
Total in each Region ..		172	767			167	417

[1] It must be noted that the numbers in the "Species" columns represent the *species* which are absent from the other region, *not* the number of species contained in the "genera" of the preceding column, which are the genera absent from the other region.

The following statement, giving statistics derived from the preceding lists, will complete the information necessary for a comparative view of the relations of the Palæarctic and Nearctic Regions as regards their land-birds :—

The Palæarctic Region has **36** families of land-birds.
Of these, **12** families are not in the Nearctic Region.
These **12** families comprise **22** genera and **57** species.

The Nearctic Region has **32** families of land-birds.
Of these, **8** families are not in the Palæarctic Region.
These **8** families comprise **40** genera and **128** species.

The **118** Palæarctic genera which are not Nearctic comprise **472** species.

The **113** Nearctic genera which are not Palæarctic comprise **282** species.

The preceding lists and figures enable us to obtain a very complete view of the amount of difference that exists between the avifaunas of the Palæarctic and Nearctic Regions.

Considering first the Family groups, we find that in the one case **one-third** and in the other **one-fourth** of the families of the one region are not found in the other; and this is an amount of difference that does not occur between any other two regions which are conterminous with each other. The most striking relation of the Palæarctic and Nearctic Regions is, therefore, not their resemblance, but their dissimilarity.

Again, the **12** families which thus differentiate the Palæarctic Region from the Nearctic comprise **22** genera and **57** species; and among them we find such characteristic groups of the Eastern Hemisphere as the accentors, the flycatchers, the starlings, the vultures, and the sand-grouse,—families entirely absent from the whole Western Hemisphere.

The **8** families which differentiate the Nearctic Region from the Palæarctic are even more important, since they contain **40** genera and **128** species; and include such characteristic New World types as the Mniotiltidæ, the Icteridæ, and the Tyrannidæ, containing between them nearly a hundred species. Now as there are only **417** species of land-birds in the whole Nearctic Region, we find that between **one-third** and **one-fourth** of the whole belong to *families* which are entirely foreign to the Palæarctic Region. It may be confidently asserted that none of the other regions can be so divided that the two parts shall show an amount of difference at all approaching to this. Yet it is proposed to unite these two regions because they are not sufficiently distinct. They are, however, very much more distinct than are the Ethiopian and Oriental Regions, though the former includes the isolated and peculiar Madagascar fauna. Less than **one-twelfth** of the Ethiopian families of land-birds are not Oriental, while only **one-ninth** of the Oriental families are not

Ethiopian; showing that, by this test of the number of families which are not found in both regions, the Palæarctic and Nearctic are three times as distinct as are the Ethiopian and Oriental Regions.

If we now consider the genera which are characteristic of the one region as compared with the other, we shall find equally strong evidence of their diversity. In the Palæarctic Region we have **120** genera which are not Nearctic, out of a total of **174**; so that a little more than **two-thirds** of the Palæarctic genera of land-birds are quite unknown in the Nearctic Region; and these genera contain **472** species out of a total of **767**. In other words, out of every **5** land-birds in the Palæarctic Region, **3** belong to *genera* which are **not** Nearctic.

Looking at the same problem from the other side of the Atlantic, the results are even more striking. Out of a total of **167** genera of Nearctic land-birds no less than **113** are not Palæarctic, the proportion being almost exactly **two-thirds.** These **113** genera comprise **282** species out of a total of **417** species; so that again almost exactly **two-thirds** of the Nearctic land-birds belong to *genera* which are not Palæarctic. This is a larger proportion than in the case of the Palæarctic Region; and nothing can more forcibly bring before us the fundamental diversity of the two areas than the fact that, almost everywhere in the Nearctic Region, out of every **three** birds we might meet with, **two** would be generically unknown to the student of the Palæarctic avifauna. There is probably no such amount of difference as this between any two adjacent regions, except perhaps between the Oriental and Australian, the latter admittedly the most isolated on the globe.

I have now shown, by a careful comparison of their mammalia and birds, that the Palæarctic and Nearctic Regions, instead of being so much alike that they should be united to form a single region, are really exceptionally distinct. They are certainly much more distinct than are the Oriental and Ethiopian Regions, and are probably quite as distinct as are any two conterminous regions.

I feel confident, therefore, that any naturalist who will study the materials I have here brought together in a form to admit of easy comparison, will arrive at the conclusion that the system of Zoological Regions established by Dr. Sclater cannot be improved by the union of two such fundamentally distinct areas as are those which he has termed the Palæarctic and Nearctic Regions.

ALFRED R. WALLACE.

IV.

Some Current Problems in Experimental Psychology.

RECENT notes in NATURAL SCIENCE and in *Nature* seem to indicate that England is at last awaking to the fact that experimental psychology—which is an endowed science over practically the whole continent of Europe, and throughout the United States of America—has some claims upon her attention. It may not be amiss, at the present juncture, to put together a rough list of the problems most prominently upon the carpet; more especially as the statement of them may serve to dispel certain wrong ideas, as to the scope and subject-matter of the new discipline, which are unhappily prevalent in extra-psychological circles.

But first, as to definitions. *Psychophysics* is the science of the relation of "mind" to "body." *Experimental Psychology* is just psychology; the science which describes "mental" processes, and enumerates their conditions. The experimental method is not intended to oust introspection; accurately controlled introspection is experiment, and experiment is introspection made scientifically valuable, *i.e.*, universally valid. *Physiological Psychology* is an experimental psychology pursued by physiological methods, and with constant reference to the "physical substrate of mind." It is, therefore, narrower than experimental psychology proper, in that its methods are more restricted: it is wider than experimental psychology, in that it admits of the psycho-physical reference, which does not obtain in psychology as such.

It is often said, whether by way of reproach or merely as fact, that experimental psychology is primarily or exclusively the psychology of sensation. But the truth or falsehood of such a statement depends altogether on the definition of sensation. (1) Some psychologists (like Münsterberg) regard "sensation" as the one ultimate constituent of mind. For them, "psychology" is, of course, equivalent to "doctrine of sensation." Only their use of the term must not be confused with the usage of physiology and of opposed schools of psychology. (2) Others (like Külpe) posit a two-fold basis of mind: sensation and affection (=pleasure - pain). (3) Others, again (like Sully), make every psychosis triple; its three

elements being sensation, affection (=pleasure – pain), and conation (effort or activity).[1] Those who adopt the views of (2) or (3) have protested, and must continue to protest, against the restriction of their science to the description of and theorising upon a single psychical ultimate. And as more authors are drawn from their ranks than from those of (1), and the literature of affection and conation is quite extensive, while the term "sensation" tends to be used, even by psychologists of the first type, in its more narrow and usual sense, it would seem that the objection stated at the beginning of this paragraph can only be accounted for by the assumption of ignorance of general psychological literature on the part of those who raise it.

Still, there is no smoke without fire. How did the erroneous opinion arise in the first place? There are several reasons that might be alleged. (i.) An experimental psychology must logically begin with (technical[2]) sensation (*cf.* Wundt, *Vorlesungen*, 2nd ed., pp. 14, 15). (ii.) Chronologically, it did so begin. (iii.) Sensation-investigations tend to be assigned as theses for University degrees, because they are easier than are investigations into affection, conation, or the more complex mental processes. (iv.) This greater easiness is partly conditioned by a fact which deserves special mention: that language is the language of sensation and its derivatives; not of affection, conation, and their derivatives. For these processes gesture-language is the only direct means of communication. All spoken or written communication of them first translates them into terms of sensation and idea. (v.) There are, on the lowest estimate, over 50,000 irresolvable qualities of sensation. There are only two of affection, and only one of conation. Hence, even if sensations are grouped into modalities, the psychological literature of sensation must be far more bulky than that of affection and conation. (vi.) Physiologists—who have done much good service to experimental psychology, and service all the more valuable so long as psychology remained or remains without endowment—know, as a rule, a great deal about sensation, but little either of the literature of the other two claimants for recognition as conscious ultimates, or of that of concrete psychology (the "higher" mental processes). Other reasons might doubtless be added to these.

I have (1) not attempted to arrange the following rough list of current investigations. Indeed, classification would be impossible except in terms of a committal psychology. For the difference between the old and the new psychologies here is just this: that the old drew their parallel columns, and headed them "Intellect," "Feeling," and "Will"; while the new, however many or few the ultimates which it postulates, regards every complex psychosis as

[1] I omit, for brevity's sake, the various sub-forms of these views.

[2] "A sensation is that simple, conscious process which stands in a relation of dependence to definite nervous organs at periphery and centre." — Külpe, *Grundriss*, p. 30.

containing in it every elementary process, in greater or less degree. In place of the parallel columns is set a criss-cross of connecting lines. And though the scheme is generally accepted, the directions and number of the lines are still (as has been indicated above) matters of individual conviction. (2) Neither is the list to be considered as in any sense exhaustive. Investigations of elementary questions (of the intensity, quality, temporal and spatial relations of sensations, affections, conditions) are not included in it. And there are, doubtless, many recent investigations of compound processes which do not receive mention.

Those to which I desire to call attention are the following:—

(1.) *Recognition.*—The problem of direct and indirect recognition has been the subject of a good deal of discussion during the last few years. In his *Grundriss der Psychologie*, Külpe (*a*) propounds a new theory on the basis of experimental work done, and (*b*) indicates the path of future experimentation.—*Cf.* also an article on *Reproduction* in the *Philosophical Review*.

(2.) *Fluctuations of Attention.*—It is well known that we can attend to an unchanging impression only for a very short time. The suggestion had been made that this phenomenon was peripherally conditioned by fatigue of the sense-organ. A series of articles in the *Philosophische Studien* shows that the fluctuations are of central origin, and rehabilitates attention as a conscious process involving other than sensational elements.

(3.) *The Modes of Combination of Conscious Elements.*—(*a*) *Fusion.* The best account, theoretical and experimental, of Fusion is to be found in Külpe's *Grundriss*. Articles have been published at intervals since the appearance of the second volume of Stumpf's *Tonpsychologie*. Sensation-fusion is one of the most promising fields for experimentation now open. (*b*) *Association.* Articles on ideational association, in part based on Scripture's investigation in the *Philosophische Studien*, have appeared in *Mind*, *The Psychological Review*, *The American Journal of Psychology*. Emotional association is treated briefly in Lehmann's *Hauptgesetze*. Here, too, experiments are badly needed.—*Cf.* also (1), above.

(4.) *Memory.*—An elaborate monograph, on the basis of Ebbinghaus' *Das Gedächtniss*, has appeared in the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*.

(5.) *Rhythm and Subjective Accentuation.*—See *The American Journal of Psychology*, and an unfinished series of papers in the *Philosophische Studien*.

(6.) *Action* (= idea + voluntary act; a disparate association).—Some contributions have been made in the American journals to the analytic psychology of the simple reaction.

(7.) *The Psychological Basis of Visual Aesthetics.*—Two articles on Fechnerian lines (*i.e.*, dealing with the aesthetics of form) have been published in the *Philosophische Studien*.

To these may be added the following, as investigations rather suggested or begun, than in any sense concluded:—

(8.) *The Intensifying Effect of Attention.*—Does attention intensify its own content, or merely inhibit other content?

(9.) *Apperceptive Completion.*—Within what limits do we overlook misprints, complete faulty stroboscopical images, etc.?

(10.) *The Influence of Visual upon Tactile Space.*—How far is a space judgment in cutaneous terms possible?

(11.) *Mental Fatigue.*—*Cf.* the *Amer. Journ. of Psych.*, the *Zeits. f. Psych.*, etc.

(12). *The Psychological Basis of Visual Aesthetics.* (Content.)

However incomplete, within the lines laid down for it, this list may be, it will at least serve its double purpose: it will indicate the nature of some of the current problems in experimental psychology, and prove that that science is not simply the science of sensation. It takes no account of recent work in social psychology, psychogenesis, mental pathology, or animal psychology.

E. B. TITCHENER.

V.

Notes on Ground-Ice.

EVERY student of geological text-books and many observers of nature must be aware of the fact that ice sometimes forms on the bottom of running streams, instead of on the top, as invariably happens with standing water. Sixty years ago the knowledge of this fact seems to have been almost confined to the unlearned, and its reality was denied by the natural philosophers of the day; it has now become one of the elementary facts of physical and geological science alike, and is only doubted by some few, like Mr. Leslie in his charming "Letters to Marco," who, though keen observers of nature, make no pretence to be regarded as scientific authorities.

The subject is referred to in every text-book of geology, principally from the point of view of the action of this form of ice in modifying the shape of the earth's surface, and the question of its mode of origin is treated in a more or less imperfect, where not inaccurate manner. The standard account, to which reference is often made, was published in 1833 by Arago,[1] whose paper is principally devoted to a proof of the reality of the phenomenon, which appears at that time to have been generally regarded as incompatible with the fundamental laws of nature. The theory advanced by him was that, owing to the swirls and eddies of the current, the whole body of the stream is reduced to freezing point,[2] and that in this water the stones and weeds on the bottom serve as so many nuclei for the ice crystals to form upon.

Within two years of the publication of this theory a totally different one was broached,[3] and supported by some careful and instructive observations. According to this the formation of ground-ice is due to loss of heat by radiation from the stones on the bottom, which are thus cooled below freezing point, and so give rise to the growth of ice crystals in a manner analogous to the formation of hoar frost.

These two are the theories found, in one form or another, in nearly every text-book, and of them the latter appears to enjoy the greater

[1] *Annuaire du bureau des Longitudes*, 1833, pp. 244–268.

[2] *See* Desire Leclerq, *Mem. Cour. Acad. Roy. Bruxelles*, vol. xviii. (1845), forob servations confirmatory of this.

[3] J. Farquharson, *Phil. Trans.* vol. cxxv., 329–343 (1835).

popularity, a result probably of its greater ease of comprehension, as much as of the observations to which it was conjoined. There are, indeed, certain facts which seem to support this theory, such as the general consensus of observers that ground-ice is only produced in clear water, and on cloudless nights, when radiation is most active; and Mr. Farquharson in his paper lays stress on the fact that it was not found near the piers of a bridge, close to an embanking wall, or under a grass-clad bank. This is, however, explicable in other ways than by radiation having been checked by these objects, and as regards the first part of the argument the coincidence may well be due to the fact that sharp frosts seldom occur in temperate latitudes except in cloudless weather, while the clearness of the water is as much a necessity of the first-named theory as a support of the second. Moreover, if there were any truth in the assumption, that the clear water of a stream flowing over a stony bottom is cooled to any important extent by radiation from the stony bottom over which it flows, this would be equally true in the case of standing water. But such we know is not the case, and it is the fact that the loss of heat takes place principally from the surface of the water, combined with its expansion and consequent decrease of density as the freezing point is approached, that makes our ponds freeze first on their upper surfaces.

Against the other theory, that the pebbles on the bottom merely act as nuclei on which the ice crystallises out from water cooled down below freezing point, it might be urged that this cannot occur with water such as is met with in nature, but only after it has been distilled or otherwise cleared of all dissolved air and solid particles. This, however, is not altogether the case. I have myself observed the cooling of water below freezing point when no special precautions had been taken. While camping in the desert of Western Rajputana it was a common experience to find, after a clear and cloudless night in January and February, that the water in my wash-hand basin was frozen over, and frequently frozen solid. On one occasion, after a cold night, I was surprised to find that the water was unfrozen, and, thinking that perhaps the night had not been so cold as I imagined—I had not then examined my thermometer—I plunged my hands into the water, which immediately became converted into a pasty mass of ice crystals and water. Here ordinary well water, in a basin which was only clean in the common sense of the word, had cooled below freezing point without solidifying; but the occurrence was an exceptional one, and it may well be doubted if it could take place as an ordinary event in the waters of a stream.

There is yet a third theory, originally published in 1816,[4] which appears to have been almost completely lost sight of since, that ground-ice is due to the presence of numerous minute crystals of ice in the ice-cold water; these are carried to the bottom of the stream

[4] T. A. Knight, *Phil. Trans.*, 1816, pp. 286–293.

by the eddies, and there, coming into contact with stones and weeds, are collected and frozen together into a tangled mass of spongy ice. It is probable that ice crystals are always present when ground ice is being formed, they are specifically mentioned by Knight,[5] by Eisdale,[6] who thought that they originated in the air and then fell into the water, and by Rae,[7] while the only observation which my limited opportunities have enabled me to make, tends to show that the accumulation of small ice crystals independently formed in the water of the stream is a most important factor in the production of ground-ice.

In May, 1888, I was encamped at Karzok, on the shores of the Tso Moriri, at an elevation of close on 15,000 ft. above the sea. The camp was pitched on the edge of a small oasis of cultivation, and the irrigation channel, which supplied the fields, flowed not far from the door of my tent. The night had been a cold one, and in the morning, while the ground was still hard with frost, I noticed that the water in the irrigation channel had risen and overflowed its banks; as this water was derived from the melting of snow on the hills, it should have been at its lowest in the early morning, and the rise of the water during the night made me look for the cause of so unexpected an occurrence. I found that a sheet of semi-opaque, whitish ground-ice had formed on the bottom of the channel and so raised the level of the water, and that the ice was still growing. As the depth of the channel was under a foot, the process was an easy one to observe, and I was able to notice that the water was full of minute crystals of ice, which were swept along by the current, and, coming into contact with the surface of the ice on the bottom, became entangled in the irregularly disposed crystals of which it was composed and frozen into one solid mass with them. Owing to a diversion of the stream, consequent on its rise of level, I was also able to observe the gradual formation of a continuous sheet of ice. At first the ice crystals accumulated against stones and weeds in the bed, and principally on the up-stream side, as was observed by Knight in 1816; but as the numerous small local clusters of crystals grew in size they coalesced to form a continuous sheet with an irregularly-mammilated upper surface.

From these observations it would seem that the earliest theory proposed comes so near the truth that the neglect it has met with is not justified. The crystallising out of water reduced to or slightly below freezing point is doubtless a cause, and may in some case be the most important one in the formation of ground-ice; but the process is aided to a much larger extent than is generally recognised by the entanglement and accumulation of ice crystals formed and floating in the water.

R. D. OLDHAM.

[5] *Phil. Trans.*, 1816, p. 286.
[6] *Edin. New Phil. Journ.*, vol. xvii., 167–171 (1834).
[7] *Nature*, vol. xxi., 538 (1889).

VI.

The Significance of the Bird's Foot.

TO say that birds have been evolved from reptiles is to utter one of the tritest of zoological commonplaces; but as to the nature of the particular reptilian stock which blossomed out into birds, there is by no means a complete accord among zoologists. Some have suggested a connection between the birds and the pterodactyles: a connection which, if it ever existed, must have done so before either class obtained the power of flight. For it is hardly possible that animals which had developed the pterodactyl form of flying organs would then proceed to change them into feathered wings. Others, led away by the resemblances between the *Ratitæ* and the ornithopodous *Dinosaurs*, have traced the pedigree of flying fowl from the latter through the former. But the striking resemblances between the feet of these groups may, in all probability, be set down as produced by similar needs and conditions, especially since both have the same reptilian origin. For even in mammals we find a very bird-like hind-limb, with coalesced metatarsals, in the Jerboa, which walks in the same manner as a bird when not performing its prodigious leaps. Conversely, there is a strong superficial resemblance between the hind-limbs of an ostrich and a horse.

Even Professor Fuerbringer, however, thinks that the first birds were probably terrestrial, afterwards diverging into climbers on rocks and trees, and inhabitants of swampy regions, the latter stock giving rise to waterfowl.

It is not without considerable diffidence that I venture to propose a different theory, namely, that birds originated from an arboreal reptile, which had acquired tree-haunting habits before it began to develop wings, and that all terrestrial and aquatic forms of bird-life can be derived from arboreal ancestors.

If we survey the various divisions of the vertebrates, we shall find, almost invariably, that the first steps towards flight are taken by already arboreal forms. Such forms, taking short flights by means of a skinny parachute, are found among reptiles, in the "flying" geckos (*Ptychozoon*), and the "flying" dragons (*Draco*). Among mammals, we may cite the "flying" Sciuridæ, the Anomalures, and especially *Galeopithecus*, which, with its interfemoral membrane and long webbed fingers, plainly indicates the way in which the bats probably

developed. The latter animals, possessing true flight—in fact, they are the most aërial of all vertebrates—still show, in their tree- and rock-frequenting habits, and activity in climbing, traces of their scansorial ancestor. In mammals, in this way, we can trace an almost complete transition from a terrestrial to an aërial life, the intermediate stage being spent in the trees.

It will, however, doubtless be objected, that the flying-fish are the animals which present the nearest approach to true flight among the parachutist vertebrates. Indeed, after watching these creatures, one is inclined to agree with Pettigrew, that they really do fly for short distances; and we know that the power of flight is in some birds quite limited.

Darwin, also, points out that there is no difficulty in imagining that it might be of advantage to a penguin-like bird, first to flap along the surface of the water like the logger-head duck (*Micropterus*), and ultimately to rise in the air.

But against this it may be urged that aquatic animals do not tend towards complexity of epidermic clothing, as witness the hairless *Cetacea* and *Sirenia*; so that it is difficult to see how feathers, those most complex of epidermic structures, could have arisen on an aquatic animal. Besides which, I hope to be able to show that the evidence is all in favour of aquatic birds being descended from terrestrial, as indicated above, and not *vice versâ*. This brings me to the point contained in the title of this paper. The avian foot is, in its way, almost as characteristic as the fore-limb; and its typical form is one adapted, by its backwardly-directed hallux, for grasping boughs. This form is found in *Archæopteryx*, and in many important existing groups, to wit, Passeres, Pigeons, and Herons, and other modifications may easily be derived from it. Thus, *zygodactylous* birds become so by a gradual shifting backwards of the fourth digit, a change which is apparently still in process in the owls, which always perch in the *zygodactyle* position. An analogous turning back of the second toe gives the *heterodactylous* foot of the trogons. As this backwardly-directed hallux is unnecessary for terrestrial and aquatic birds, we find in these a tendency towards its disappearance; and in some groups the process can be traced. Thus, in the Gallinæ we find the Curassows and Megapodes with a well-developed thumb, while in the terrestrial game-birds it is small, elevated, and almost functionless; indeed, I have noticed that domestic fowls, when roosting, turn the hallux forwards. It is noteworthy that the Megapodes, with their grasping feet, also display the reptilian character of hatching their eggs otherwise than by incubation, and the young emerge from the shell in an almost adult condition. Similarly, among the *Anseres*, we find that in *Anseranas*, where the webs of the feet are least developed, the hallux is well-developed and incumbent, whereas the rest of the family show it in a nearly or quite functionless condition. Likewise those archaic Anseriformes, the

Screamers, combine with their slightly-webbed feet a normal hallux, so that in this group there is a fairly clear indication of a transition from semi-arboreal land-birds to waterfowl, which may help to explain the tree-nesting habits of many of the ducks, such as *Dendrocygna*, *Aix*, *Clangula*, etc.

Among birds of the family Limicolæ, the hallux is on the point of disappearance; in many plovers it is absent, and where present very small and useless. Among the gulls, which are usually considered as offshoots of the terrestrial Limicoline stock, though usually present in a functionless condition, this toe is on the very point of disappearance in the common Kittiwake (*Rissa tridactyla*), some individuals of which species have it, while others have only a mere tubercle. Here, again, it is noteworthy that *Dromas*, probably a connecting link between Gulls and Plovers, and therefore presumably more archaic than either, exhibits semi-palmate feet with a fair-sized posterior toe.

The arboreal pigeons have a typical grasping foot, while in their terrestrial relatives the Sandgrouse it is disappearing, being quite absent in the highly-specialised *Syrrhaptes*.

Few important groups exist in which the hallux is entirely wanting, the most noteworthy among Carinates being the Bustards; but the affinities of these to hallux-possessing forms are obvious. In many birds usually regarded as tridactylous, subcutaneous vestiges of the missing toe are to be found, notably in some Woodpeckers and Albatrosses, thus actually proving what is easily inferred, that the toe has disappeared in these forms. It is noteworthy that in almost every case where a hallux appears at all, it retains to the last its posterior direction, thus showing its original function. This may be seen even in some Ratitæ, *e.g.*, in *Apteryx*.

The exceptions to this rule may usually be easily explained. The forwardly-directed first toe of some Swifts is obviously a special adaption, as also are the versatile halluxes of the Coliidæ and Goatsuckers; while the hallux of the Steganopodes, often stated to be turned forward, really has an inward or backward direction, and functions in perching in the ordinary way, as anybody may observe in living birds. These birds are, still, less attached to the water than most aquatic birds, and their semi-arboreal habits have no doubt descended to them directly from an ancestry which they share with the Herons.

It may be argued that many arboreal birds may have been originally terrestrial, just as certain geese (*e.g.*, *Cereopsis*) have left the water for the land. But we know that terrestrial life leads sooner or later to a reduction of the toes, and that the hallux is the first to disappear in almost every case, so that it would have had to be re-acquired to fit a ground-dweller for a life in trees—and there is, as Garrod has already pointed out, no reason to believe that an organ originally lost is ever reproduced.

And even the highly-specialised and pre-eminently arboreal Passeres are evolving terrestrial forms in the Larks, waders in the Wagtails, and even waterfowl in the Dippers—none of which have as yet had time to lose the typical foot of their order; and thus avian history is repeating itself.

It would seem, therefore, that this highest group of birds is, in virtue of its habitat and foot-structure, in reality one of the most primitive. And this is paralleled among the Mammalia by the pentadactylous condition of the otherwise highly-organised Primates.

If, then, as I hope, I have given reason to believe that the arboreally-adapted form of foot is the one we should naturally expect to find in the ancestors of the birds, this feature gives, I think, an additional claim to *Archæopteryx* to be regarded as one of those ancestors, rather than as a side-branch of the avian tree.

Its teeth, the free and clawed digits of its hands, and its long tail without pygostyle, fulfil the requirements of a schematic primitive bird, and if its grasping feet may be added to the design, we may fairly say that in the Solenhofen fossils we have a creature which stands as nearly as possible upon the central line of avian development in past ages.

FRANK FINN.

SOME NEW BOOKS.

The Monograph on Salpa.

The Genus Salpa. A Monograph, with 57 plates. By W. K. Brooks, Ph.D., LL.D., Professor in the Johns Hopkins University. Memoirs of the Biological Laboratory of the Johns Hopkins University, II. Baltimore: The Johns Hopkins Press, 1893.

Salpa is itself so interesting an animal and belongs to so interesting a group that the results of Professor Brooks's researches deserve at once a fuller and a less technical account than is given usually in a review. The account need not be technical, for all morphologists who take a special interest in the Tunicates will have to pore over this magnificent memoir. Even to those who pay no special attention to them, the Tunicates are interesting because they seem to possess, more certainly than any other group, indications that they represent the stock from which the vertebrate animals were derived. But in addition wa this general interest, Dr. Brooks has brought before us matter so but prising that it may well attract the notice of all who care for the man blems of nature.

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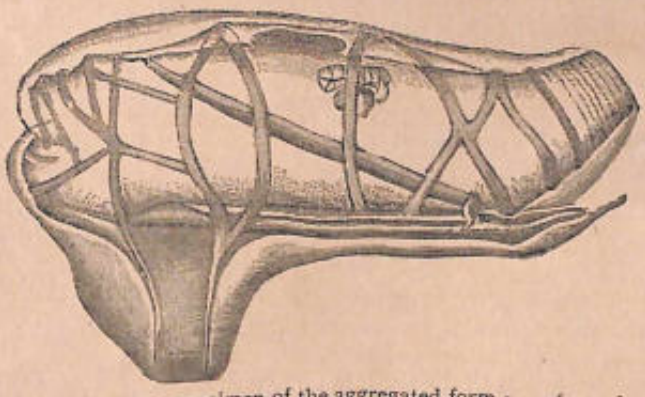


Fig. 1.—*Salpa pinnata:* a specimen of the aggregated form seen from the left side.

Salpa is a transparent swimming Tunicate, which may be compared to a barrel, open at both ends. It is, in fact, an enormous Pharynx which swims through the water, gulping great mouthfuls at every contraction of its muscles. A rod-like gill stretches from above the wide open mouth in front, through the hollow of the barrel, to below the posterior aperture behind; but this is so narrow that it offers no obstruction to the flow of water through the animal. Along the ventral line of the inside of the barrel runs a ciliated, mucus-secreting band, which is the homologue of the endostyle in ascidians and *Amphioxus*. At the posterior end of the barrel, in a thick mass, are contained the viscera, and this mass is the only part of the animal which is not transparent. Representing the hoops of the barrel are a number of muscular bands by the contraction of which the water is driven through

the body. The whole animal is surrounded by a thick, tough, transparent "mantle."

The food of *Salpa* consists of minute organisms like diatoms, radiolarians, and so forth, and as these are practically unlimited in quantity on the ocean surface where *Salpa* lives, *Salpæ* are often found swarming in numbers beyond description. They vary in size from a quarter of an inch to eight inches. Though most abundant in the tropics, they are found from further north than Scotland and Norway to beyond Cape Horn and the most southerly parts of Australia. "They are abundant," says Dr. Brooks, "only after the water has been for some time undisturbed by winds: and as prolonged calms are most frequent in warm seas, these waters are most favourable for the development of these animals, which multiply with most astonishing rapidity. The smaller species are often so abundant that for hundreds of miles any bucketful of water dipped up at random, will be found to contain hundreds of them. In such places collecting with the surface net becomes impracticable, for almost as soon as the net is dropped into the water, it becomes choked with a mass so that nothing can enter it."

Since the time of the poet and naturalist Chamisso, it has been known that there are two generations in the life cycle of *Salpa*. These are a generation of solitary individuals, and a generation of



FIG. 2.

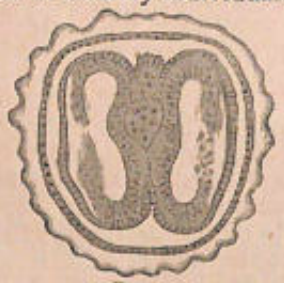


FIG. 3.



FIG. 4.

FIG. 2.—Chain of germ-cells surrounded by follicle-cells, lying in a blood canal.
FIG. 3.—Section through a young embryo.
FIG. 4.—Fertilised egg-cell surrounded by follicle-cells.

individuals aggregated into chains or clusters. The specific characters of the two generations differ from each other so widely, that without rearing them there is no possibility of determining which simple and solitary forms belong to each other. The solitary *Salpa* is born from an egg which is carried within the body of the aggregated *Salpa*, and the embryo, by means of a nutritive placenta, is nourished during its development from the blood of the aggregated form. The aggregated *Salpæ* are produced asexually by budding from a stolon within the body of the solitary form. The eggs arise at an exceedingly early stage in the development of the animal. Thus the curious result happens that three generations are simultaneously present in the same animal. A solitary *Salpa* contains a developing chain of the individuals of the aggregated form, and within these are already present the eggs which are to give rise to the next generation of solitary forms. Actually, the germ-cells can be seen in a solitary form before the stolon is visible which is to give rise to the generation of individuals in which the germ-cell will develop. As the stolon forms, it includes within it the mass of germinal cells; and as the stolon elongates to form the chain of individuals, the mass of germ-cells also elongates and becomes pulled out, so that a single egg-cell is shut off in each individual of the aggregated series. This chain of eggs is enclosed in a chain of follicle-cells, as in Fig. 2, which,

however, does not show the ventral band of thick epithelium which ultimately gives rise to the testes. From these follicle cells short fertilising ducts grow out to the wall of the cloaca of each individual of the chain. Through these ducts fertilisation takes place, and each salp of the chain thus comes to contain a fertilised egg, which is to give rise to a solitary individual. In Fig. 1 is shown an individual of the chain-salp of *Salpa pinnata*, with the developing embryo attached to the wall of the cloaca. The egg and the embryo in its youngest stages are really outside the wall of the cloaca, in one of the blood sinuses of the chain-salp, but attached to the cloaca by the fertilising duct which opens into the cloaca. As the embryo grows it pushes into the wall of the cloaca, without, however, breaking through, and so becomes suspended in a sac, which is really the in-pushed wall of the cloaca, and which opens into the blood sinus. This sac remaining open to the blood sinus of the chain-salp, expands considerably, and becomes the placenta, by which the embryo continues to get its nourishment from the blood of the chain. The edge of the cloaca, round the place where the embryo is pushing in, grows in as a double fold, the hollow between the layers of which is, of course, in continuity with the blood space in which the embryo was formed. This double fold comes to form a special sac, the amnion or embryo-sac, which opens into the cloaca by a small pore, the space left by the edges of the fold not completely meeting each other. Thus a horizontal section cut through, such an embryo as hangs into the cloaca in Fig. 1, would pass first through the outer wall of the amnion, next through the blood space, thirdly through the inner wall of the amnion, fourthly through the brood-pouch, which is part of the cloaca of the chain-salpa, next through the epithelial capsule, then through the embryo (*see* Fig. 3). The embryo-sac, however, does not increase in size, and the growing embryo rapidly pushes itself through the small pore, and comes to hang directly into the cloaca.

Within these complicated membranes a process of a most astonishing nature has been going on. The fertilised egg-cell was at first surrounded by a layer of follicle-cells, which, themselves being derived from the germinal cells, differ from the egg only in not being actually developed into mature eggs and becoming fertilised (Fig. 4). The egg-cell divides slowly, the follicle-cells divide rapidly, and many of them push their way in towards the centre of the mass, and actually push apart those cells which result from the division of the egg-cell. During this time constant migration of nuclei takes place from the ingrowing follicle-cells to the cells which have come from the egg-cell. No doubt this is a process of nutrition, not very different in kind from some known methods, although it is at least curious to find nuclear matter being added from an extrinsic source to the developing embryo. If it be remembered that most now believe that the nucleus of a fertilised embryo is derived equally from the nuclei of the male and female generative cells, that this nuclear matter is the bearer of the inherited qualities of the embryo, and that by subsequent assimilation and growth it gives rise to the entire nuclear matter of the new organism, then it will be sufficiently astonishing to find the nuclear matter of any cells serving simply as nutritive material. But, if this is difficult to understand, what is to be said of the other process, by which the embryo is, so to say, broken in pieces during its development? For what else is the separation of the segmentation spheres by the intruding follicle-cells? Driesch and Wilson have shown cases where the separation of the first two spheres by artificial means resulted in the formation of twin embryos.

Here is a natural parallel, with the remarkable difference that not many embryos, but only a single embryo comes from the separate pieces, which, after pursuing for a time a separate course, re-unite. But more astonishing matter remains. The actual embryo is first blocked out in follicle-cells.

"These form layers and undergo foldings and other changes which result in an outline or model of all the general features in the organisation of the embryo. While this is going on, the development of the blastomeres is retarded, so that they are carried into their final positions in the embryo while in a very rudimentary condition. Finally, when they have reached the places which they are to occupy, they undergo rapid multiplication and growth, and build up the tissues of the body directly, while the scaffolding of follicle-cells is torn down and used up as food for the true embryonic cells."

"An imaginary illustration may help to make the subject clear. Suppose that while carpenters are building a house out of wood that brickmakers pile clay on the boards while they are carried past, and shape the lumps of clay into bricks as they find them scattered through the building where they have been carried with the boards. Now, as the house of wood approaches completion, imagine that brickmakers build a brick house over the wooden framework, not from the bottom upwards, but here and there wherever the bricks are to be found, and that as fast as parts of the brick house are finished, the wooden one is torn down. To make the analogy complete, however, we must imagine that all the structure which is removed is assimilated by the bricks, and is thus turned into the substance of new bricks to carry on the construction."

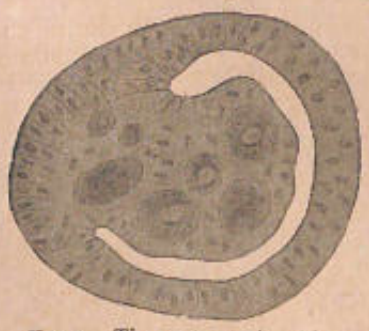


FIG. 5.—The segmentation spheres lying in a mass of follicle-cells that have grown in between them.

Fig. 5 shows a stage where the cells resulting from the early divisions of the egg-cell have been separated by the intruding follicle-cells. Figures showing the outlining of the embryo by the follicle-cells would be too complicated to reproduce without elaborate explanation and colouring, as in the beautiful illustrations of Dr. Brooks's monograph. But it must be repeated that, down to details like the structure of the nervous system, this system of preformation in follicle-cells occurs. The fate of all the structures formed of follicle-cells is the same. They degenerate, forming food for the true embryonic cells. Former observers had described something like this. Salensky, for instance, stated that the embryo of *Salpa* began as a true embryo, but afterwards the accessions from the follicle wall turned the embryo into a bud. He believed, therefore, that its development was to be compared in no way with ordinary embryological processes. Dr. Brooks considers that, however abnormal, it is a true embryo, and the appearance of budding is illusory. Certainly his figures bear out this view completely.

Dr. Brooks believes that comparative embryology has established that the ancestors of all the Tunicates once possessed small eggs with little or no food-yolk, and that these small eggs divided completely, giving rise first to a mulberry-shaped mass and then to a hollow sphere of cells. By the inpushing of the cells at one point they gave

rise to a little two-walled sac or gastrula. In many groups of vertebrates this simple form of development becomes impossible; for the egg-cell becomes so loaded with inert food-yolk that it does not divide completely, but the shape of the embryo is modified precisely in proportion to the amount of food-yolk present. The yolk is not distributed in the same way in all the animals possessing it. In some

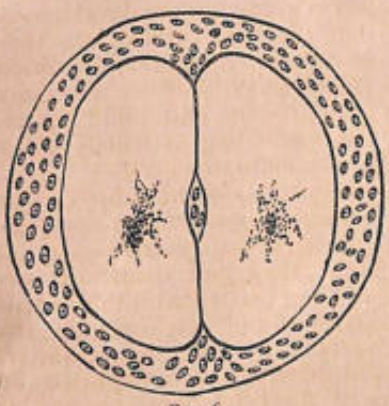


FIG. 6.

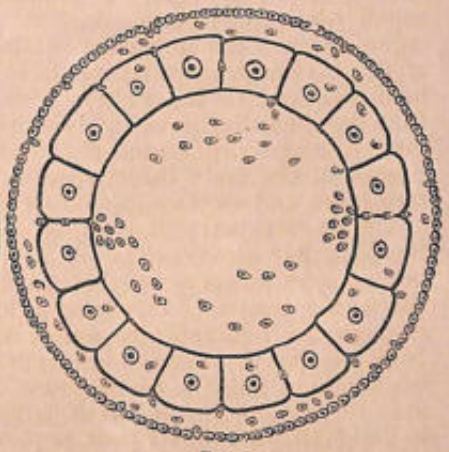


FIG. 7.

FIG. 6.—Egg of *Distaplia*: the first division has occurred, and follicle-cells are growing in between the segmentation spheres.

FIG. 7.—Diagram of a blastosphere where follicle-cells are passing into the segmentation cavity.

cases it is restricted chiefly to the cells which are to form the alimentary canal. In others, as anyone who has examined a young tadpole must know, all the cells are packed with yolk-spheres. In *Pyrosoma*, a near ally of *Salpa*, there is a large amount of food-yolk, and that is massed not in the future digestive-cavity but in the body-cavity. But the food-yolk is aided by follicle-cells, which push their way into the developing cells and are absorbed by them. Dr. Brooks suggests a method by which the peculiar arrangement in *Salpa* might

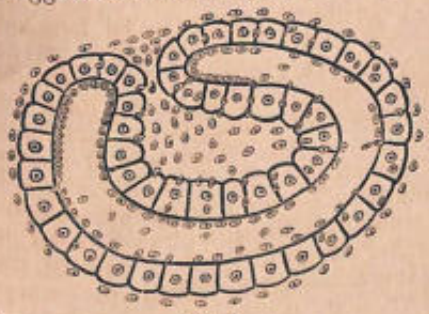


FIG. 8.

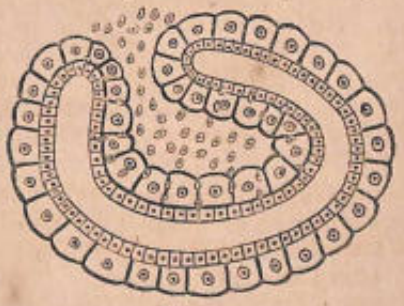


FIG. 9.

FIG. 8.—Diagram of invagination in an embryo such as that of Fig. 7.

FIG. 9.—Diagram of the gastrula of an embryo in which the follicle-cells form an epithelium.

have come about. Fig. 6 represents an actual stage in the development of *Distaplia*, which has cells bloated with food-yolk, but still able to divide completely. Two segmentation spheres are represented, surrounded by a number of follicle-cells, some of which are pushing their way in between the segmentation spheres, and which are absorbed as

food. Fig. 7 represents an imaginary stage in the development of a form which has lost food-yolk, and which, in consequence, has a large hollow segmentation cavity into which follicle-cells are migrating from the layer of follicle-cells surrounding the embryo. In Fig. 8 is represented the invaginating stage of such an embryo. The follicle-cells are, of course, inside the segmentation cavity, but they also follow the invagination, and fill the hollow that usually forms the digestive tract. Now in Fig. 9 is represented a similar stage, but in a case where the follicle-cells, instead of migrating irregularly, arrange themselves as a regular epithelium. In it is to be seen the ordinary invaginate gastrula, but the cells derived from the follicle arrange themselves as a regular cast of this. In subsequent development it is easy to see that the follicular epithelium might follow all the developments and changes of the real embryonic cells, so that each fold of tissue would be formed not only of embryonic cells, but of follicle-cells closely adherent. Dr. Brooks supposes that *Salpa* had such a history, and that, when this condition of things was reached, the development of the embryonic cells began to lag behind the development of the follicle-cells, which originally merely followed the course of the cells they came to anticipate. In Fig. 10 is shown the case of an embryo in which the invagination has taken place in follicle-cells, but where the real embryonic cells are so delayed that they appear merely as scattered segmentation spheres, and this last figure is merely a diagrammatic representation of what actually occurs in *Salpa*, as may be seen by comparison with Fig. 5.

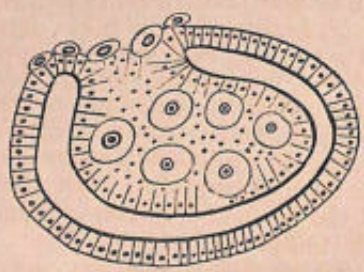


FIG. 10.—Diagram of an embryo in which the segmentation spheres delay development.

There has already appeared in NATURAL SCIENCE, vol. iii., p. 223, a brief account of the interesting views to which the development of *Salpa* has led Dr. Brooks. As that account was based on three chapters of this monograph which appeared separately, it is necessary now only to remind readers that Dr. Brooks has come to the interesting conclusion that a simple pelagic form, of which *Appendicularia* is the nearest living representative, is the common ancestor of the Ascidians and the Chordates; and that consequently there can be only among such organs as are found in simple transparent, surface-living creatures any homologies between vertebrates and invertebrates; arguments linking annelids, crustacea, and so forth with vertebrates, because of similarities in segmentation, body-cavity, etc., are dismissed as empty shadows of morphological dreams.

In this short attempt to bring to the notice of the general scientific public some of the more interesting results of Dr. Brooks' work, it has been impossible to touch upon many of the more complicated and technical matters which all specialists will have to study with attention in this great contribution to scientific literature.

FERNS.

THE STUDY OF THE BIOLOGY OF FERNS BY THE COLLODION METHOD, for advanced and collegiate students. By G. F. Atkinson, Ph.B. 8vo. Pp. xii. and 134. New York: Macmillan & Co., 1894. Price 8s. 6d. nett.

THE author, who is associate professor of cryptogamic botany in Cornell University, Ithaca, N.Y., was led to the preparation of this

work by the success which attended his efforts in applying in his classes the "collodion method" to the preparation of the very delicate tissues of ferns, and especially to the infiltration of prothallia, without shrinkage. The first part of the book, occupying nearly three-fourths of the whole, is descriptive, and is an account of the history of development and anatomy of Ferns in the limited sense of the homosporous leptosporangiate Filicinæ. It is profusely illustrated by admirable figures, all reproduced from preparations made by the author or his pupils by the method referred to, and serving at the same time as a first-class recommendation of that method. Chapter I. deals with the gametophyte, from the germination of the spore to the fertilisation of the archegonium; while the sporophyte is the subject of the five following chapters. Chap. II., on the embryo, contains a very fine figure (fig. 49) of the young embryo of *Adiantum concinnum*. In Chapters III. and IV. the morphology and anatomy of the stem, root, and leaves are illustrated, while Chapters V. and VI. form a valuable contribution to our knowledge of the structure of the sporangium in the different families and their manner of dehiscence, with the distribution of the spores. Under '*Substitutionary Growths*' (Chap. VII.) we find an account of the development of the buds or bulbils on the pinnules or in the axils of the leaf of Aspleniums and other ferns where sporophytic budding occurs. Part I. concludes with a chapter on the Ophioglosseæ, which are introduced, not because of any near relation to the Filices, but because fresh material is easily procurable, while its members present excellent subjects for comparative study.

Part II., *Methods*, comprises notes on technique (preparation of the collodion, etc.), and hints for the preparation, infiltration, sectioning, fixing and examination of the prothallia and other organs of which the structure or development is to be studied.

The book is not a large one, considerable space is given to figures, and the text is not crowded; it is, therefore, not exhaustive, but merely an admirable and readable illustration of what has been done, and done well, by a certain method, and a guide to further work on the same lines. The general appearance of the book is quite up to Messrs. Macmillan's standard, and when we remember that every one of the 163 figures is new, the price cannot be considered exorbitant.

Flowering Plants.

An Introduction to Structural Botany (flowering plants). By D. H Scott, M.A., Ph.D., &c. 8vo. Pp. xii. and 288, with 113 figures. London: A. & C. Black, 1894. Price 3s. 6d.

This book, which "is intended as a first guide to the study of the structure of plants," forms a new departure in elementary botanical works. It stands out from the ever-increasing crowd of guides, text-books, and manuals in virtue not only of originality of design, but also of the fact that the subjects treated have been specially investigated for the purpose of the book, so that we have not the mere compilation of a book-man, but an account based on the results of the author's own observation. Three plants, the wallflower, white lily, and spruce-fir, are selected as illustrating respectively the dicotyledonous, monocotyledonous and gymnospermous types of seed-plant, and in as many chapters the author gives us a clear, well-arranged, and concise account of their external characters, the structure of the stem, leaf, root, flower, fruit, and seeds, and the development of the reproductive organs and embryo. There is also a shorter chapter on

the physiology of nutrition. When the student has honestly worked through these chapters and seen, with the help of a competent teacher, a good series of preparations illustrating the various points of anatomy, etc., he will be well-grounded in structural botany, and ready for more detailed study and investigation, with the further advantage of having nothing to unlearn. The importance of the practical demonstration of structural features is insisted on in the preface, and with good reason, as the excellence of the figures might tempt the teacher to dispense with a microscope, especially as his sections will probably often be inferior to those so ably drawn by Mrs. Scott. Many of the figures are new, a welcome change from the well-worn "after Sachs"; several good ones have been borrowed from Strasburger; and Payer's diagrams illustrating the development of the flower of the wallflower and lily have been successfully reproduced by process. Almost the only feeble one is the last, a seedling of *Picea*, and after Kerner too! Messrs. Black have made a valuable addition, well produced in handy form, to their list of scientific publications.

GEOGRAPHICAL DISTRIBUTION OF PLANTS.

MANUEL DE GEOGRAPHIE BOTANIQUE. Par Dr. Oscar Drude, traduit par Georges Poirault, et revu et augmenté par l'auteur, avec 4 cartes en couleurs, et 3 figures dans le texte. Livraisons 2, 3. 8vo. Pp. 49–128. Paris: P. Klincksieck, 1893. Price of each part, 1 fr. 25. Subscription price, to complete work (12 or 13 parts), 15 francs.

IN reviewing the first part of this translation and re-edition, which appeared last June, we commended the idea and also the italicised notice in the advertisement which stated that *the parts would rapidly succeed each other*. This promise has not been very well kept, which seems a pity, as there can be no doubt as to the value of the work.

In parts 2 and 3 now to hand, Section III., on biological varieties of organisation determined by geographical and topographical factors, which is an account of forms and zones of vegetation, brings us to the end of the first part of the book, and we proceed on page 40 to the second, in which the author brings together evidence to prove that the areas now occupied by plants have been determined by geological evolution, by the superficial structure of the globe, and by climate. The tendency to dispersive migratory power, the development of special floras or faunas as the result of geographical isolation, are considered, and insular floras and those of high mountain chains and deserts adduced in evidence.

Insular floras supply proof also of the date of issue of parts 2 and 3, which is given on the cover as 1893. On page 114, however, we find a reference to a review of the subject by Mr. Hemsley, which, as correctly stated in a foot-note, appeared in *Science Progress* in March of the present year. We trust that there may be no delay in subsequent issues to necessitate such falsification of date.

LICHENS.

A MONOGRAPH OF LICHENS FOUND IN BRITAIN, BEING A DESCRIPTIVE CATALOGUE OF THE SPECIES IN THE HERBARIUM OF THE BRITISH MUSEUM. By the Rev. James M. Crombie, M.A., F.L.S., &c. Part I. 8vo. Pp. viii., 519, with 74 figures in the text. London: Printed by order of the Trustees, 1894.

THE ordinary person, or even the botanist who is not a lichenologist, will marvel at the number of British Lichens, and the more when

he is reminded that the present volume is concerned with only half of the known British species. The remainder of the work, Mr. Carruthers tells us in a prefatory note, is so advanced that it may be expected to appear next year, and will contain a complete index to all the genera and species. The re-arrangement of the group in the Herbarium of the British Museum, and the incorporation of a great number of specimens, showed the necessity of a revised catalogue, while the extensive series, including a large number of authentic specimens from the principal workers, supplied material, in many cases unique, for determining species and geographical distribution in the British Isles. The book, which is systematic, and nothing else, is well arranged, and forms a valuable addition to the list of British Museum catalogues, as well as to the literature of Lichens.

Considering the extreme technicality of the descriptions, the glossary is somewhat inadequate, and we hope Mr. Crombie will see his way to make it more complete in the second volume. In the case of common species it seems hardly necessary to give in addition to the distribution the localities of all the specimens in the British Museum (which, we presume, is the meaning of *B. M.*, though it is not so stated); we think the space might be used to better purpose for more illustrations; figures are a great help to the proper understanding of descriptions, and the seventy-four provided is not a large number for so important a work, and so difficult a group. A good morphological introduction embodying the latest views and the results of modern research on these interesting little plants, the nature of which has been the subject of so much discussion, would have made the work more complete, and increased its value.

The Birds of Norway.

Bird Life in Arctic Norway. By Robert Collett, Foreign Member, Z.S., Professor of Zoology in the University of Christiania. Translated by A. H. Cocks, M.A., F.Z.S. 8vo. London: R. H. Porter, 2s. nett.

Ornithologists owe a debt of gratitude to the author of this charming essay, and scarcely less to its very competent translator, for the graphic pictures of northern bird life which it brings before them in a cheap and serviceable form. It was a happy thought that inspired Professor Collett to publish a Norwegian edition of his original essay (read before the second International Ornithological Congress at Buda-Pesth in May, 1891), since his own countrymen cannot fail to appreciate the value of his arduous labours in the elucidation of Scandinavian zoology; but it would have been a decided misfortune if the circulation of this essay had been limited to the Continent. Mr. A. H. Cocks has himself an enviable knowledge of boreal bird life, and his translation of his friend's work has been accomplished with a skill and accuracy that merit the highest praise. The preface supplies a tersely-written description of the physical features of Northern Norway, which will be acceptable even to those of us who have spent some of our happiest days in rambling over the mountain wastes included in this too brief survey. Professor Collett considers that Arctic Norway should be studied in three natural divisions, (1) the coast district and the belt of islands girding the coast up to North Cape; (2) the deep fjords of the Arctic Ocean and the adjacent river basins in East Finmark; (3) the interior plateaux of Finmark. The first chapter supplies a picturesque sketch of the

breeding habits of several species of sea-fowl, which will be perused with none the less pleasure if the details supplied are already familiar to the majority of readers. The woodcuts of the young of *Alca torda*, *Fratercula arctica*, and *Rissa tridactyla* are quite gems in their own way, being admirably executed. The staff of the Museum at Tromsö come in for some well-deserved recognition, while Professor Collett reminds us of the salient features of bird life on the pretty little island which gives its name to the capital of Finmark. Everywhere is heard, he says, the wild cry of the Fieldfare (*Turdus pilaris*); in every meadow we commonly find a pair of Blue-headed Wagtails (*Motacilla flava*); from the thickets in the gardens, and from the willow bushes in the moister places may be heard the best songster of the Arctic district, the Blue-throated Warbler (*Cyanecula suecica*); Redwings and White Wagtails, Bramblings and Mealy Redpolls are equally characteristic of this region. The second chapter deals with bird life on the Porsanger Fjord, which affords Professor Collett an excuse for detailing the habits of the lovely little Red-necked Phalerope (*Phalaropus hyperboreus*). "Often they may be found in flocks on the sea far from land, rocking upon the surface in the strongest swell, like small specks of foam. But in the small tarns up in the interior of the country, between the leaves of *Comarum*, *Menyanthes*, and *Hippuris*, we may be able to find their nest, or meet with the four delicately-formed, brownish-yellow, young in down, being conducted by one of the parents among the water plants. This one of the parents is, as is well known to the majority of my readers, always the male. With the utmost indifference to danger, he runs, anxiously screaming, directly in front of our feet, to divert our attention from the eggs or the small young. The females on the contrary, which are a little larger, and purer coloured, keep themselves to themselves during the nesting season, and generally form the little flocks which we have seen floating about on the small pools of water, or in the sea close to shore, far removed from the burden of family life. Here the plainly-coloured male is the weaker sex, which must wholly and entirely undertake the hatching of the eggs, and the bringing up of the young. This trait is by means peculiar to this species, although hardly in any other case to so great an extent as the present, where it also takes expression in the colour of the male. It is more or less conspicuous with most of our arctic waders of the Stint and Sandpiper families." Many other paragraphs are equally to the point, and will be read with keen enjoyment by everyone. Before taking leave of Professor Collett's delightful pamphlet, which we lay aside with unfeigned regret, it is only right that attention should be drawn to the brief, but important, appendix furnished by Mr. A. H. Cocks at the suggestion of Mr. Sclater. Herein we find an admirable list of the Birds of Norway, very properly arranged in accordance with the rules of the B.O.U. The total number of birds, we are told, hitherto found in Norway is 278, while no fewer than 212 species have nested, or are believed to have nested, in that country. But, although the number of breeding species is so large, the number of non-breeding visitors is only estimated at 66; of these, no fewer than 40 are accidental visitors—a curious contrast to the avifauna of Great Britain. In view of a second edition being required, as we trust it speedily may be, we would suggest to Mr. A. H. Cocks the extreme desirability of supplying some details of the supposed occurrence of such rare visitants as Barrow's Goldeneye (*Clangula islandica*) and the Greater Shearwater (*Puffinus major*). If the occurrence of these species in Scandinavia is really fully authenticated,

there should be no reserve in withholding the particulars from British naturalists, to many of whom the transactions of foreign societies are inaccessible.

H. A. Macpherson.

For Beetle-hunters.

L'Amateur des Coléoptères. Par Henri Coupin. Bibliothèque des Connaissance Utiles. Pp. viii., 352, 217 figures. Paris: J. B. Baillière & Fils, 1894.

This is a very good, practical book. It opens with a chapter describing the various implements for the capture of beetles and giving instructions for their use. The British entomologist will, perhaps, get some comfort from the fact that his French co-worker, like himself, needs to be warned to ignore the rude remarks "des petits paysans" on his equipment and pursuits. A list of the principal families of beetles follows, with notes on the food, habits, and localities of each. A short account of their structural characters might, we think, have been added, with advantage to the reader. Then follow nineteen chapters, each devoted to some special locality for collecting; we have directions for "la chasse sous les pierres," "dans les jardins," "au bord de la mer," "dans la maison," "dans les animaux putréfiés" (an inviting locality), etc. The beetles which the collector is likely to find in each habitat are mentioned, and figures are given which will enable him to make out many of the common species. This method of treatment by localities is fresh and attractive; the worker will soon feel the need of a systematic treatment of the subject, but this, we feel sure, is a result aimed at by the author. The habits of many genera are noticed, and we are specially glad to remark that the author directs the attention of the beginner to the collection and study of larvæ and pupæ. Some coleopterists seem to ignore the fact that the beetles are metabolous insects.

These chapters on collecting are followed by one upon the preservation and arrangement of the collection. The author recommends phenic acid, if used with care, as a reliable preservative for the cabinet. He advises collectors not to mount their insects touching the drawer-bottom except for purposes of exchange with Englishmen; we are glad to think that this fashion is passing away amongst us, and that our continental comrades would now find the insects in our best collections well raised on the pins. The final chapter is devoted to "les collections picturesques." The author gives examples, with figures, of the ornamental necklaces, etc., which South American and other tribes make from the elytra of brilliant tropical beetles. He wishes that Europeans would follow the example of these savages, and make use of metallic beetles "dans les élégances féminines." Some of us would rejoice at this if there were any hope that it would stop the slaughter of birds for millinery.

For Mr. William Sikes.

Habitual criminals will turn with interest to the report on the best means available for their identification, which has just been issued by H.M. Stationery Office at a price (10d.) calculated to put it within the reach of the most unsuccessful. Mr. Francis Galton's method of identification through the impressions of the finger-tips is regarded as the most scientific and accurate, and is recommended for the final test in each case. But, owing to the difficulty of classifying a very

large collection of such impressions for the purpose of ready reference, it is suggested that the primary classification of prisoners should depend on measurements of the length and breadth of the head, the length of the left middle finger, of the left forearm, and of the left foot. The Committee are strongly of opinion that it is essential to the complete success of the registry to secure the services of an expert practised in the methods of scientific anthropometry, and, if possible, one who has had practice in training other persons in making scientific measurements. A scientific adviser would be required and would be able to settle such questions as the limits to be adopted in England for the classes of large, medium, and small, and the amount of variation to be allowed for in individual measurements; he would be able to superintend the training of warders in taking measurements, and he would instruct the officers in charge of the registry in the decipherment and classification of finger-prints. He might also be able, after experience of the working of the scheme, to suggest modifications. On every ground, then, the Committee think it desirable that the English Anthropometric Office should, from the first, have the advantage of scientific guidance not inferior to that which the French Service d'Identification enjoys in having M. Bertillon at its head.

IN the "Revue de Géographie," Paris (Jan., 1894), M. J. Girard has a paper on the Mouths of Rivers, dealing especially with modifications in form due to the velocity of the water, deposition of sediments, and waste or growth of banks. In the same number, and continued from December, Professor Malotet writes on the sandy plain of French Flanders.

MR. EDWARD STANFORD has just issued a new edition of his "London Atlas of Universal Geography." The maps of Europe, England, Scotland, and Ireland, and Switzerland, have been redrawn, a fine sheet map of London has been added, and the following maps are new: Channel Islands, Canaries and Madeira, Asia Minor, and the Euphrates Valleys, north-west frontier of India and the Pamirs, Siam, Madagascar, Argentine Republic, and New Guinea. The atlas is too well known to need any recommendation, and, containing 100 maps, is published at £12.

WE have received from Mr. Welch, 49 Lonsdale Street, Belfast, a catalogue of photographs of Irish views of geological interest. Several of these photographs have been taken at the suggestion of Professor Grenville Cole and Mr. M'Henry, who have supplied the geological information given in the catalogue. The photographs are issued as platinotypes or silver prints, and can be had in three sizes.

MESSRS. FRIEDLÄNDER & SOHN, 11 Carlstrasse, Berlin, N.W., by request of the German Zoological Society, have in hand an "International Directory of Zoologists." This will include zoologists, collectors, draughtsmen, preparers, modellers, and dealers, and will be compiled by Professors F. E. Schulze of Berlin, and H. Ludwig of Bonn. They will be glad to receive names, which should be sent at once.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments among Botanists have recently been made:—Dr. Hans Molisch to the Professorship at Prague vacant through the death of Dr. Weiss: Dr. L. Tost to be extraordinary Professor in the University of Strassburg; Arturo Baldini to be Curator of the Botanical Gardens at Bologna; Dr. Fr. Schult to be extraordinary Professor in Kiel University; Mr. M. A. Carleton, lately assistant in the Agricultural College at Manhattan, Kansas, to be assistant in the Division of Plant Pathology at Washington, D.C., where he will pay special attention to the rusts of cereals and other plants; and Mr. J. Percival, who has for the last few years been lecturing on subjects of agricultural interest for the Surrey County Council, to be Professor of Botany in the new Kent and Surrey Agricultural College at Wye. We also note that Professor Julius Wiesner has returned from his travels in Java.

THE United States Government have appointed Mr. F. L. Scribner, recently director of the Tennessee Agricultural Experiment Station, to a newly-created post chiefly connected with the investigation of grasses and forage plants. Such an official is an admirable invention; but why should he be handicapped at the outset by being labelled "Agrostologist"?

DR. KUMM has been appointed Curator of the natural history collections in the West Prussian Provincial Museum at Dantzig.

AMONG the names of those nominated by the Council of the Royal Society for election we notice those of Mr. W. Bateson, Mr. G. A. Boulenger, Mr. R. Lydekker, and Dr. D. H. Scott. It is not likely that anyone whose opinion is worth having will cavil at this year's selection. Still we cannot help regretting that no geologist has been nominated.

MR. CHARLES BARON CLARKE has been selected for the new President of the Linnean Society in succession to Professor Charles Stewart. A more satisfactory selection could scarcely have been made.

THE ruling of the Chairman that Mr. Thiselton Dyer's motion expressing approval of the Gresham scheme was out of order, has probably saved the Convocation of the London University from making a sad exhibition of business incompetence. At the previous meeting, Convocation carried a resolution referring the whole question of the constitution of the University to a joint committee representing both the Senate and Convocation. This proposal has been accepted by the Senate, and the Committee has been appointed. Mr. Thiselton Dyer's resolution simply re-opened the whole question before the Committee's report had been received. This course would have been disrespectful to its own committee, and practically a breach of faith with the Senate. The main result of this precipitate haste would have been to alienate many votes that would have been given for the resolution had it come on before the appointment of the Committee, or would be after the report is received. In the present condition of politics, no immediate steps are likely to be

taken by the Government in giving effect to the recommendations of the Gresham Committee; so that the delay can do no harm, especially as the matter is mainly one of detail; for both sides agree that a single teaching university for London is the one thing needed.

A MUNIFICENT benefactor, this time a private individual, has been found for University College, Liverpool, in the person of Lord Derby, who has given £10,000 to endow a Chair of Anatomy.

THE sum of £1,000 has been placed by the Goldsmiths' Company at the disposal of the governing body of the Imperial Institute for fitting up and equipping a department of scientific and practical research. This will deal with the investigation and practical valuation of new and little-known natural products received from India and from the colonies composing the Empire. We understand that this extension of the Institute's activity will not be allowed to interfere with the social evenings and smoking concerts, or with the special "grand illumination" of the galleries every night.

ANOTHER City company, the Grocers', has offered three research scholarships, value £250 each, open only to British subjects, as an encouragement to the making of exact researches into the causes and prevention of important diseases. Meanwhile Chelsea and Victoria Street send out their protest, and the Russian moujik naturally believes that cholera is exported to him from England.

AT Oxford, Congregation has unanimously passed the resolution: "That it is expedient to establish new degrees, to be granted after a course of special study or research, to members of the University who have passed the examinations qualifying for the degree of B.A., and to persons not being graduates of Oxford who have given satisfactory proof of general education and of fitness to enter upon a course of special study." What these degrees shall be entitled is not yet decided, nor does it much matter; to have the degrees at all is a great point gained.

A STATION for the investigation of marine zoology and botany is being founded at Cumbrae, under the personal supervision of Mr. David Robertson, "the naturalist of Cumbrae."

MR. THOMAS BROCK, R.A., has finished his sketch—model for a statue of the late Sir Richard Owen—which is to be placed in the Natural History Museum. The statue will represent Owen in his robes as Hunterian Professor of the Royal College of Surgeons, and a representation of a bone of *Dinornis* will be placed in his hand to commemorate one of his most remarkable successes.

THE anniversary meeting of the Royal Geographical Society will be held on the 28th of May. The only important changes of Council expected will be the election of Major Leonard Darwin as secretary, in the room of Mr. D. W. Freshfield, and Sir John Kirk as foreign secretary, to replace the late General Sir C. P. Beauchamp Walker. The Society's awards have been distributed as follow:—The Founders' Medal to Captain H. Bower, for his remarkable journey across Tibet; the Patrons' Medal to Elisée Reclus, for his valuable service to geography, chiefly by the publication of his "Nouvelle Géographie Universelle," in 21 volumes; the Murchison Grant to Captain Joseph Wiggins, for his work in the waters of Northern Siberia; the Back Grant to Captain H. J. Snow, for his maps of the Kuriles; the Gill Memorial to G. E. Ferguson, for survey work on the West Coast of Africa; and the Cuthbert Peek Grant to Dr. J. W. Gregory, for his journey and observations in the Baringo and Mount Kenia district.

AT the annual general meeting of the British Ornithologists' Union, held on May 9, Lord Lilford was re-elected President. The report of the Council gave a

flourishing account of the finances. It was agreed that a new (seventh) series of the "Ibis" should be commenced in 1895, with the thirty-seventh volume, and that Dr. Sclater and Mr. Howard Saunders should be appointed joint editors.

THE scientific women of America have formed themselves into a National Science Club, of which Mrs. Ada D. Davidson, who was in evidence at the World's Fair (NATURAL SCIENCE, vol. iii., p. 341), is president. At the annual meeting recently held in the hall of the National Museum, Washington, she gave an address and read a paper on Trilobites. Further lectures will be given in the same place, under the auspices of the Science Club.

LETTERS have recently been received from Mr. Scott Elliott, who writes from the west of the Nyanza, and is probably by this time in Ruwenzori. He had crossed the Kagira River, so that he apparently intends to keep well to the south of Bunyoro and the country raided by Kaba Rega. The fighting with this potentate is really all in Mr. Scott Elliott's favour, as it will keep Kaba Rega too much employed to be able to raid to the south. The re-establishment of a station in Soru will also aid, and appears to be a confession of the great mistake committed by Sir Gerald Portal in reversing Captain Lugard's policy. Further news regarding the fate of Prince Eugenio Ruspoli, the son of the Syndic of Rome, has come to hand, and confirms the story of his having been killed by an elephant. The accident happened in the valley of the Omo, one of the two rivers that flow into the north end of Lake Rudolph, so that Prince Ruspoli had got further into one of the most interesting countries in Africa than any previous explorer. If adequate observations have been taken and maps prepared, Prince Ruspoli's life will not have been spent in vain. Another expedition is just starting for this district, under command of Dr. Donaldson Smith, of Philadelphia, accompanied by Mr. F. Gillat and a well-trained zoological collector. The expedition will start from Bulhar, on the Somali coast, cross the Haud to Milmil, and thence strike through the Galla country to the north end of Lakes Rudolph and Stephanie. The expedition will be so powerfully equipped that it need have little to fear from opposition by the natives. It ought to yield rich results to science. It is understood that Dr. J. W. Gregory would have accompanied the expedition had his health permitted. Mr. Coryndon, of the black "White Rhinoceros" fame, is now well on his way to his collecting camp somewhere between the north end of Lake Tanganyika and the eastern margin of the great forests of the Upper Congo.

A STEREOSCOPIC photomicrograph of the diatoms *Heliopelta* and *Coscinodiscus* forms a frontispiece to the fourteenth volume of the *American Monthly Microscopical Journal*. These pictures, which are taken by Dr. W. C. Borden, of Newport, R.I., are intended to show the superiority of stereoscopic as opposed to ordinary photomicrographs for the representation of certain microscopic objects. In the same number of the journal (vol. xiv., no. 12), Mr. K. M. Cunningham calls attention to a remarkable collection of photomicrographs, formed by Dr. Henderson, of Mobile, between the years 1860 and 1870. Most of these were taken on wet, or collodion, plates, with Dr. Herapath's ¼-inch lens, "Ross," and, after 30 years, the silver prints are, in some cases, perfectly preserved. It is to be remembered, as Mr. Cunningham points out, that these photomicrographs must have been taken by sunlight, and not with the approved methods furnished by the electric light or lamp.

WE regret to have to record the deaths of Dr. Joseph von Szabo, of Budapest, which occurred on the 10th April last; of the veteran geolcgist, August von Klipstein, who died at Giessen on 16th April, aged 93 years; and of Dr. J. Schmalhausen, the Professor of Botany at Kieff.

CORRESPONDENCE.

THE THREEFOLD DIVISION OF THE DRIFT.

IN the appreciative review of the late Professor Carvill Lewis's Glacial Geology of Great Britain and Ireland, which appeared in your May number, Professor Lewis is credited with removing, among other stumbling blocks, that of the supposed threefold division of the drift. "He showed that the old view of the threefold classification of the drift into an upper and a lower boulder clay, separated by sands, is absolutely untenable; that even within very short distances this supposed sequence is often reversed, and we may get the boulder clay in the middle, with sands above and below."

Will you permit me to point out that from a very early period of my study of the drift, my observations led me to doubt the correctness of such a classification. In all the Glacial papers published by me since 1873, and they have been pretty numerous, I have contended for the unity of the Glacial Period in Britain, and have stated again and again that the so-called Lower Boulder Clay, Middle Sands and Gravel, and Upper Boulder Clay do not represent two Glacial Periods, divided by an inter-Glacial Period, but are continuous deposits. The arguments quoted from Professor Lewis are such as I have frequently used, and that at a time when I was practically alone in my opinion. Professor Lewis told me personally that he agreed with many of my glacial views, and the evidences of the fact can be seen in the book itself.

It follows from this that there is a possibility, however remote, of an "orthodox" geologist being right, though he may think there is something still to be said in favour of submergence.

Allow me to add that I have read the work of Professor Lewis, whom I held in esteem, with great interest, and think that his field notes and elucidatory remarks are of very great value, showing extensive and accurate observation put into a form that can be used by any competent geologist, whatever may be his views. What is still more rare, they exhibit an open and truthful mind, that does not blink facts, whichever way they may seem to tend.

Park Corner, Blundellsands. T. MELLARD READE.

May 1, 1894.

MR. WILLIAM TAYLOR (Llanbryde, Elgin) writes deprecating the tone of our review of Mr. E. T. Newton's memoir on the Fossil Reptiles of the Elgin Sandstones (*suprà*, p. 305). He remarks that the Naturalists of Elgin are grateful to Mr. Newton and the Geological Survey for the labour they have bestowed upon the subject, and points out that no unconformity has yet been discovered between the reptiliferous beds and those containing Palæozoic fishes. He asks, "might not *Elginia* and *Gordonia* be Permian?" He then comments upon the lamentably frequent tendency of professionals to ignore the work of local observers, and adds: "We cannot help pointing out that Patrick Duff, of Elgin, was right about the Parallel Roads of Glenroy, while Darwin was wrong. Duff was right in saying that *Dendrodus* was the tooth of *Holoptychius*, and Owen was wrong. Professor Judd said that fossil reptiles would not be found south of a certain line of fault: Dr. Gordon, of Elgin, declared they would be so found, and he has proved to be correct."

TO CORRESPONDENTS.

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NOTES AND COMMENTS.

Mathematical Biology.

In one of his delightful "Imaginary Portraits," Mr. Walter Pater gives a rendering of the mediæval legend of the return of the golden age, as a pagan Renaissance caused by the actual return among men of a denizen of ancient Greece. There came, under the influence of the reborn pagan, a period strangely in dissonance with the times before and after. "One man engaged with another in talk in the market-place; a new influence came forth at the contact. Another and then another adhered. At last a new spirit was abroad everywhere. The hot nights were noisy with swarming troops of dishevelled women and youths with red-stained limbs and faces, carrying their lighted torches over the vine-clad hills, or rushing down the streets, to the horror of timid watchers, towards the cool spaces by the river. A shrill music, a laughter at all things, was everywhere."

It is not the kind of pagan to cause these occurrences who has been born among us. But the prevailing application of physical reasoning to Biology, so often referred to in Natural Science, might be pictured under the idea that someone who, with Democritus, had sat at the feet of Leucippus, had been working in the German laboratories and had been indoctrinating Oxford and Cambridge and London. Professor Weismann's germ-plasm in its later elaborate form is a notable instance of the new tendency. In it the whole organism is formed of units of different grades. The unit of each higher grade is a multiple of the unit of the grade next lower. The multiples of each unit are built up in some physical scheme of architectonics. Bütschli refers the structure of protoplasm and its physical properties to questions of density and surface tension. In many parts of his argument one must have a text-book of physics open to understand him. A large part of the current work upon variation is expressed in mathematical terms, while the study of growth almost necessarily involves mathematical reasoning.

In Dr. Haacke's "Gestaltung und Vererbung," a notice of which appears in another part of our columns, almost a burlesque of this tendency to mathematical reasoning appears. Again and again consecutive pages are filled with formulæ that stretch over three lines of serried signs and figures and brackets, while his new theory of heredity and development depends upon geometrical conceptions.

These physical views are to be traced chiefly in the work of morphologists, and more than once contributors to our columns have contrasted, in this respect, morphological work with the tendency towards vitalism of the physiological school of the present time. But the contrast is more apparent than real. The "vitalists" are studying the phenomena of life as things-in-themselves, as being kindred to, but not compounded of, chemical and physical phenomena. The result of physical reasoning applied by morphologists as yet has been to give a fuller idea of the individual and homogeneous nature of living matter all through the animal and plant kingdoms, rather than to explain the nature of living matter on physical principles. The new method tends not to make biology a branch of geometry, but to introduce a new science of biometry.

ANOTHER CRITIC OF WEISMANN AND ANOTHER THEORY.

IN addition to Dr. Haacke's book, we have before us two pamphlets published at Hamburg in 1894, but originally delivered as addresses, by Dr. George Pfeffer, to the Naturwissenschaftlicher Verein. The title of the first, rendered into English, is "The Intrinsic Errors of Weismann's Germ-Plasm Theory." It objects to the theory chiefly on the ground that it implies the conception that Natural Selection works in the same method as would be employed by a creator working with a definite object in view. It is certainly the case that many organisms have structures or properties by which they are saved from destruction or gain an advantage in the struggle for existence. Dr. Pfeffer thinks, and, it is true, many critics of Weismann agree, that Weismann's view of evolution implies an illogical extension of the above statement into the statement that these adaptations, protective or aggressive, have been called into existence by Natural Selection because of their utility; that, in fact, Natural Selection, on Weismann's showing, is a teleological cause. Of course, Weismann himself has expended much pains in attempting to convince, and has succeeded in convincing many, that his theory implies no such illogicality. We do not think that Dr. Pfeffer has brought forth anything that will increase the faith of the opponents of Weismann or unsettle that of his supporters. The title of the other pamphlet is more difficult to render into English. As near as may be it is "The Transformation of Species—a Process of Self-Development." It is based on addresses delivered in March, 1893, and in January, 1894. Like Dr. Haacke, he makes much of the "blessed word" equilibrium. The whole organic world is in equilibrium; a

change of the conditions anywhere throws the whole system out of gear, and when it settles again into a new position of equilibrium, everything will be slightly altered, some things much altered. Similarly, the body of every organism is in equilibrium and changes of any part are reflected over every part. The method of change favoured by Dr. Pfeffer is change of function. Changes of species, he says, occur by reason of the change of function of the species in question.

The difficulty about these, and many similar pamphlets in English and in German, is that they contain much shrewd criticism and ingenious suggestion, coupled with a complete failure to understand the difficulties of the general questions they lightly solve in a magazine article or an after-dinner speech. What we want is not general theories, but particular facts. We want to be shown in the laboratory or in the field how far and in what way changes of function, and so forth, do occur; how the changes affect, and to what extent they affect other parts of the organism; and, above all, how far and in what way these changes are inherited by the offspring of the animals or plants in question. It is the fashion to abuse Professor Weismann's views as theoretical and divorced from experiment and observation, but that is only a fashion of the ignorant. If his conclusions come to nothing, there will be left behind a large and valuable body of contributions to anatomy, embryology, and natural history. Without this side of his work, it is certain that the theory known as Weismannism would have received neither the vast body of praise nor of abuse that have been its meed.

The Normal Nature of Ammonite Abnormalities.

If Weismannism has done nothing else, it has served to instil fresh life into the study of abnormalities and variations of all kinds. This study is beset with many difficulties, and not the least of them is the absence of a standard of comparison. In the investigation of normal forms the observations of one investigator can be confirmed and corrected by those of others. In the case of abnormalities there is much more room for the error arising from personal equation, but where independent investigations lead authors to similar conclusions, it speaks well for the results. Curiously enough, this is to a certain extent the case in reference to a paper which appeared in our last number, "Can the Sexes in Ammonites be Distinguished?", by Messrs. Buckman and Bather (p. 427). After that had gone to press a paper "Ueber Ammonoiden mit 'anormaler' Wohnkammer," was received from Dr. J. F. Pompeckj.[1] It deals with Ammonites which are said to show "abnormal body-chambers," that is to say, body-chambers which in breadth or thickness are less than they should be if the previous rate of increase of the shell had been maintained.

[1] Jahreshefte Vereins Naturkunde Württ. 1894. Pp. 220–290, pl. iv.

The opinion of Munier Chalmas that this so-called abnormality was a sexual character was commented on adversely by Messrs. Buckman and Bather, who regarded it as one of the characters of old age, and we now note that Dr. Pompeckj comes to a similar conclusion. His paper also deals with the question of resorption or absorption of the body-chamber in course of growth. This idea was put forward to explain the occurrence at the same localities of very different-sized Ammonites of presumably the same species, the smaller with "abnormal" body-chambers, and the larger (the presumed adults of the smaller) with normal coiling at the diameter of the smaller. Dr. Pompeckj, however, rejects such absorption as a principle of Ammonite growth, and explains this concurrence of forms by Dr. Walther's doctrine of dispersal after death (NAT. SCI., iv., p. 245), intimating that all such specimens, whether big or little, are full grown. But there are other difficulties in connection with this, which Dr. Pompeckj's hypothesis does not seem to meet; and his conclusion (p. 289) that "an Ammonite with abnormal body-chambers is, almost without exception, to be considered as full grown," may not be readily accepted. Other opinions of Dr Pompeckj's will no doubt be challenged, and exception might be taken to the application of the term "abnormal" to that which is a normal process of growth in racial or individual decline, and which even when carried to excess, as in *Macroscaphites*, scarcely deserves to be called an abnormality. All the same, the work is a notable addition to Ammonite literature.

AUTOTOMY IN ECHINODERMS.

THE obscure processes of budding and regeneration are at least as interesting as the nature of variations. It is a well-known text-book fact that Echinoderms have considerable power of recuperating external injuries, to such an extent, indeed, that a single arm torn off a starfish is said to reproduce the whole animal. It is also generally believed, and in some cases known to be true, that these animals can of their own accord break off or eject portions of their body, which may, in a few instances, form new individuals. We wish to direct attention to two observations, which, though not very recent, seem, owing to the places of their publication, not to have attracted the attention they deserve.

In the fifth volume of the *Proceedings of the Liverpool Biological Society* (p. 81), Mr. H. C. Chadwick describes how he placed three individuals of the holothurian, *Cucumaria planci*, in a jar, and how a strange thing happened to each of them. The middle portion of the body was pulled out and became very thin, till at last the animals broke in two. Then the front halves crawled away, "leaving their tails behind them" motionless. Each front half developed a new anus, and each hinder part developed a new mouth and circlet of tentacles. Then one of the animals derived from a hinder part divided again,

so that before two months were up Mr. Chadwick had seven sea-cucumbers instead of three.

In the twenty-first volume of the Norwegian North-Atlantic Expedition, Dr. D. C. Danielssen tells us that among the specimens of *Bathycrinus carpenteri* dredged, a large number were found to have thrown off the part of the crown that lay above the basals, and in several instances to have reproduced it again. He further points out that, while the other specimens dredged had genital products developed in their pinnules according to the normal method of sexual reproduction, the crowns that were thus thrown off were sterile; and he suggests that, when the sexual powers of the old crown are worn out, it is rejected, and a new one with fresh powers grown in its place. Now, if we remember that the most recent investigators concur in deriving the genital rachis of the crinoid arm from an extension of the so-called "dorsal organ," which lies between the basals, then we see that this recuperation of the crown may be regarded as a means of obtaining a fresh extension of this dorsal organ into that part of the crinoid where alone its generative faculty is available for sexual purposes—namely, into the pinnules. This idea certainly countenances Dr. Danielssen's suggestion. At the same time, we must not forget that in certain ophiurids, as Lütken and Cuenot have observed, reproduction by fission only occurs in immature individuals, and sexual reproduction appears to ensue only when this asexual method has been many times repeated. On the other hand, it is interesting to observe that, out of Mr. Chadwick's three holothurians, two discharged a quantity of ova before fission set in. Another interesting fact may be gathered from Dr. Danielssen's account, to wit, that the stages of growth of the new crown are similar to the ordinary stages of growth in the young crinoid: the arms long remain closed, and the anus is formed at a late period. Until the alimentary canal resumes its functions, whence does the rapidly-growing animal obtain its nutriment? What can be its reserve store? Is it indebted to the dorsal organ, and is this regeneration more closely allied to development *ab ovo* than we think; or does the animal live on the blood contained in the cavities of the stem?

Few observations have yet been made on the relations of spontaneous fission to sexual reproduction in the Echinoderms, and the subject seems to afford a fertile field for investigation, not without its bearing on the deeper problems of the time.

The Depths of the Sea.

Increased knowledge of the depths of the sea will no doubt help us to a solution of problems like those we were discussing in the foregoing paragraph; but, notwithstanding the numerous exploring voyages of the past 20 years, we have still much to learn about this part of our globe.

Dr. Otto Pettersson has just published, in the *Scottish Geographical*

Magazine for June, an important paper on Swedish Hydrographic Research in the Baltic and the North Seas. Dr. Pettersson commences by stating that "the system adopted by the Swedish naturalists at the present time, in their explorations of the sea which surrounds the Scandinavian peninsula, is to despatch a number of ships simultaneously from different ports across that part of the sea which is to be explored, each ship being provided with a complete set of hydrographic instruments, worked by assistants specially trained for their task in the laboratories of Stockholm's Högskola, the Polytechnic Institute, etc. The route of each ship, and the position of every sounding station, are determined beforehand, according to previous experience. In every successive expedition the same sounding-places are chosen, in order to ascertain the alterations which have occurred in the arrangement of the water-strata. The advantages of this method of research are obvious. All the observations being taken within a few days, the hydrographic state of a certain part of the sea at all points is exhibited practically simultaneously, unaffected by changes of wind and weather." Surveys of the Baltic have been undertaken in 1877, 1878, and 1879, but the more systematic surveys of which this paper treats were taken between 1890 and 1893.

After treating in detail, and with illustrations, of the special apparatus used, Pettersson gives specimens of the results obtained as regards temperature at various depths in the Gullmar Fiord, the deepest depression of the Baltic (S.S.E. from Landsort), and the deepest part of the Aaland Sea. In the Gullmar Fjord temperature ranged steadily downward from 17·50 C. to 4·60 C. (60 metres), then rose again gradually to 5·04 C. (120 metres, bottom). In the Landsort series the temperature marked 13·06 C. at the surface, 2·15 C. (60 metres), rose again gradually to 4 C. at 280 metres, to sink again to 3·95 C. at bottom (400 metres). In the Aaland Sea 8·05 C. was registered at the surface, 1·80 at 30 metres, 2·95 from 40–70 metres, and then a steady fall to 1·65 at 265 metres (bottom). Full instructions are given for the method of obtaining analyses of gases dissolved, and the determination of the total carbonic acid contained in sea-water, as well as chlorine titrations, specific gravity, and salinity. A description is then given of Pettersson's Plankton apparatus, the results obtained with which showed that water-layers of different origin and different depths contained different and characteristic forms of animal life. For instance, on 2nd August, 1893, the biologists accompanying the expedition distinguished three strata of water in the Gullmar Fjord, which showed marked differences with regard to temperature and salinity, and marked differences in the fauna. The detailed results are given in the pages, but space precludes our inserting them here. Dr. Aurivillius is now engaged on an investigation of the animals obtained, and we await his results with interest. The final paragraph of this interesting paper is

as follows:—"The Plankton obtained in November was very different from that which we found at the same place and depths six months before. Suffice it to say, that in quantity the winter Plankton was far in excess of that of the summer; that the vegetable matter seems to predominate in the colder season; and that abundance of living organisms were brought up from a depth of 70 metres in November, while no Plankton was found at the same depths in August. In the middle of the Skagerack the character of the Plankton at 30 and 40 metres is different from that at 10 metres, a fact which could be predicted from the hydrographic conditions of the central part of the Skagerack."

The "Quarterly Review."

The Swedish explorations of the ocean floor lead us naturally to the ocean surface. The current number of the *Quarterly Review* has an intelligent article entitled "Ocean Meadows," in which is given an interesting popular account of surface flora and fauna. The account is based largely upon the revision of those wide-spread algæ, the heterocystic Nostocaceæ, by MM. Ed. Bornet and Ch. Flahault, and on Maurice Gomont's monographs on the Oscillaria. The writer points out the many problems of pelagic life still waiting solution—problems that have not only a scientific but a great economic interest. For without question the "unvintagable sea" is one of the greatest food sources of the land. The fisheries of the world are capable of almost indefinite extension: but this extension can proceed only when we have sufficient knowledge of the feeding and breeding of food-fish to allow us to take full benefit of this supply of food without disturbing breeding grounds or driving fish to remote inaccessible haunts. The writer urges that further work be done. "No costly equipment is needed. The use of a cruiser (of dignified speed only) would, no doubt, be furnished by the Admiralty for a brief period, while the Government grant administered by the Royal Society is often spent with less return than an investigation of this kind, costing a small portion of its annual amount, would yield. Let the fitting men come forward and demand it."

Whether or no the precise method recommended by the "Quarterly Reviewer" be the most excellent, we rejoice to see a claim long advocated in scientific journals so ably enforced by a journal of which none can say that it is prejudiced by too strong a predilection for things scientific. The writer of the *Quarterly Review* article might have strengthened his case by reference to two elaborate monographs dealing with oceanic matters, and both recently noticed in Natural Science. The results published by Dr. Brooks in his Monograph on Salpa (*see* Natural Science, vol. iii., pp. 222–225, and vol. iv., pp. 457–462) bear in many important ways upon economic problems. Dr. Brooks was aided by his Government, for

the material was collected by the United States Fish Commission. The beautiful monograph by Mr. Savile Kent on the Great Barrier Reef (*see* NATURAL SCIENCE, vol. ii., pp. 453–460) is of at least equal economic importance, and serves the better to point the present moral, in that the expense of the exploration and of the publication of the volume was largely borne by the Government of Queensland.

Another article in the same issue of the *Quarterly* is of greater length and equal scientific interest. It makes a demand, not upon the pockets of the ratepayers, but on the fortitude with which they shall see the breaking of an old idol. It has been one of the most popular of our beliefs that Shakespeare was as faithful a depicter of birds and beasts as he was of human nature. But the generation of critics who have palmed upon us this belief have themselves been no naturalists. In this article the "natural history" of Shakespeare is shown to consist almost entirely of literary conventions. In the majority of cases, instead of observing for himself, he has been content to borrow uncritically from his predecessors.

THE JUNGLE BOOK.

ANOTHER writer, who, if he be not read two hundred years after this as much as Shakespeare is read to-day, yet is read more to-day than was Shakespeare by his contemporaries, has written much of animals. But, in addition to the fact that discussing the "Nature writing" of our greatest English writer leads us to the "Nature writing" of our own days, we make no apology for directing the attention of all naturalists who have still that good fortune before them to Mr. Rudyard Kipling's new book. It is an old and not unfounded reproach that too close a scrutiny of the mechanism of the organic world tends to destroy for anatomists the romance of the living world. For them too often Nature becomes an explicable or partially explicable mechanism and an animal a bundle of tissues to be dissected, or the mere seat of functions to be explored and measured. A great novelist once said that the best novels were Nature seen through a temperament. Here is a book about animals seen through a temperament, very different from that generally in association with biological investigation. It is written in the spirit of primitive races, to whom the beasts and the birds of the field and the forest are a people, with their own language and customs, for whom the woods and the plains are full of the mysterious glamour of life. The book tells of the battles and the alliances of animals, of the noon-day heat of the plains, and the stealthy bustle of the night. Keen observation and the artistic method employed by Mr. Kipling in his stories of man do not fail him in these stories of animals, nor is there wanting evidence of that peculiar turn of mind by which Mr. Kipling sees in the canons of army life the supreme standard of moral excellence. But criticism, as the author himself would say, "is another story." Of the tales in the "Jungle Book," two in especial

delighted us. In an earlier volume, under the title "In the Rukh," Mr. Kipling described how an Englishman, serving under the Commissioners of Woods and Forests in India, found an ideal native ranger. This was the foster-brother of wolves who, having grown up among the beasts of the jungle, had learned all their secrets and had been sworn into alliance with them. Here is the story of his up-bringing: of how he was taught by Baloo, the bear, and Bagheera, the black leopard, and of how he fought and conquered Shere Khan, the man-eating tiger.

The other story that attracted us is laid in very different surroundings. It tells of the lives of seals, of their breeding-places, and of their molestation by man.

In this volume, where animals are personified and made to tell their own stories, it is of no value to trace out the exact degree to which zoological accuracy has been carried. In the main the characters of the animals are displayed so that they need not disturb the equanimity of the most sensitive taxonomist. The general reader will get a picture of the chief animals dealt with, such as all the details of the taxonomist would never give him.

The Education of Sally.

Mr. Rudyard Kipling has written several stories of monkeys, but we doubt if he has studied them so carefully as the late Professor Romanes, who successfully conducted the education of the chimpanzee at the Zoological Society's Gardens. Although a full account of this was published in the *Proceedings* (1889, p. 316) of the Society, our readers may find it interesting to have the matter recalled. Sally's education was carried on under considerable difficulties, as the constant presence of visitors distracted her attention, and required an unusual amount of patience in her teacher. She was first taught to associate actual numbers with the names for them. This was done by asking her repeatedly for one straw, two straws, or three straws; these she was to pick up and hand out from among the litter in her cage. When she handed a number not asked for her offer was refused. When she obeyed correctly she was rewarded by a piece of fruit. Lastly, if two straws or three straws were demanded, she was taught to hold one straw or two straws in her mouth until she had picked up the remaining straw, and then to hand the two or the three straws together. This prevented her from being correct in her action merely by the interpretation of vocal tones.

Before long she learned exactly what was meant by the sounds of the first three numbers, and then with similar success her education was extended up to six. Beyond six she was inaccurate, but she never failed to realise that seven, eight, nine, or ten meant a number of straws greater than the lower numbers.

Professor Romanes believed that the result of these experiments

did not imply that in "counting" the ape employed any system of notation. "We know," said he, "from our own experience, that there is counting and counting, *i.e.*, distinguishing between low numbers by directly appreciating the difference between two quantities of sensuous perc eption, and distinguishing between numbers of any amount by markin g each perception with a separate sign." In the case of some persons, he went on to say, the former method of perception could reach so far as the number twenty. It was this method that he imagined the ape to exhibit, and as the perceptions of such an animal are naturally less proficient than those of man, it was natural that above six or seven vagueness should begin, while, at least, it was perceived definitely the number was greater than those correctly interpreted.

Professor Romanes went on to attempt to make Sally associate colours with their names, but although Sally could distinguish between white and coloured straws, she could not be taught to distinguish between any of the colours. Hence he came to the remarkable conclusion that Sally was colour-blind, although of course he had no means of deciding whether her colour-blindness were only an individual defect.

The Bird's Foot.

In Mr. Finn's interesting paper on the bird's foot, published in our last number (NATURAL SCIENCE, vol. iv., p. 453), the writer assumes, and most ornithologists would agree, that the typical number of digits in the bird's foot is four. In support of this he refers to Forbes's discovery of the rudiment of a hallux in three-toed birds like the albatross. But he did not mention an important piece of evidence discovered, or at least noted, by Garrod, and pointing in the same direction. The chief muscle of the hallux is a flexing muscle named the *flexor longus hallucis*. In all cases where the hallux is absent, this muscle still persists with a well-developed fleshy belly and a separate tendon, the latter uniting with the deep flexor tendon of the other toes. This, certainly, is strong evidence, were more evidence wanted in favour of three-toed birds being derived from a four-toed stock. But viewed merely as a rudimentary organ, it is not so interesting as at first appears; for in the great majority of birds, and in all near allies of three-toed birds, the tendon of the flexor of the great toe is connected by a tendinous slip with the common deep tendon of the other toes. So it must be inferred that the *flexor longus hallucis* of three-toed birds remains, not as a mere rudiment, but because it is functional, as an accessory to the flexor of the other toes. A three-toed passerine bird would be interesting, because in passerines there is no connection between the flexor of the great toe and the common flexor of the other toes. But there is no three-toed passerine.

The Colours of Birds' Eggs.

The value to be assigned to the colour of eggs in classification, is a point in which the personal equation plays an enormous part. Probably no two ornithologists would agree about many of the most common cases. Some of the difficulty, no doubt, is due to our ignorance of the natural processes by which the colours and markings are produced. Dr. Heinrich Wickmann has recently published, at Münster, a pamphlet of sixty-four pages on the origin of the colouring of birds' eggs. He states that the egg-shells of most birds consist of three parts. There is the inner elastic and fibrous layer, with which we are all familiar, and which is double, the middle calcified layer, and a thin organic cuticle, which is not found in all eggs. In coloured eggs the ground colour is to be distinguished from flecks of colour. These latter extend only a very small distance into the calcified layer, while the ground colour penetrates to the innermost part of the shell. But white eggs, says this author, are not unpigmented and white merely because chalk is white. There are separate white pigments present, and an egg of which the white ground colour is due to one pigment may be flecked with white spots of another white pigment.

He agrees with Kutter that the colours are deposited while the egg is in the uterus, before it has reached the cloaca, and gives a list of birds, including the duck, the heron, partridge, crow, falcon, blackcap, cuckoo, nightingale, and snipe, in which he has found actually in the uterus an egg with the complete normal colouring. Further, he rejects all former hypotheses as to the place of origin of the colours, and comes to the conclusion, although on what seems to us insufficient evidence, that all pigments are formed in the ovary shortly after the egg has entered the tube, and are discharged into the tube, down which they follow the egg to the uterus. He believes that the origin of the colouring matters is to be found in the blood, which undergoes katabolic changes during the formation of the *corpus luteum*. The different colours of the eggs, even of nearly-allied birds, he explains in a somewhat cavalier manner, by attributing them to slight differences in the chemical constituents of the blood of these animals. We do not think that Dr. Wickmann has left nothing for future investigators, and we regret that he has given insufficient details of his methods of investigation; but he has made a notable contribution to oölogy. In a concluding paragraph, the writer says, with some pride, that all the work was done in his private room, and that he has to thank no professor nor institution for aid.

Tadpoles.

The day has gone by for accepting Tadpoles, or indeed any larval forms, as simple survivals of an ancestral condition. We know now that the larvæ of many sponges echinoderms, marine

worms, and above all of many insects, are so modified in connection with their own conditions of life, that only in the broadest way can they be said to be more primitive than the adult forms to which they give rise. They are more primitive chiefly in that they are simpler animals, animals with less complexity of structure, and thus animals which have a broad similarity to the less complicated more ancient form of life. In the case of the larvæ of those animals which are now called *Chordata* because, though they may never attain to the possession of a bony backbone, they possess the gelatinous notochord which is accepted as the predecessor of the backbone, there is strong reason to believe that the simple structure of the free-swimming stage is misleading. The Tadpoles of Ascidians and Amphibia, for instance, possess many simple characters which we believe that the original ancestors of all the Vertebrata possessed. But this simplicity is most probably a case of convergence. Tadpoles live in water. They capture their food, escape from enemies, move about, live and breathe much in the same way and in the same surroundings as the simple pelagic ancestors of the Vertebrata. And thus in many ways they resemble these ancestors. But they are not now taken, at least by most zoologists, to be actual surviving ancestral stages.

The curious Tadpoles of the African Frog, *Xenopus lævis* (*Dactylethra capensis*), may be taken as a favourable instance of this change of view. Among the striking peculiarities of these Tadpoles are a development of pigment so slight that the nerves and blood vessels can be seen through the transparent skin; a pair of long whip-like tentacles protruding in front from the angles of the mouth, and a fish-like appearance, due to the absence of any constriction between the broad head and the body. These points, and some others, were taken to imply that the Tadpoles of *Xenopus* represented in a peculiarly intimate fashion some fish-like ancestor of the frogs. This resemblance was exaggerated in some figures published by the late W. K. Parker in an otherwise most valuable account of this Tadpole. The figures were copied in many text-books, and the fish-like appearance was still more exaggerated, some innocent markings on the back of the animal being so drawn as to resemble the dorsal plates of some extinct armoured fish.

Mr. F. E. Beddard, F.R.S., recently had an opportunity of re-investigating these forms, and the results of his work appear in the last issue of the Zoological Society's *Proceedings* (1894, p. 100). He shows how some of the mistaken impressions have arisen, and describes how, in most respects, the Tadpoles of *Xenopus* are not markedly different from the Tadpoles of the common Frog. In the first two days of their life they possess, just like the frog, a suctorial gland below the mouth, and the head is separated from the body by a constriction. After the third day these disappear, and the peculiarities of the more advanced form come into existence. The details of structure mentioned are too technical to repeat here, but the

general conclusion of Mr. Beddard's investigation is that there is no more reason to consider the Tadpoles of *Xenopus* fish-like and ancestral than in the case of any other Tadpoles.

The Botany of Tropical Africa.

The recently issued number (ser. 2., vol. iv., part 1) of the Linnean Society's *Transactions* (Botany) contains an account of the plants of Mt. Milanji, Nyasaland, collected by Mr. Alexander Whyte. At the instigation of Mr. H. H. Johnston, our Commissioner and Consul-General in British Central Africa, Mr. Whyte, towards the end of the year 1891, explored the Natural History of this mountain and district, and his collection of plants has been worked out by the officers of the botanical department of the British Museum. The region is about twenty miles to the south of Lake Shirwa, and sixty miles W.S.W of Blantyre. It comprises an isolated range of, for the greater part, precipitous mountains, many of the gullies and ravines being well wooded, while favoured nooks of the highlands are bright with gorgeous displays of flowers. Including those now described for the first time, no less than 62 per cent. of the species collected by Mr. Whyte belong to tropical vegetation. Six per cent. of the remainder are plants with a wide distribution in the tropical and subtropical regions of the world. Thirteen per cent. are North African plants, chiefly Abyssinian, while nineteen per cent. are South African. Milanji is, in fact, in the region where the floras of North and South Africa meet and intermingle with the characteristic tropical vegetation. Several Malagasy plants are also found. Of special interest are two species of *Erica*, which carry the South African heaths into the tropics, and a new conifer, of which the nearest allies are also found at the Cape. The plant in question, *Widdringtonia Whytei*, is described as a magnificent tree, reaching a height of 140 feet, sometimes with a clear straight stem for 90 feet, and a diameter of 5½ feet at six feet from the base, sometimes giving off long, straggling branches nearer the base. The timber is of a pale reddish colour, of excellent quality, and easily worked. Unfortunately, these fine trees are rapidly disappearing before the forest fires, the few left being confined to the upper ravines and valleys, a large forest finding a comparatively secure habitat in the damp gorges of the Lutshenya valley. It is pointed out that its nearest ally, *W. juniperoides*, of the Cederberg Mts., Cape Colony, seems also to be dying out, as it is now rare, though it once formed great forests. We are glad to learn that steps have been taken for the preservation of the Milanji forests, and that seedling plants are being raised with the prospect of affording increased supplies of this useful timber in the future.

Apropos of trees, the great hall at the Natural History Museum now boasts the largest section this side the Atlantic. In one of the bays on the right-hand side will be found a horizontal slice from one of the Californian mammoth trees (*Sequoia gigantea*). It measures

15 feet across, and its age, when cut down in 1892, has been computed, by counting the annual rings, at 1335 years, which means that it sprang from the seed in 557 A.D. It is somewhat appalling to think of the ages required for the action of Natural Selection in forms where the life of the individual is prolonged to such an extent.

THE MUSEUMS' ASSOCIATION.

THE Museums' Association may be said, in fashionable phrase, to have arrived, when so eminent an authority as Hofrath Dr. A. B. Meyer, Director of the Royal Zoological and Anthropologico-Ethnographical Museum at Dresden, dedicates to it a handsome quarto, with twenty plates, descriptive of the various improvements in Museum fittings that he has introduced into his own Museum. This volume, which is, by the way, the first number of a new publication, the "Abhandlungen und Berichte," of the above Museum, describes in detail and gives working plans of wall-cases, table-cases, skeleton-stands, large spirit-tanks for exhibition purposes, cases of drawers, cases and trays for eggs, nests and shells, a craniometer and a craniophore, trucks and trollies and other minor aids to Museum exhibition. In the construction of cases, trays, and drawers, Dr. Meyer's chief peculiarity lies in the exclusive use of glass and iron, which render the cases fireproof and, he contends, absolutely proof against dust. A description of some of these cases has already been given by him in the Report of the Museums' Association for 1891, and we hope that some member of that body will lose no time in publishing an English rendering of this very suggestive and practical paper.

The Museums' Association is, at this our time of publication, meeting in Dublin, and the address of the President, with which, through the courtesy of Dr. Ball, we are in this number able to present our readers, deals with the history of the Dublin Museums. Meanwhile, there has just been published in the *Proceedings of the Liverpool Naturalists' Field Club* a very similar address by their President, Mr. G. H. Morton, which relates the story of the Museums of Liverpool. The present Museum, which has reached a very high stage of development, through the exertions of the late Rev. H. H. Higgins and Mr. T. J. Moore, was preceded by quite a different Museum, belonging to the Royal Institution of Liverpool. Under the care of William Swainson, T. Stewart Traill, Henry Johnson, Francis Archer, and other well-known naturalists, the collections of this Museum assumed considerable importance, and included a large number of valuable type-specimens. "It was," says Mr. Morton, "very useful to students of natural history, and a source of instruction to the general public. For many years 30,000 persons visited it annually on the free days." But with the rise of the present Free Public Museum, and after the death of the curator, Henry Johnson, a sad state of things ensued. "Neither the proprietors nor the committee cared anything about the Museum, and it perished from

neglect. In 1877 the Mammals, Reptiles, Crustacea, Polyzoa, and Actinozoa were sold to the Corporation of Nottingham, and soon after the Birds, Mollusca, Fossils, and Minerals to the Corporation of Bootle. The Museum contained thousands of interesting specimens collected by members of families still well-known in the city, and they were presented in the hope that they would be preserved—and many were well worth preserving—but the labels and all particulars have been lost and nothing is remembered about them." Mr. Morton does not draw any moral, but it is plain enough. First, co-operation and not competition is wanted in the scientific world; neither men nor money are enough to support more than a very limited number of scientific institutions, even in one of the richest cities of the world. Secondly, if proprietors and committees are unable to recognise the public and national, or rather international, nature of their trust in the preservation of scientific specimens, then the nation should have power to take their trust from them. The sale of these valuable collections may have been within the limits of the law, but it was none the less an iniquity and morally unjustifiable.

Sunday Opening of Museums.

Before leaving the Museums' Association, we cordially recommend its members to read Mr. Holman Hunt's recent address to the Sunday Society. Dr. Ball himself gives evidence that the Museum Curator, overworked though he be, welcomes the opening of his Museum on Sunday. Otherwise he must feel that his labour is largely wasted, since so many of those for whom it is intended have no chance of seeing it. In provincial Museums, too, where so much of the curator's work must be done in the public galleries, Sunday, the curator's holiday, is pre-eminently the day for the admission of "the great public." Such curators will read with pleasure the following extracts from Mr. Hunt's eloquent address:—

"To Christians afraid of scientific pursuit, and who looked upon art as far from deserving of confidence, he would say a word to dissipate their fears. Museums, it was true, presented startling facts to the eyes of men who, like the Mohammedan conqueror of Egypt, ordered the burning of the Alexandrian library with the verdict that all wisdom necessary for the true believer was in the Koran. Now, independent investigation into the bottomless pit of knowledge must by all scientific students be impartial, and must stop at that point where materialistic facts failed to afford further evidence. They, therefore, gave no intentional corroboration of spiritual ideas. He believed that every full-minded person who went to a Museum, and made himself acquainted with the evidences existing there of the links in the order of creation, and of their relation to earlier and later facts, had instinctively increased in him the certainty of the Author's existence, and of His grandeur and of His all-sufficiency to bring about justice and mercy. One most important lesson taught in all Museums, whether the example looked at be a work of nature or of art, was in the demand it had made for long-continued patience. Every fossil spoke of its adaptability to its conditions when in life, as having been produced

by the exercise of steady overcoming of difficulty, and it marked a gradual step towards higher perfection; and so every work of art—and in this class they might include mechanical contrivance—declared as traceably the patient control of the Maker's energies. . . . The exquisite work of all ages, which all might see in Museums, was the surest spur to ambition, and this in its full force made all pains and patience well spent in the accomplishment of good work, and made the artist of every kind turn with disdain from encouragements to mediocrity, which were certainly among the dangers of our day."

DARWIN AND THE Y.M.C.A.

DURING the recent assembly of Christian young men in London, the Rev. James Hastings, M.A., of Kineff, N.B., was reported by the *Daily Chronicle* to have made the following remarks:—

"This is the age of specialists and dwarfs. Darwin is the best example, and I refer to him just because his case is so well and widely known. You remember his confession: that he had spent himself so long and so exclusively upon insects, that the avenues of the soul into which God and music were wont to enter had become closed for ever. This seems to be Darwin's contribution to the knowledge of the world, that responsible men may lose their organs if they refuse to use them."

We do not think of accusing the Rev. James Hastings, M.A., of Kineff, N.B., of bearing false witness, for it is plain that that good man has never troubled to look into Darwin's writings for himself; but it is interesting to notice the evolution of this myth, now "so well and widely known." It was founded upon a few lines in the autobiographical part of Darwin's "Life and Letters." The complete passage is too long to quote; anyone may find it in the first volume of the "Life and Letters," pp. 100–102. Darwin says that he lost the taste for music and poetry and pictures which he had up to about the age of thirty, but that novels remained a wonderful relief and pleasure to him, especially if they had a happy ending. He suggests that his mind had become a "machine for grinding general laws out of a large collection of facts," and that this may have caused an atrophy of that part of the brain alone upon which the higher tastes depend. Of course, there was no word about the soul or God, and as for music, one may read a few pages further on that he never had any ear for music, and that although, like many other people with a similarly defective ear, he enjoyed hearing many tunes, he was never able to recognise an old favourite.

It was that astute ecclesiastic, the late Canon Aubrey Moore, who first laid hold of this passage for controversial purposes. He himself knew well, and did much to combat the view, that Darwinism and infidelity were interchangeable terms, but apparently he could not resist the temptation of suggesting that poor Darwin was devoid of a faculty that the Canon and other ecclesiastics possessed. It was left for later and narrower-minded persons to work up the idea into its present form.

I.

Some Shell-boring Algæ.

A NOTE in the February number of NATURAL SCIENCE (1), to the effect that botanists seemed inclined to keep to themselves information on the subject of shell-boring algæ, suggested that it might be of interest to readers if some account of the results of the investigation of these plants by MM. Bornet et Flahault (2) were forthcoming. The gradual disappearance of the empty shells of bivalves and other molluscs has, until recently, been regarded as the result of their disintegration by the friction produced by the movements of the sea-water, and by the slowly dissolving action of the sea-water, charged with carbonic acid. No doubt this explanation holds good in part, especially for localities where the sea is "wild." In quiet, more or less land-locked bays, another, and to the naturalist much more interesting, cause is now known to contribute to the disappearance of the empty molluscan shells. During the last fifty years many naturalists have made known the presence of tubes of various kinds, branching, and penetrating in different directions, into the substance of fossil and recent corals, fish-scales, and molluscan shells. The late Professor M. Duncan (3) gives, in "On some Thallophytes parasitic within recent Madreporaria," a summary of earlier investigations of the subject, and adds considerably to it himself. Unfortunately, the botanical aspect of the question is by all ignored or misunderstood, the tubes are considered to be parasites living on the organic matrix of the shell or coral, and are of interest chiefly as illustrations of the persistence, through long ages, from the Silurian to the present time, of identical or almost identical lowly organisms. Thus the boring tubes are ascribed to "*Saprolegnia* or *Achlya*, a confervan, a fungus with green coloring matter, if indeed, *Saprolegnia* can be considered a fungus." Two species, *Achlya penetrans* and *Achlya* (*Saprolegnia*) *ferax*, Ktz., are recognised by Duncan, but with this reservation:—"The classificatory position of the parasite is in the midst of a group of forms which have complicated life-cycles, such as the Achlyans (proper), the Saprolegniæ, the Empusinæ and Botritidæ, and the filamentous false-root bearing genera *Codium* and *Bryopsis*—forms which are more or less the expression of one organism under different conditions and age." Professor Duncan's investigations were mainly concerned with corals, of which

he examined many recent specimens, from the littoral zone to a depth of 1,095 fathoms, from waters, with a temperature ranging from 39·7° F. to 55° F., from Davis Straits southwards. His general conclusions were that the boring tubes are more abundant in a coral, the warmer the sea-water and the less the pressure to which it is subjected, and that "the fossil corals of Silurian age were also affected by closely allied, if not specifically identical, forms." It remained for the Swedish botanist Lagerheim (4), in 1886, and more especially for the French algologists Bornet and Flahault (2), in 1889, to supply an accurate botanical account of the perforating organisms, and to point out their most important biological significance—that of shell destroyers.

In a paper by Bornet and Flahault, entitled "Sur les algues perforantes," ten genera are described and their life-histories in some cases more or less fully made known. Of these ten, five are new to science, and are confined, so far as is known, to the peculiar habitat furnished by molluscan shells. All the perforating plants agree in their general mode of development. At first they spread out horizontally in the epidermal layer of the shell, forming an irregular network or radiating round a central point. From the horizontal layer branches grow out, some at right angles, penetrating into the substance of the shell, dissolving its calcareous matter as they proceed. Other branches elongate themselves, parallel to the primary filaments. As time proceeds, these horizontal filaments become so numerous and their branches so close together, that the intervening calcareous matter of the shell finally disappears, and the plant, now in immediate contact with the surrounding water, is able to send off its reproductive cells. The surface of the shell becomes at the same time rough and irregular. It is to Bornet and Flahault we owe the important generalisation that this constant gnawing away of the shell ends in its complete destruction, and is the chief cause of the disappearance of shells in quiet bays where they are not mechanically destroyed by the ceaseless activity of the waves.

The boring plants belong mostly to the blue-green algæ (Cyanophyceæ), and the green algæ (Chlorophyceæ); one is a red alga, two are fungi, and the brown algæ are unrepresented.

The presence of a perforating green plant in a shell is easily distinguished from a mere superficial green deposit of germinating *Ulva*, etc., by scraping with one's nail, or, better, by breaking the shell in two. If the broken surface shows greenness to any depth, one may be satisfied it is due to a perforating alga. The identification of the particular species present requires, in most cases, decalcification of the shell and microscopic examination subsequently. A good deal of information can often be obtained by examination of the shell untreated, under the quarter-inch objective, or by rough powdering of the shell in a mortar. Many of the fragments thus produced stand on edge when placed on a slide, and can be examined as shell sections.

The examination of the living plant is, as the French algologists state, extremely troublesome and fragmentary. The most satisfactory method of examination is by decalcification of the shell with Perenyi's fluid.[1] This solution slowly dissolves the calcareous matter of the shell, and fixes the protoplasm of the boring plant, without destroying its colour. The three commonest and most readily recognised plants are *Gomontia polyrhiza*, *Hyella cæspitosa*, and *Mastigocoleus testarum*. *Gomontia* produces distinct green patches, which, seen with a pocket lens, appear as numerous branching green filaments radiating from a common centre. At certain times dark green specks, the organs of reproduction or sporangia, are observable on the filaments. These sporangia, recognisable readily enough, when young, as part of *Gomontia*, acquire such peculiar characters ultimately, that there is every justification for the mistake made by Lagerheim, who discovered them, of describing them as independent plants of the genus *Codiolum*. *Hyella* and *Mastigocoleus* are often found together, and also intermixed with *Gomontia*. When found pure, *Hyella* is distinguished from *Mastigocoleus* by its patches, which are smaller, more numerous, closer together, later confluent into a network which may give the shell a mottled appearance "like a Fritillaria petal." Both genera give patches of the same colour, grey, blue-black, or violet (Payne's grey); but in *Hyella* one cannot make out with a pocket lens such fine radiating filaments as compose the patches in *Mastigocoleus*. The red alga, *Conchocelis rosea*, Batt., was not known until 1892, when Batters (5) obtained it in 6—8 fathoms off the Cumbrae Islands, in the shells of *Mya truncata* and *Solen vagina*. *Conchocelis* is recognisable to the naked eye as a pink stain penetrating into the substance of the shell. Many of the genera, and the two fungi especially, give no external indication to the naked eye of their presence in the shell, and are only recognisable after decalcification and subsequent microscopic examination.

It would be out of place here to enter into a consideration of the numerous interesting botanical questions discussed by Bornet and Flahault. It must suffice to give an adapted form of their table of genera, and the characters by which they are most easily recognised.

A. Coloured Plants:—

1. Rhodophyceæ (Red algæ)
   Bangiaceæ ?

Filaments branched, septate, chromatophores pink and star-shaped, spores (?) single.—*Conchocelis*.

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[1] Perenyi's fluid: Nitric acid, 10 per cent., 4 vol.
Alcohol, 3 vol.
Chromic acid, 0·5 per cent., 3 vol.

This reagent has long been known to zoologists. I used it through their suggestion in the Plymouth Laboratory in 1889, on calcareous algæ.

A. COLOURED PLANTS—*continued*.

2. Chlorophyceæ (Green algæ).
   - *i.* Filaments segmented.
     - *a.* Filaments monosiphonous, conferva-like.
       - *a.* Joints often irregular; branches separated at the base by a wall.—*Gomontia*.
       - *b.* Joints regular, cylindrical; branches without basal wall.—*Siphonocladus*.
     - *β.* Filaments anastomosing, producing parenchymatous expansions.—*Zygomitus*.
   - *ii.* Filaments unsegmented.—*Ostreobium*.
3. Phycochromaceæ or Cyanophyceæ (Blue-green algæ).
   - *a.* Nostocaceæ.
     - *i.* Filaments very much branched, provided with hairs and lateral heterocysts.—*Mastigocoleus*.
     - *ii.* Filaments simple or little branched; without heterocysts and hairs.
       - *a.* Filaments very fine, 0·95 to 1·5 μ thick, branched.—*Plectonema*.
       - *β.* Filaments simple, 4–6 μ thick, a fresh-water plant.—*Phormidium*.
   - *b.* Chamæsiphonaceæ.
     - Trichomes composed of distinct cells, contents of which finally divide into secondary cells. Plant very refractive.—*Hyella*.

B. PLANTS COLOURLESS, appearing to belong to the *Fungi*.
1. Filaments very fine, straight, uniform, unsegmented.—*Ostracoblabe*.
2. Filaments irregular, presenting globular swellings.—*Lithopythium*.

Perforating Thallophytes (algæ and fungi) have been found in foraminifera, corals (recent and fossil), calcareous pebbles, fish-scales, and molluscan shells (empty and living), and may yet be found in calcareous rocks. They are to be found from the littoral zone to a depth of 1,095 fathoms, from the Arctic regions to the Equator, and 14° S., and at Cape Horn. They occur in sea- and in fresh-water. Some genera are extremely rare, having been seen on one occasion only. Others are very common. Thus, on the northern shore of Dublin Bay I should have no great difficulty in obtaining a thousand specimens of the razor-shell and others attacked by *Gomontia*, and, in all stages of disintegration, as a result of its boring activity. There can be no doubt that there is every probability of large additions to our knowledge of the group by painstaking inquiry, both in the completion of the life-histories of the forms already made known, and in the discovery of new species.

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T. JOHNSON.

The Laboratory, Glasnevin, Co. Dublin.

II.

An Address to the Museums' Association on the Museums of Dublin.[1]

ON behalf of those who, some months ago, formed themselves into a Reception Committee in order to make arrangements for this visit of the Museums' Association to Dublin, I have much pleasure in offering you a hearty welcome. Our only cause of regret is that more of the members have not found it possible to visit us on this occasion. Indeed it has been a great disappointment to those who have laboured to make this meeting a success to have received letters of apology from several whose urgent engagements have prevented them from being present with us to-night.

For the honour you have done me by electing me as your President for this year, I beg to offer you my sincere and grateful thanks.

The addresses of several of my predecessors have dealt with the general principles which should govern the administration of Museums; on the present occasion I propose to describe what may be referred to as the evolution of a Museum which has a very wide and comprehensive range—a wider range, perhaps, than any other Museum, putting the British Museum out of the question as being beyond comparison, and scarcely excepting even the great National Museum of America at Washington; for although our affiliated Museum in Edinburgh is in some respects more extensive, it does not include within its walls an archæological collection similar to ours.

In many cities, notably in Berlin, the process of decentralisation or specialisation of Museums has been carried to a considerable extent; and, while there may be much to be said in favour of specialisation where there are extensive resources to draw upon, on the other hand where this is not the case it cannot be denied that there are certain advantages to be obtained from centralisation.

In a comprehensive Museum no one section is wholly dependent on a single officer and a small staff, while all may derive advantages from co-operation. Uniformity and continuity of method are secured,

[1] This address was delivered on the 26th June by Professor V. Ball, C.B., LL.D., F.R.S.

and all branches receive the benefits which are derivable from the workshops, skilled artisans, and plant which can alone be efficiently maintained in a large central institution.

It is a minor, but at the same time an important, matter to remember, too, that miscellaneous contributions and bequests to an institution with many branches can generally all be dealt with and provided with proper places in the various sections; but such donations to a specialised Museum are often out of place, and cannot, or at least should not, be exhibited. Moreover, there are many objects which belong to the borderlands of Science or of Art, or of some of their distinct subdivisions respectively, and for such objects, instead of having one location, it may be necessary, wherever there are distinct Museums, to have several more or less complete repetitions. It will be admitted, I think, that, generally speaking, it is undesirable that two or more Museums in the same city should overlap in their ranges by exhibiting the same or similar objects, or by competing at sales for their purchase; the advantages of that state of things being indeed restricted to the sellers, none being felt by the public at large, whether as ratepayers or visitors to the Museums.

The position of our great central National Museum here may be compared to a river into which many tributaries have poured their contents; but to prevent any misconception from the use of that term, they should rather be called contributaries. Although the sources of the main stream in this case stretch far back into a now remote past, I purpose to deal with these contributaries first, giving brief sketches of each of them, and omitting such details as it would be tedious, even if it were possible, to deal with on the present occasion.

On the subject of Provincial Museums in Ireland I shall say very little. Attached to the Queen's Colleges at Belfast, Galway, and Cork there are Museums which are primarily intended for teaching purposes, but are also open to the public. At long intervals I have seen all of them, and each, I believe I am correct in saying, represents more or less the views of the Professors respectively who have had charge of them, controlled, no doubt, to a considerable extent by the amount of funds which have been available.

There are two other Museums in Belfast, namely, that belonging to the Corporation, and that of the Natural History Society. Already these and the College Museum overlap in some departments, which is perhaps unfortunate; but that is a matter which it is not for me to comment upon further here, as I know there are difficulties connected with it, and I do not venture to suggest a remedy, though what one suggestion would be is sufficiently indicated by the remarks made above upon our centralised system in Dublin.

Minor Museums exist in Larne, Armagh, Kilkenny, and possibly in some other towns; they owe their origins to the activity of local societies or of individuals, some of whom have passed away, and although they contain objects of interest and value, they include some

others which are rather out of place, and their development is hampered by lack of funds and other causes.

Although, through the kindness of the Rev. Dr. George Stokes, I have been placed in possession of much interesting historical information regarding the Scientific Societies of Dublin in the 16th and 17th centuries, some of which appear to have possessed collections, I have not as yet had the necessary leisure for following up exhaustively the clues which he has placed in my hands, nor, indeed, could I devote in this address the time and space necessary for doing full justice to the subject; but I have seen enough to prove that the task, if undertaken, will be productive of interesting results, and that it will show that in those early times a spirit of active scientific enquiry was already in existence in Dublin, in which such men as Sir Wm. Petty, Molyneux, Marsh, Pococke, Rutty, Madden, Keogh, Berkely, St. George Ashe, Robert Boyle, and others whose fame was not merely local, actively participated. Some of their contributions to science are well-known, having been printed in the early volumes of the "Philosophical Transactions." There was, indeed, a very intimate relationship at the time between at least one of these societies and the Royal Society of London.

Trinity College.

This grand institution, which for upwards of three centuries has maintained its high position, successfully accomplished the aspirations of its founders, and secured for itself respect and admiration all the world over as a centre of light and learning, has founded within its walls, and keeps still in active operation, several Museums which, with the progress of scientific education, have been gradually more and more specialised to meet the educational necessities of the present day, as follows:—

I.—Museum of Natural Philosophy.—Founded about the year 1730. It contains a collection of various philosophical instruments.

It is in the charge of the Professor of Experimental Philosophy.

II.—Museum of Anatomy and Zoology.—The Zoological Museum was founded in the year 1777, and contains a large series of typical specimens.

The objects of most general interest in this Museum are the skeleton of the Irish giant, Magrath, and the only existing Irish example of the Great Auk.

Many of the older specimens were formerly in the private Museum of the late Dr. Robert Ball, whose collections were merged in those of the University in the year 1844, and who, up to the year 1857, was Director of the Museum. It has since been in the charge of the Professors of Anatomy and Zoology.

The Anatomical collections, formerly combined with those of Pathology, are now arranged in the galleries of this Museum.

There is a Zoological Laboratory attached, and the building is close to the School of Anatomy.

III.—ANTHROPOMETRIC MUSEUM AND LABORATORY.—A portion of the Zoological Museum has been set apart for the above purpose.

IV.—MUSEUM OF GEOLOGY AND MINERALOGY.—This Museum consists chiefly of teaching collections of fossils, minerals, and rocks, but there are some fine specimens of the Irish Elk and of Reptilian fossils.

About the year 1850 the collections of the Geological Society were deposited in this Museum. Catalogues of the Mineral collection were published in 1807, 1818, and 1850. Formerly it formed only a section of the Natural History Museum, but it is now in the charge of the Professor of Geology and Mineralogy. Its first arrangement in an instructive and most attractive form, in its present habitation, in 1853, was carried out by the Rev. Dr. Haughton during the earlier years of his occupancy of the Chair of Geology.

V.—MUSEUM OF ENGINEERING MODELS.—Founded in 1846. It contains a variety of Engineering Models illustrating the progress of invention in the steam engine, the construction of bridges, etc.

It is in the charge of the Professor of Engineering.

VI.—MUSEUM OF SURGICAL AND MEDICAL PATHOLOGY.—Is intended for students in Medicine and to illustrate the Professors' lectures. It has recently been re-arranged and placed in a more spacious building attached to the Medical School.

VII.—HERBARIUM AND BOTANIC GARDENS.—The Herbarium contains:—

I. Plants of Great Britain and Ireland.
II. General Herbarium,
Also a laboratory for research and demonstrations.

The Herbarium is in the charge of the Professor of Botany, Dr. E. Percival Wright, who has recently published an interesting account of it in the "Book of Trinity College."

The College Botanic Gardens (situated in the suburbs) contain about eight acres of highly-cultivated ground, including several glass-houses.

VIII.—MUSEUM OF MATERIA MEDICA.—Contains specimens of drugs; is in charge of the King's Professor of Materia Medica and Pharmacy.

The Library of Trinity College has of late years developed a section of Antiquarian exhibits of considerable interest. Here may be seen the Book of Kells, the original of the Irish Harp, the so-called harp of Brian Boroihme (Boru), and many other treasures.

In the year 1868 a distinguished representative of the University, the late Dr. Henthorn Todd, stated in evidence before the Science and Art Commission which sat in that year that, while not committing the University, it was in his opinion its duty to aid a National

Museum by contributing by donation, or loan, such collections as it could spare without trenching on the teaching collections proper.

This view of their position has been practically adopted by the Board, and the following collections have been transferred to the custody of our Museum.

1863. 150 casts of Irish fishes, prepared under the superintendence of the late Dr. Robert Ball.

In 1882, a portion of Captain Cook's collection and the Haliday collection of insects.

In 1883, a collection of fossils and casts of fossils of mammals and reptiles from the Sivalik Hills.

In 1888, a large number of bundles of duplicates of exotic plants.

[In 1894, weapons and implements of the Cook collection, to be deposited on loan.]

All the above collections are arranged, and are, or will shortly be, open to the public. They have been labelled, and of some portions there are detailed printed catalogues.

The Royal Irish Academy.

The Royal Irish Academy was incorporated by Royal Charter of George III., on the 28th January, 1786, for the purpose of advancing the study of Science, Polite Literature, and Antiquities in Ireland. From its commencement it was considered by the Irish Parliament to be a proper object for public bounty, and it has since continued to receive Government aid in carrying out its work.

It has been said that a remark by Sir Walter Scott animadverting on the fact that Ireland contained no National Collection, although abounding in Antiquarian remains, was connected with the inception of the Academy's Museum. Be that as it may, we find that on the 24th June, 1839, a real beginning was made by the presentation to the Academy of the Cross of Cong by Professor MacCullagh, and of two gold torques, found thirty years before at Tara, and presented by Dr. Petrie, on behalf of the subscribers who had purchased them for this purpose. In their Report, dated March, 1841, the Council state that they consider that the formation of a Museum is an object which the Academy should continue steadily to pursue. A sum of money was allotted to the Museum Committee, and reference is made to a subscription just then raised of upwards of £1,000 to enable the Academy to purchase the collection of antiquities of Dean Dawson, of St. Patrick's. Space and time fail me to describe the acquisition by the Academy of the Petrie and other collections which were subsequently added. Even Sir Wm. Wilde's Catalogue, published in the years 1861–2, which served to spread abroad the reputation of the Academy's Museum, was not absolutely exhaustive of its contents.

In the year 1877, after some correspondence, the Academy

accepted the proposition of the Government that it should transfer its Museum to the Dublin Museum of Science and Art as soon as the building should be ready to receive it, one of the conditions being that it should be retained in Ireland for the benefit of the public.

The actual transfer of the collection to the new building did not take place till after the rest of the Museum had been opened to the public on the 29th August, 1890, but as the cases were ready for the reception of the specimens on their arrival, they were placed on view and the Gallery opened to the public within a fortnight's time after the move was commenced by officers of the Science and Art Department specially deputed for the purpose; and on the 29th October the lists of the specimens were duly signed in witness of the transfer having been accomplished.

Since then much has been done with reference to the better mounting and arrangement of the specimens in this collection, which naturally, as being the special production of Ireland, absorbs a large portion of the attention of visitors, for whose benefit and instruction a very detailed system of labelling with printed labels has been commenced.

To this great and most important section of the Museum I commend your special attention.

THE MUSEUM OF THE ROYAL COLLEGE OF SURGEONS.

The history of this Museum covers about the same period a that of the College itself, *i.e.*, it was founded in 1784, and in 1795 the first catalogue was prepared. In 1820 it was decided to form a Natual History Museum, in addition to the collection of Anatomical preparations. As a very full account of this Museum exists in the "History of the Royal College of Surgeons," by Sir Charles Cameron, it is not necessary to give further details here. To our Museum it has contributed some specimens; among them may be mentioned a collection of fossil bones, and casts of fossils of Sivalik Mammalia.

But the most important contribution was its Curator, the late Dr. Alexander Carte, who, in 1851, was transferred to the Royal Dublin Society as Director of their Museum, and with it he subsequently passed into the service of the Science and Art Department in 1877.

GEOLOGICAL SOCIETY OF DUBLIN.

The first annual meeting of the Society was held on the 8th February, 1832, an address being given on the occasion by the Rev. Bartholomew Lloyd, D.D., F.T.C.D., President of the Society.

During the following quarter of a century or so, the Society acquired a collection of rocks, fossils, and minerals, which, about the year 1850, was deposited in the Museum of Trinity College, where for many years the meetings of the Society were held.

In the year 1864 the Society's title was enlarged by a "Queen's Letter" to that of The Royal Geological Society of Ireland. For the

last few years its operations have been in abeyance, no meetings having been held.

The Royal Zoological Society of Ireland.

This Society, founded in the year 1832, though not ostensibly connected with the formation of a museum, has in two capacities contributed very materially to the acquisition of specimens, besides which it may be claimed for it that it has in a very important degree served to maintain a vigorous interest in practical zoology in our midst. Here first, for instance, was the practicability demonstrated of exhibiting living pictures to the public of the subaqueous existences of the lower orders of animal life. In spite of subsequent claims which have been made, it is known to those competent to speak with authority that the late Dr. Robert Ball invented, and so far back as 1854 put in practice at the Zoological Gardens, the system of Aquaria which has since received such a wonderful development throughout the world. The original building in which the tanks were let into the wall and were aërated by an ingenious device, still exists in the gardens, though now used for another purpose. Of the two ways in which the Zoological Society has contributed to the formation of Museums the more obvious is, of course, in the supply of specimens of animals which have died in the gardens. Of them a goodly number have found their way either to the Museum of Trinity College, that of the College of Surgeons, that of the Dublin Society, or its successor the Science and Art Museum. Not only have specimens for stuffing been thus provided, but anatomical subjects, including anthropoid apes, the larger carnivora and elephants, have, in the hands of Haughton, Macalister, Cunningham, and others, afforded most important results. As the recipient of donations and bequests of collections and valuable specimens, which have been transferred to our Museum, the Zoological Society also deserves to be mentioned. And in this connection the magnificent and unique specimen of *Plesiosaurus cramptoni*, presented by the late Sir Philip Crampton, and a fine collection of Irish birds, bequeathed by the late Thomas W. Warren, may be referred to.

Natural History Society of Dublin.

This Society was founded in 1838 for the sole object of elucidating the natural history of Ireland. It was proposed to effect this by making a standard collection of specimens and by holding meetings. The collections grew rapidly, and they were for a time open to the public; but financial pressure rendered it impossible to continue this and, at the same time, meet the cost of publishing the papers read at the meetings.

In the *Proceedings* of the Society for November 13, 1857, and *Nat. Hist. Rev.*, vol. v., 1858, there will be found an enumeration of the specimens contained in the Museum.

By the close of the year 1869 the Society was obliged, by want

of space, to adopt measures for the disposal of its collections and library, and subsequently, about the year 1880, some of the rarer stuffed birds were transferred to the Dublin Society's Museum. Since then the Society has suspended operations. But the Naturalists' Field Club is now a flourishing young society.

GEOLOGICAL SURVEY.

The Geological Survey commenced existence as a branch of the Ordnance Survey in 1832. It was placed under the charge of Captain Portlock. Large collections were made of zoological and botanical, as well as of geological, specimens. In 1844 the Government of the day detached the Geological from the Ordnance Survey, and created it into a separate branch, Sir H. T. De la Beche being appointed Director-General.

As provided for in Lord Sandon's letter of the 9th February, 1876, the collection of the Geological Survey, consisting mainly of specimens of the rocks and fossils of Ireland, together with a few British and foreign examples for comparison, was transferred from the College of Science during the year 1890, to the gallery in our Museum specially prepared for its reception. On being thus for the first time fairly spread out for examination, it became apparent that there were not a few gaps, but these have been to a large extent since supplied by the labours of the officers of the Survey, in whose custody the collection still remains. And a very interesting and instructive collection you will find it now to be.

MUSEUM OF IRISH INDUSTRY.

It was decided by her Majesty's Government in 1845—Sir R. Peel being Prime Minister—to establish in Dublin an institution somewhat on the plan of the Museum of Practical Geology in London, but more extended in its objects, as it was to embrace the general range of the Industrial Arts. Lord Castlemaine's house in Stephen's Green was taken for the purpose. The preparations of the galleries lasted from 1847–1852.

In addition to the more purely Exhibitional Department, situated in the galleries, there were at first established as portions of the institution:—

1. A Government School of Science applied to Mining and the Arts.

2. A Chemical Department, with laboratories, first termed "Museum of Economic Geology," afterwards changed to Museum of Industry. In 1853, it was transferred from the Department of Works to that of Science and Art, then newly created as a branch of the Board of Trade.

This Museum was abolished as such in the year 1865, and certain of the collections have since been transferred to our National Museum collection.

THE SCIENCE AND ART MUSEUM.

The early history of this Museum, for nearly a century and a half previous to the year 1877, when it was transferred to the Science and Art Department, forms a part of the general history of the Royal Dublin Society, and I propose, therefore, in the first place, to give a sketch of the history of the Society, with special reference, however, to the inception, growth, and development of the Museum, which constituted but one department of the Society's many-sided functions.

The Society had its origin in a private association, formed in the year 1731 by fourteen citizens of Dublin, for improving husbandry, manufactures, and other useful arts and sciences. This association was incorporated by charter of George II. in 1749, and was granted a yearly sum of £500 out of the privy purse.

On the 22nd of February, 1733, the Museum was first opened to the public. Its contents then consisted chiefly of agricultural implements and models of flax and cider mills, which, with the permission of the Lords Justices, were exhibited in a vault under the House of Parliament. The first eight meetings were held in Trinity College, in the rooms of the Philosophical Society. Afterwards one of the committee rooms of the House of Parliament was used for the same purpose.

The first regular place of meeting by the Society was acquired in 1756 in Shaw's Court, from whence it removed in October, 1767, to Grafton Street, and thence again in 1796 to Hawkins Street, where a specially built edifice afforded accommodation to the Museum, laboratories, etc.

In 1761 the Society received its first Parliamentary grant, and since that time many special, besides annual, grants have been made, which were entrusted to the administration of the Society for the benefit of the public. During the latter years of the last and the early part of the present centuries the Mineral collection received many acquisitions both by purchase and donation, and for a long time it formed the most important part of the Museum.

In 1790 Parliament directed (30 Geo. III., c. 28) that a Botanic Garden should be formed, and in 1792 a cabinet of Mineralogy was sanctioned, and in the following year a sum of money was granted (33 Geo. III., c. 13) for erecting a suitable repository for it.

In 1800 the Society received a grant from the Irish Parliament of £1,500, and for twenty years after the Union the income amounted to £10,000 (Irish currency) per annum. The resources of the Society were so far husbanded that in December, 1814, it was enabled to purchase Leinster House from the Duke of Leinster for £20,000, and in the following year the collections were transferred there. By the removal of the National Library to the new buildings in 1890, the rooms of that splendid mansion have been freed from the cases which concealed so much of their noble proportions and artistic decorations. The rooms have been restored of late years, and now present an

appearance the beauty of which you will have an opportunity of seeing during the next few days.

In 1819 the grant to the Society was reduced to £7,000.

In 1820 the Society assumed the title "Royal" on the occasion of George IV. becoming the patron, and in 1832 the Lords of the Treasury directed a reduction in the Society's estimates to the amount of £1,600, and that the apartments and collections should be rendered accessible to subscribers, who need not be elected members of the Society.

In the year 1836 a Select Committee of the House of Commons reported in favour of an extension of Leinster House, in order to afford increased museum accommodation and recommended an expenditure of £3,000 for this purpose. It was, moreover, affirmed to be expedient, in reference to future grants of public money, that it should be fully understood that the members of the Society must be regarded as trustees administering public money, and that a clear and distinct guarantee should be given by the Society that the public were entitled to the full and entire use of the property. In response to a special request by the Treasury, this view of the position of the Society was admitted by a resolution passed on the 22nd of December, 1836, and on the 11th of May, 1837, a further resolution of the Society affirmed that, in consideration of the Parliamentary grants made to the Society, it was just and reasonable that it should not alienate any of its property without the approval of the Lords of the Treasury.[2]

In the year 1840 the Irish Government entered into correspondence with the Society, pointing out that it had not carried out certain recommendations with regard to its development as a scientific institution, to which the Society replied that it was not, nor was it ever intended to be, a scientific body; but in this respect its intentions have since undergone considerable change. In 1841 the Parliamentary grant was withheld, but was restored in the following year. In the same year the Society established an Agricultural Museum; in it the principal objects were implements, seeds, models of fruits, vegetables, and veterinary preparations. In 1844 a sum of £150 was voted for the annual support of this Museum.

In 1842 a special extra sum of £300 was voted by Government to the Society for the purpose of providing gratuitous public lectures in provincial towns—a system which had been first established in 1839. The regular annual grant to the Society at this time was £5,300.

In 1843 the Society memorialised the Government for a special grant of £3,000 for the formation of a National Museum, and £4,000 for renewal of buildings in the Botanic Gardens. In this memorial it was pointed out that while the annual grant had diminished from £10,000 in 1800 to £5,300 for the current year, that to the British Museum had increased from £3,000 in 1804 to £81,000.

[2] According to a Treasury Report, issued in 1876, the public grants made to the Society between 1801 and 1835 inclusive amounted to £293,705.

Already part of that sum of £3,000 had been expended by the Society on the building for the new Museum, but the grant which was applied for was refused by the Lords of the Treasury, and the building when completed was never used as a Museum, having, in the year 1849, on the recommendation of Mr. Poynter, been handed over to the new School of Design under the Board of Trade. As it had been built by the Society, a claim for a compensatory grant was put forward, and in 1853 the Government voted a sum of £5,000, provided half that amount was raised by public subscription, for building a Natural History Museum. It was proposed to make it a truly National Museum, and an efficient adjunct to the "educational functions of the Society, and especially to the Schools of Art." The foundation of the Museum was not laid till three years later, namely, on the 7th of April, 1856, the ceremony being presided over by Lord Carlisle, who was then Lord Lieutenant. As had been fully provided by Act of Parliament (17 & 18 Vict. c. 99), its design was in harmony with that of the intended National Gallery, which was also to contain Marsh's Library.

Dargan's Great Exhibition was held on the Society's grounds in 1853, but it is no part of this account to deal with it, nor with any of the other exhibitions and shows which have been held under the Society's auspices.

In 1854 the Society first commenced to report its operations to, and was placed under the Science and Art Department of, the Board of Trade.

At about this period the Museum of Irish Industry had developed somewhat beyond its originally intended scope as a technical Museum; this led to some controversy with the Royal Dublin Society, which was intensified by the fact that the professors had been transferred from the Society to the first-named institution.

In 1857 the completion of the building of the new Natural History Museum, on the occasion of the visit of the British Association to Dublin, was inaugurated by the delivery of lectures by Sir Wm. Thompson on the Atlantic Telegraph Cable, and by Dr. Livingstone on his African discoveries.

In 1858 an Exhibition of Decorative Art was held in the new Museum, to which a loan collection was contributed by the South Kensington Museum.

In 1859–60 the Natural History Museum received important loans from the Royal Irish Academy and from the Royal Zoological Society. A well-known collection of shells, by bequest from Thomas W. Warren; donations of birds and insects from British Columbia, by Dr. (afterwards Sir) Charles Lyell; and a highly valuable collection of Arctic mammals, birds, fossils, and minerals from Captain (afterwards Admiral Sir) Leopold M'Clintock, who continued for many years to be a contributor to the collections, were also received.

Much pressure was at this time used by the Government, even

to the extent of threatening to withdraw the annual grant of £6,000, in order to induce the Society to consent to open the Botanic Gardens at Glasnevin on Sundays. At last the Society consented, and on Sunday the 18th August, 1861, the first opening took place, and the total attendance for the first twelve Sundays amounted to 78,132.

In 1862 a report on the Society was drawn up by a special committee, in which its position and its past relations with several departments of the Government were discussed, and the question as to whether it was a private body or a public institution, chartered for the administration of public funds, was specially dealt with. In this same year a Treasury Commission was appointed to inquire into the Society and other analogous institutions in Dublin, which were supported wholly, or in part, by public grants, and to report thereon.

During the year 1863 the enlargement of the powers of the Council by charter, and a proposal for amalgamation with the Museum of Irish Industry, which had been founded in the year 1845, were discussed.

In consequence of the lack of funds for additional case accommodation, very slight progress was made during the year in the permanent arrangement of the Museum. The Society presented a special memorial for the grant of a sum of money for perfecting the several institutions as follows :—

Museum of Natural History	£4,500
Library	1,000
Botanical Museum and Garden, Glasnevin	1,000
Agricultural Department	3,000
Total	£9,500

This sum, it was understood, had been considered requisite by the Treasury Commission, which had reported as above in 1862. The delay in its recommendation to Parliament arose from the fact that the question of the future position of the Museum of Irish Industry was still under consideration.

In 1864 a Parliamentary Select Committee assembled, on the 20th April, to enquire into the Scientific Institutions of Dublin which received aid from the State. One object of this enquiry was to obtain a revision of the Treasury Report of 1862, and of certain recommendations therein specified, the most prominent of which were :—

I. The amalgamation of the Museum of Irish Industry with the Royal Dublin Society, and the reduction of the staff of lecturers and teachers.

II. The amalgamation of the pecuniary grant to the Royal Dublin Society.

In 1867, Dr. Alexander Carte being then the Director of the Natural History Museum, the staff was increased by the appointment of two

Assistants, Messrs. A. G. More and W. F. Kirby, both of whom, as is well known—especially the former—did much good work for the Museum. Mr. Kirby subsequently received an appointment in the British Museum.

Towards the end of 1867 considerable progress had at last been made in the arrangement of the collections, and numerous valuable donations were received. In the following year, 1868, the first steps were taken towards opening the Museum in the evenings—one evening, Wednesday, being selected as an experiment—otherwise the Museum was only open for four days in the week.

It may be of interest to record here, in connection with the project of establishing a Department of Science and Art in Ireland, that "A Memorial from 245 Students of the School of Art, was addressed to the Lords of the Treasury, praying that, in the event of a Science and Art Department being established in Ireland, the present right of Irish Students to compete with those of England should be preserved."

In September the Council was informed that a Commission had been appointed "to consider the establishment of a Science and Art Department for Ireland." The report of this Commission contains much interesting information. It decided against the formation of a local Department, and sketched out a plan for the establishment of a State Museum.

In 1869 the question of opening the Museum on Sundays was under consideration, but action was deferred, and in fact the proposition was not carried out till the year 1884, or 15 years afterwards.

In 1870 the Department of Science and Art, pending negotiations which were in progress, declined to recommend Parliament to vote a sum for the erection of a Museum of Ornamental Art.

The acquisitions of the Museum in this year were again very considerable.

It was resolved to open the Museum on two evenings in the week (Mondays and Thursdays) and to close it altogether on Tuesdays and Fridays. Afterwards these days were altered.

The evening attendances far exceeded those in the day, having been 89,338 and 13,899 respectively, or an annual total of 103,237. On one evening upwards of 3,000 persons attended, and some confusion was caused by the consequent overcrowding.

From a report by the Committee of Natural History, it would seem that there was much dissatisfaction with the Natural History building. It was stated to be in several respects unfitted for the use to which it was applied, and was not on a scale worthy of being called national. The Committee also claimed that the officers should be put on the same footing, as regards salaries, as the corresponding officers in the British Museum.

In 1872 the *Proceedings* of the Society contained a report by

a committee appointed to consider the question of a General Museum of Science and Art in connection with the Society. It concurred in the opinion of the Commission which met in Dublin, 1868, "that the formation of a separate Department for Ireland would be detrimental to the interests of Science and Art in that country." It then refers to the proposal of the Commission that the several proposed institutions should be concentrated on one site, namely, a Science and Art Museum, a Public Library, a Museum of Natural History, a Museum of Irish Antiquities, a National Gallery and a School of Art.

Plans were prepared by which it was shown how this object might be attained on the Society's premises by the incorporation of Leinster House, and the connection of its first floor as a means of communication between the proposed buildings. The committee demurred, however, to the control of these institutions being transferred to another authority.

The Lord Lieutenant, Earl Spencer, to whom the report was forwarded, expressed himself strongly in favour of the establishment of a "General Museum of Science and Art," but considered that the plan would not be perfect without the co-operation of other societies.

The most important donation to the Museum during the year was made by Sir Richard Griffith. It consisted of his unique collection Irish fossils (Carboniferous and Silurian), which contains nearly all typical examples described by M'Coy and Salter in their "Synopsis of the Carboniferous Limestone and Silurian Fossils of Ireland" respectively. It serves to illustrate the state of palæontological science at the time of the publication of Sir Richard Griffith's large map, and is now well placed for inspection in the Palæontological Gallery.

In a report by the Council of the Society, read on the 5th of March, 1874, the delay in providing funds to meet several urgent applications, and in the provision of the new Museum, etc., is animadverted upon.

A committee appointed to report on the question of the extension of the buildings for the Museum, Library, etc., confirmed, except in some minor details, the report of the committee of 1872.

The premises of the Society were visited and inspected on the 7th of December, 1875, by the Right Hon. Viscount Sandon, M.P., Vice-President of the Committee of Council on Education, Mr. W. H. Smith, M.P., Secretary to the Treasury, the Right Hon. Sir Michael Hicks-Beach, M.P., Chief Secretary for Ireland, and Major (now Major-General Sir John) Donnelly, R.E., with reference to the increased accommodation for the Museum and Library, and the formation of a general Museum of Science and Art, and a letter was received by the Society on the 17th of February, 1876, from Lord Sandon, relative to the establishment on the Society's premises of a general Museum of Science and Art. An objection was at this time raised by the agent of the

Pembroke estate to the proposition, then current, to erect the buildings for the new Museum on Leinster Lawn. Various deputations from the Society to the Lords of the Committee of Council on Education, for the purpose of discussing the terms of Lord Sandon's letter, took place at this time.

A proposition which was made, for the amalgamation of the Royal Irish Academy with the Royal Dublin Society was put forward during the year, in the hope that it would facilitate the settlement of the pending question as to the new Museum, but it did not meet with acceptance.

The *Proceedings* contain several references to the bequest by the late Mr. J. H. Foley, R.A., the eminent sculptor, of his models of statues, busts, etc., to the Royal Dublin Society.

On November the 18th a letter was received from Mr. W. H. Smith, Secretary of the Treasury, stating that it was intended to obtain an Act of Parliament "to vest all the buildings and land now held by the Royal Dublin Society in the Government" in order "to afford the State the requisite control over property and buildings upon which a large amount of public money is to be expended."

A resolution was thereupon passed by the Society to the effect that it was desirable to facilitate the acquisition by Government of the sites necessary for the several institutions, provided that "the rights of existing members be preserved undiminished."

The volume of *Proceedings* of the early part of the year 1877 contains frequent references to the negotiations which were carried on between the Society and the Government in reference to the establishment of the Science and Art Museum.

An agreement supplemental to the provisions of Lord Sandon's letter was entered into between the Government and the delegates appointed by the Society, by which several important concessions were made in consideration of the property and rights hitherto possessed by the Society, but which were now relinquished; and accommodation for the Society in Leinster House was secured to it on the same terms as govern the occupation in Burlington House by learned societies.

By a letter of the Science and Art Department, dated 9th August, Dr. Steele was appointed to carry on the administration of the Natural History Museum, School of Art, Library and Botanic Gardens, as soon as the Bill should receive the Royal Assent. He was to have the same authority of administration as had been exercised by the Council of the Royal Dublin Society, except as regards the appointment of superior officers; and the funds for carrying on the work were to be those provided in the current estimates.

The total of the grants which had been entrusted to the Society for administration on behalf of the public, inclusive of Public Works vote, amounted, from 1801 to 1877, to £627,056; the regular income

of the Society from members' subscriptions for the same period having been about a tenth of that sum, and the subscriptions for special purposes having yielded about £4,000.

Having thus brought down the history to the year 1877, when the complete transfer to the Government of the buildings and collections was concluded by the payment to the Society of a total sum of £35,000, including compensation for disturbance of the cattle shows, which formerly used to be held on the premises, I now proceed, I fear somewhat imperfectly, because of necessity briefly, to describe what the main features of the collections were which were so transferred.

These consisted mainly of zoological specimens in the Natural History Museum which had been acquired during a long course of years by donation and purchase. There was also an extensive Mineral collection, though not up to date. There were numerous isolated Geological and Palæontological collections, for the exhibition of which but limited space had been available; these included Sir R. Griffith's collections, and several valuable collections from the Arctic regions. There was a nucleus of an Ethnographical collection, a few antiquities, and some other miscellaneous objects, besides a collection of agricultural implements, veterinary specimens, and seeds, and a Botanical collection and Herbarium in the Botanic Gardens. The Art collections consisted mainly of casts and w[illegible] colours in the School of Art, and there was the then unsecured [illegible] sion to the Foley Collection of Casts.

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The First Annual Report of the first Director of the Science and Art Museum, Dr. William Edward Steele, who for 25 years previously had been Assistant Secretary and Registrar of the Royal Dublin Society, was dated January, 1879, and it covered the period from the 14th August, 1877, when the Dublin Science and Art Museum Act passed, up to the close of 1878. It describes the initial steps taken in the formation of the Art Department of the Museum, which has since then grown to such important dimensions that it almost fills the large building completed for its reception less than five years ago. A special loan collection of scientific and art objects was prepared for exhibition in the old Museum on the occasion of the visit of the British Association in 1878.

In that year a local Board of Visitors of the Museum and Botanic Gardens was appointed, in accordance with the terms of Lord Sandon's letter above referred to.

During the year 1879 the want of space for exhibiting the growing art collections was keenly felt. Mr. Longfield was appointed to assist the Director in this department of the Museum. Among the acquisitions were the Foley casts, to which reference has already been made. For the Natural History Department the unique fossil of *Plesiosaurus cramptoni*, which had been lent to the Museum in the year 1861, was purchased from the Zoological Society. There were also many other purchases and presentations.

In 1880 the question of the site for the new Museum was under discussion, and the opposition to the proposed site on Leinster Lawn, principally by certain inhabitants of Merrion Square, led to its abandonment in favour of the other (west) side of Leinster House.

In this year the Ethnographical collection, to which some important additions had then recently been made, was taken in charge by the Art Department of the Museum, and was re-arranged and labelled. Already it formed a nucleus around which the considerable acquisitions of the last fourteen years have since been added, and it now constitutes a very important section in the new Museum building.

In the year 1881 a number of architects submitted designs for the new Museum, and some of them were selected to make detailed plans for the final competition.

In this year Dr. Alexander Carte died. For thirty years he had controlled the Natural History Museum. Among other original devices of his for the better exhibition of specimens was the beautiful arrangement of the birds, which is still in operation as he left it. It possesses manifold advantages over the shelf system, adopted in most Museums. He was succeeded by Mr. A. G. More, his senior assistant.

In 1882 unavoidable delays arose to postpone the first steps being found towards the erection of the new Museum; but by the close of the year the temporary gallery for the reception of the Art collections was nearly completed, and it was opened to the public about six months later.

Among other acquisitions which were received was an important addition to the Ethnographical collection, consisting of a portion of Captain Cook's original collection of South Sea Island weapons, dresses, etc., presented by Trinity College. A valuable donation of Sèvres China was also received from the French Government. In the Natural History Department the invertebrate collections were re-arranged, and their educational value increased by the addition of glass (Blatschka) models, geological charts, etc.

In May, 1883, the Director of the Museum, Dr. William Edward Steele, died, having devoted nearly six years of anxious and untiring service to the foundation of the Art Department of the Museum, and to the many intricate questions connected with the transfer from the Society to the Science and Art Department, of the Museum and the other institutions with which the office of the Director is connected.

In September of the same year, being at the time Professor of Geology and Mineralogy in the University of Dublin, I was appointed to succeed the late Director, and here, perhaps, I may be permitted to refer to a fragment of personal biography. For reasons I need not enter into, the position of Director of the Museum, with the numerous extra responsibilities connected with three other institutions, namely,

the School of Art, the National Library, and the Botanic Gardens, was well known to be one of some difficulty and responsibility, and my reason for accepting it, and relinquishing a post of much greater ease and freedom of action, was mainly this, that the management of a Museum of wide range had been, I may say, the dream of my life, and the occasions when I had temporary charge of Museums in India, including that of the Imperial Museum in Calcutta, which had been just then transferred from the Asiatic Society of Bengal to the Government, under circumstances similar to those I have described above, only whetted my long-cherished desire for a permanent appointment of the kind.

In what I have to say regarding the further stages in the evolution of this Museum during the past eleven years, I shall pass without mention the difficulties which have had to be met, and the controversies they have given rise to; much as they have added to our labours and anxieties, they are not matters suitable for introduction here, but the curious may find them, or some of them, recorded elsewhere.

One of the first steps which I found to be necessary was the extension of the already established workshops in connection with the Museum; and soon after a printing press was provided, by which printed labels could be speedily prepared on the spot, and the educational value and, so to speak, the self-explanatory character of the collections extended.

Shortly afterwards application was made to the Lords of the Committee of Council for permission to open the Museum on Sunday. This was immediately granted; and the proposition, it may be added, met with no opposition whatever from any quarter, but, on the contrary, was hailed with delight by the public, who have since availed themselves of the privilege in ever increasing numbers. This was in 1884, or just ten years ago, and as in the case of the three other institutions in Dublin, which were first opened on Sundays many years earlier, with two of which I am also officially connected, the movement has been an unqualified success, and a boon which is much appreciated by the public. I very well remember the terms of approbation in which it was spoken of by the visitors when they were interrogated on the first few Sundays after the first experimental opening. Among them were working men who had never been inside the walls of the Museum before, and others who voluntarily explained that their usual occupations on Sundays consisted in driving about on cars, with an occasional visit to a public-house. It may be asked by some, How about the employment of labour on Sunday? To it I can reply that no compulsion whatever has been used. The rate of Sunday pay being liberal, we find plenty of volunteers from among the staff to supply frequent changes of a sufficient number to take Sunday duty in turn. These facts have been placed before the Society in London which has the

opening of Museums and similar institutions on Sundays in view, and my sole reason for not having taken any more active part in the operations of that Society than I have done is, that my hands are already sufficiently full with my own work here. I observe, however, that the procedure of that Society appears to have made less headway against opposition than might have been expected from the time it has been in existence.

If Dublin is, as I believe it to be, fairly free from rowdyism and disorder on Sundays, I think it may be to some extent due to the large numbers of persons who are tempted to take their families to the various resorts in and near the city, where they can enjoy so much innocent pleasure and instruction as the institutions referred to can afford.

In 1884 further temporary accommodation was provided, by which the overcrowding in both sections of the Museum was relieved, and space for new acquisitions was provided. The printing of regular quarterly acquisition lists in such a form as to be capable of being cut up into labels was commenced, and has been continued ever since. This was in addition to the descriptive labels printed in the Museum press, to which reference has just been made. A slight increase to the staff of the Art Department, by the appointment of a att[illegible]hnical Assistant, was sanctioned, and the employment of experts to c[illegible]atalogue special groups though not initiated was advanced a fluen[illegible]e, so as to become a very important accession to the powers of hel[illegible] limited staff of the Museum. In this way we have since had the T[illegible]dvantage of the services of Mr. R. Lydekker on two occasions, the t[illegible]te Mr. William Davis, Mr. Kidston, and Mr. Foord, who have r[illegible]espectively prepared catalogues of and arranged portions of the Palæontological collections; while Mr. W. F. Wakeman and Mr. George Coffey have dealt similarly with the antiquities and coins; Dr. Will, of Erlangen, with the Coleoptera, and Mr. Greenwood Pim with the Lichens. Besides this, there have been volunteer experts who have arranged minor groups.

Of the advantages to be derived by Museum officers from visiting Museums in other countries than their own, I can speak from the experiences of my colleagues and myself. I hardly ever visit a Museum, no matter how small it may be, that I do not see some object of interest, or some method of exposition that is suggestive, either of imitation, or, perhaps, even of avoidance, for it is almost equally important in Museum management, as indeed it is in other affairs of life, to know what *not* to do, as to learn what it is advisable to have done!

Referring to my own visits to Museums, I derived, I think, more direct and immediate benefit from those of America and Canada, in the year 1884, than from those of the continent, including Berlin. There is an originality of conception and method, and a breadth in execution which render the Museums of Washington,

New York, and Cambridge (Boston), particularly instructive for one who had to reorganise an old, and initiate an orderly system in a new Museum. In many of the minor Museums which I visited in the States and Canada, I also acquired suggestive ideas, which have, in due course, produced satisfactory results.[1]

In the year 1885, the principal events here were connected with the completion of the plans for the New Museum and Library, and the laying of the foundation by His Royal Highness the Prince of Wales, on the 10th of April, the ceremonial connected with which having been witnessed by about 8,000 persons.

In the latter part of the same year, the lower hall of the Natural History Museum, which had previously been the receptacle of many miscellaneous objects, was reorganised, scattered collections of reptiles, fish, and fossils being brought together and systematically arranged in suitable cases; but this work was not wholly completed till the year 1890, when the fossils were removed to the gallery vacated by the Art Collection. During the same year, Mr. A. G. More's list of Irish Birds and several other publications referring to the collections were issued, and arrangements were concluded with the Science and Art Department for the publication of Miss Stokes's "Arts of Christian Ireland."

In 1886, the work of concentrating the collections in the sections assigned to them was vigorously pressed on. The general reference series of birds' skins, belonging to about 5,000 species, was provided with glass topped drawers, and a cast of the skeleton of the Megatherium was successfully mounted. An arrangement was also made by which the majority of the specimens obtained in a dredging expedition off the West Coast of Ireland, organised by the Rev. William S. Green, were to become the property of the Museum.

In 1887, the New Museum Building had acquired sufficient form to admit of a scheme of allotment of its twenty-four rooms and two courts with their galleries. This scheme, with but slight modification, has been carried out in the arrangement of the collections; needless to add it facilitated the transfer, as each object, when leaving its old quarters, had a definite destination, and so the rooms were filled concurrently, and we were able to open to the public much sooner than would otherwise have been the case.

The scheme of allotment, with lists of the principal contents of each section, will be found in the General Guide to the Museum, to which reference can easily be made, so that the details need not be given here.

A very important alteration in the casing of the Mammal collection in the Natural History Department, by which the specimens were brought forth from the darkness of cases under the

[1] See Appendix to thirty-second Report of the Science and Art Department, 1885.

gallery into the light of their present position, was commenced, but took some time to accomplish.

Preparations were also set on foot for the exhibition of Groups, showing the breeding habits of birds, and for providing maps to illustrate the geographical ranges of individual species.

Mr. A. G. More, Curator of the Natural History Department, was compelled to retire owing to ill-health, and Dr. Scharff, the present Keeper, was appointed to succeed him.

In 1888, many very important purchases were made, and the assistance of experts in the arrangement of the collections was largely availed of.

On the 4th of November, 1889, the completed New Building was handed over by the contractors, and the work of transfer of the collections was immediately commenced. Considerable purchases were made at the Paris Exhibition.

The year 1890 was memorable, from the fact that, on the 29th of August, the New Museum was opened to the public by the Lord Lieutenant, Lord Zetland, the occasion being celebrated by a suitable ceremonial, at which a large number of persons were present. In spite of the extra work thrown on the In[illegible]ff, a considerable amount of other work, such as the preparation Anne[illegible]a General Guide Book and several special handbooks, was has b[illegible]mplished.

Digit[illegible]Later on in the year, after the official opening, the collection of pos[illegible]Royal Irish Academy was transferred, as has been related on a a[illegible]evious page.

cl[illegible]In the Natural History Department, the arrangement of the d[illegible]pecial collections to illustrate geographical distribution and evolu-[illegible]ion was commenced. These now constitute a notable feature in the Museum.

The Electric Installation, commenced during the year 1890, and completed in 1891, has proved a very great success. It has not as yet been extended to the Natural History Museum, where we are most anxious to be freed from the deleterious effects on the specimens caused by the gas.

The evening opening of the Museum, though the attendances are fairly good in regard to numbers, is, perhaps, from some points of view, not so satisfactory as the Sunday opening. It is, indeed, true of both that many of the visitors are less intent upon seeing the collections than they are on meeting their friends, and the crowds of people who use the place as a promenade interfere somewhat with the comfort of those who desire to view the exhibits; but a decided improvement in the quality and conduct of the evening visitors was secured some years ago, by altering the hour of closing from ten o'clock to nine o'clock, and it is difficult to devise a plan by which mere promenaders could be excluded from an institution which is open to all respectably-clad persons. The police and attendants

cannot well do more than restrain openly unseemly conduct should it ever take place.

And here it may be stated that this Museum differs from most others in the fact that it is open free to the public for every day in the year save two, namely, Christmas Day and Good Friday, the necessary cleansing being accomplished daily before the opening hour, or after the closing hour, by the regular staff; and I think it will be admitted that the Museum presents no signs of neglect in this respect.

With our own skilled artisan staff we succeeded in suspending the skeleton of a large whale, weighing about six tons, under the roof of the Natural History Museum, where it is well seen, and no longer encroaches on valuable space, as it did when supported on pillars in the basement hall. The method of suspension can be freely commended for adoption in other Museums. Your attention may also be directed to the methods of shelving devised for the Mammals and for the Palæontological Gallery; they have many manifest advantages respectively.

During the same year we received on deposit, which was afterwards changed into a permanent donation, the life-size model by the late Mr. Birch for a proposed bronze statue of Lieutenant Hamilton. T[illegible] incident which this statue records, namely, the attack on [illegible]om Residency at Kabul, in the year 1879, is vigorously represented b[illegible] the figure of that gallant young Irishman; and the statue has bee[illegible] the may add, since its erection, an object of special attraction to mance visitors.

In the year 1891, some long-deferred additions to the strength [illegible]e the staff were at last sanctioned; these would have been still more acceptable had they been made in response to our earnest appeals before the extra pressure of the previous two years came upon us.

Under the temporary custody of Professor Johnson, which was afterwards changed in 1892 to a permanent charge, the Herbarium and Botanical Museum advanced considerably, and, indeed, in all Departments great progress was made.

The total attendances for the three years were as follows:—1889, 218,880; 1890, 313,564; 1891, 344,071; and the Sunday attendances were 1889, 40,599; 1890, 61,386; and 1891, 58,000.

In the years 1892, 1893, and 1894, the work of arrangement has also progressed in all branches, but there are no very special circumstances to be recorded here. Additions as they arrived have, so to speak, dropped into the sections provided for them, and the various arrangements for the good order and smooth working of the Establishment have operated, I believe, with satisfaction to all those who are immediately concerned. The public attendances in 1892, 343,714, were very close to those of 1891; and in 1893 they reached their maximum, namely, 367,645, or an average of over 1,000 a day, although the Sunday attendances fell from 57,536, in 1892, to 51,894,

in 1893. During the current year the total attendances promise to be very much larger.

In preparing the lines upon which this Museum should be arranged, both with regard to its then present condition and future acquisitions, we encountered from one quarter some opposition to the idea of our having any legitimate aspirations beyond those of making it a teaching Museum, which would exhibit only such types as were requisite for instruction in the various branches with which it dealt. But it appeared to us that our proper duty under the circumstances, while by no means losing sight of the educational aspect, was to steadfastly keep in view the desirability of making it a repository, in the fullest sense of the term, of all such objects as might fitly find a place in a national institution; and that, not merely in the sense of representing the productions and industries of this country of ours, but in the wider sense of providing for the nation a means of seeing and studying the productions of the world at large, and the arts of many nations, these, as examples for imitation, to be the best of their kind attainable with the means at our disposal.

While we endeavour to provide, both for our own fellow countrymen and for visitors, fitting exhibits in all branches of Irish decorative-art, antiquities, and science, we are also glad to say that our Annexts to provide exhibits from other countries have often met with has fully-expressed approval, and we rejoice now at the prospect of *Digit*[illegible] being examined by the experts whom this Association has brought together.

There is one Section which is not and cannot be largely dealt with in such a Museum, namely, that of purely industrial objects. For these, we are convinced, periodical exhibitions afford the proper medium for making them known. In such exhibitions, the exhibits can, and do, keep pace with the times, whereas in a Museum they must of necessity be in some departments far behind the progress of invention, as frequent renewals up to date would be impracticable; while in other departments it would be impossible, even with enormous financial resources, to exhibit at all. How, for instance, not to speak of minor industries, could we exhibit steam or electric plant so as to be constantly up to date?

Anxious that this National Museum might be made to spread its influence over a wider area than the metropolis alone, I sometimes dream of projects by which peripatetic loan collections might be sent out from it to provincial centres in Ireland, but so far the project is in a visionary stage without concrete form. But if it is the case that as yet collections cannot be sent out to be seen by the people throughout the land, it is a gratification to find among our visitors so many tourists, not only from various parts of Ireland, but also from Scotland, and these with the large contingents which now arrive every year from the western counties of England by way of the Isle of Man, swell our numbers of average daily admissions.

Under the existing system of administration four institutions, namely, the Museum, National Library, Botanic Gardens, and School of Art, whose functions necessarily overlap in some important respects, are linked together, and so far co-operate for mutual advantage. The books in the Library are available to those working the other three institutions, and the specimens in the Museum and Botanic Gardens are available to the students of the School of Art, while the official administrative staff is common to all.

The annual Parliamentary vote for the four institutions amounts to about £20,000, besides the vote taken by the Board of Works for maintenance. This expenditure may be regarded as a moderate outlay upon the part of the State, considering that an average of 2,600 persons visit these institutions daily, when all are open together.

I have thus conducted you through, or rather past, the controversies and labours which have resulted in the present condition of affairs; and I venture to hope that your inspection of the Museum and the other institutions during the next few days will prove a sufficient reward for your trouble in coming here, and that some of you, at least, will derive permanent advantages, carry away new ideas and also, and not least, pleasant recollections of this the first visit of the Museums' Association to Ireland.

III.

Recent Progress in Our Knowledge of Earthworms and their Allies.

THE present paper is intended rather to appeal to those who desire to be abreast of the latest information concerning a group of worms already provided with a voluminous literature, than to aim at a popular exposition of a subject hardly, perhaps, suited for such treatment. For some reason or other, the Oligochæta in general were, until a few years ago, perhaps, the least known group of any among the invertebrates; and yet they are particularly suited for investigation, and for many reasons. They are abundant, easily collected, and easy to manipulate by dissection or with the microtome. Immense efforts are made yearly, at a large cost, to bring back possibly a few skins and horns which are intrinsically as a rule of no interest at all, or of the very smallest importance. An infinitesimal fraction of the necessary expense incurred in the shooting and preservation of such material would supply many workers with abundant matter for study if devoted to the collecting and proper preservation of terrestrial and fresh-water invertebrates. To some extent this is now being done; and in the particular group upon which I report here, several collections of some importance have recently been made. Through the liberality of the Government Grant Committee of the Royal Society, I was able to secure the assistance of Mr. F. Finn to make collections in Zanzibar and Mombasa; more recently still, the Hamburg Museum sent out Dr. Michaelsen to South America, with the result that a very large collection of Oligochæta were brought home by him, and most kindly entrusted to me for study. Besides, the attention of naturalists travelling abroad with other aims has been successfully directed to the importance of this group; and in consequence a good deal of material has dribbled in to various Museums and individuals. Judging from the work of the last two or three years, which I shall endeavour to summarise here, we appear to be fairly advanced at the present moment in our knowledge of the main structural features of the group, at any rate of the terrestrial forms; the aquatic Oligochæta have been less collected, and are in a backward state so far as information goes. I shall now try to make a

connected story of recent progress, which will at least serve as a clue through the very widely scattered literature of the past year or two.

A good deal of attention has been paid to the phenomena of the distribution of these creatures, which are so important to the student of that branch of Biology. Apart from the notices of the occurrence of new species the most recent summary of that side of the study of the Oligochæta is to be found in no. 1. The difficulty which besets every person who attempts to philosophise upon the facts of distribution is to disentangle the really indigenous from the newly imported fauna. With earthworms this is apparently easier than with many groups. One may fairly speak of a "Holarctic" region embracing, of course, Europe, Northern Asia, and North America. The most prevalent genus is the common earthworm of this country (really consisting of many species), *Lumbricus*. There are somewhere about sixty species of this genus, considered *sensu lato* to include the very closely allied *Allolobophora* and *Allurus*. Nearly all of these are common to the Old and the New World; or, rather, but few of the *Lumbricus* of America are different from those of Europe. Now it is probable, in spite of the fact that this genus occurs most abundantly in gatherings from almost every other part of the world, tropics as well as the southern temperate latitudes, that the northern region is its true home. Two principal facts support this contention, which are om follows. The genus is most abundant in cultivated ground near the coast in the extra-Holarctic parts of the world; and, secondly, the sp[illegible] the found are invariably identical with those of the northern temper[illegible]ce zone; the true indigenous earthworm fauna of South America, [illegible], example, has, as a rule, to be sought for outside the towns and the[illegible]e immediate suburbs. Were the *Lumbricus* truly indigenous to thes[illegible] parts of the world, one would fairly expect to meet with species different [illegible] those of the north; but one does not. Removing, therefore, this genus from the indigenous fauna of any countries save those enumerated above, it is easy to map out the globe into regions which, on the whole, correspond with those of Mr. Sclater. The one possible addition that might be made from this point of view to his six regions is an Antarctic form; but as I have treated of that matter in a quite recent number of this Journal, I leave it aside. Earthworms possess less facilities for active migration than many animals; a land-connection seems to be nearly absolutely necessary to their wanderings. It is perhaps otherwise with the aquatic Oligochæta, which are often, in correspondence with this, widely scattered. Vejdovsky (15) and I myself (2) have found that the so-called cocoons of the small fresh-water form *Æolosoma*, distinguished by the beautifully and variously-coloured oil drops in the skin, are really chitinous cysts into which the worm temporarily withdraws itself. This is an evident assistance to involuntary migration, and it seems possible that some of the supposed "species" of this genus which have been described from Africa and America are identical with European species. The formation of

these cysts, in fact, doubles the animal's chances of being conveyed to distant places; the cocoon is, of course, the other chance. The cocoons of earthworms, being larger and often buried at considerable depths in the soil, are not likely to be accidentally attached to the feet of migratory birds, which, moreover, frequently affect the margins of lakes and pools, where the smaller cocoons of the aquatic Oligochæta would be very liable, one would imagine (there are no positive facts known to me), to be caught up and carried away. Another matter to be borne in mind is the more common occurrence here than among the earthworms of the escape of several young worms from one cocoon. The importation, therefore, of a solitary cocoon might serve to found quite a large colony in a new country. These are some of the facts and suggestions which arise from recent work upon the Oligochæta as regards the general phenomena of distribution.

But distribution has a bearing upon another branch of the subject which has received much attention—the classification of the group. The classification of the Oligochæta affords a literal instance of "Quot homines, tot sententiæ." The workers are, unfortunately, at present few, though this is mending, and everyone has his own individual scheme of arrangement; indeed, many of us have provided more than one scheme, though at fairly decent intervals. In
In empting to decide upon the conflicting claims of different families
Anne ccupy a basal position in the series, it is reasonable to be in-
has b ced to a moderate extent by the facts of distribution. It is usually
Digit d that a wide and discontinuous range indicates an archaic form.
pos nus, for instance, *Peripatus* and the Tapir. Now, if we apply this
a o the earthworms it is perhaps the genus *Perichæta* which affords the
ch most striking example. The genus has complete circles of setæ, from
d twenty to upwards of one hundred in number, according to the species,
s upon each segment of the body, and is found in Australia and India, Central America and the West Indies; it is feebly represented in tropical Africa and South America. The family Perichætidæ, may perhaps, be split up into four or five genera, but the differences are not great and there are gradations. The principal claim urged in favour of *Perichæta* as the most archaic existing form of earthworm was the condition of the excretory organs. In most text-books the typical Annelid excretory system is said to be a pair of tubes in each segment of the body, opening into the body-cavity by a ciliated funnel and on to the exterior by a pore. Researches, beginning with the year 1885, have shown that among earthworms a very large number of genera do not possess this arrangement at all. In *Perichæta*, for instance, that system is represented by a dense tufting of the body-walls and septa by innumerable coils of excretory tubes which open on to the exterior by innumerable pores scattered irregularly over the surface of the body. It has been stated by more than one naturalist that these nephridial tufts communicate from segment to segment, so that there can hardly be said to be any metamerism in an organ which in

so many Annelids is usually the most typically metameric. Bourne has, however, in his latest paper upon the earthworms of India, denied the connection from segment to segment. The matter is, therefore, still *sub judice*. From this diffuse and irregular network it has been held that the two pairs of separate nephridia per segment have been derived by reduction. Intermediate stages are offered by species with two and three separate pairs. This view of the origin of the nephridial system of the Oligochæta has, however, received a series of shocks at the hands of Vejdovsky (16), Bourne (10), and myself (3). We have all shown beyond any doubt that in the genera *Octochætus*, *Megascolides*, and *Perichæta*, genera in which the excretory system of the adult is constituted on the irregular, diffuse plan, the earliest appearance of this system is in the shape of a series of paired tubes, one pair to each segment. So far, therefore, *Perichæta* has no right to be regarded as a specially archaic form. It is, indeed, a matter of the greatest difficulty to adjust the rival claims of the existing families. More could doubtless be done if the exact relationships of the Oligochæta to other groups of "worms" could be definitely seen. In spite of the immense accumulation of anatomical detail during the last few years, but few facts which throw a light upon this most important question have been laid hold of. Something has been done to remove the barriers that were at one time supposed to divide the Oligochæta fr the Leeches. The generally unpaired median male pores of Eudrilidæ recall those of that group of Annelids; the continuity o oviduct with the ovary is another character that removes one differe between the Leeches and the majority of the Eudrilidæ; finall the same family of earthworms shows a considerable subdivision of th cœlom by the formation of sacs enclosing the spermathecæ and othe parts of the generative apparatus; this is perhaps a foreshadowing of the almost obliterated cœlom, to speak with no great exaggeration, of the Leech. In two genera, viz., *Megascolides* and *Deinodrilus*, the dorsal vessel is enclosed in a sac forming a sort of pericardium, which is a step in the same direction. Even in histological details there are resemblances now known to exist which were formerly regarded as differences. Long ago Ratzel spoke of the leech-like muscular fibres of certain Oligochæta. The exact and beautiful researches of Cerfontaine (13) have proved that, in the common earthworm—and there is no doubt that the discovery applies to other Oligochæta—the muscular fibres consist of a softer granular core and of a radiately striate denser sheath which appears to be the exact counterpart of the muscular fibres of the Leech. This particular matter, however, takes us rather further. Quite recently Hesse (14) has pointed out that in the Enchytræidæ the muscles are more like those of a Nematode; he finds that, although there is apparently this ring of denser muscular substance surrounding a granular core, a gap exists in the said ring; through this split, often hardly recognisable owing to its fineness, the granular substance protrudes and is continuous with a large nucleated

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cell. This accounts for the non-discovery of a nucleus within the muscular sheath of the fibre which was wanting to complete its likeness to the fibre of a Leech. A little more fanciful, perhaps, is my own comparison of the remarkable integumental network formed by the ducts of the paired nephridia, which ramifies through the body-wall of *Lybiodrilus* and more than one other Eudrilid, to the intra-integumental nephridial tubes of the Nematoidea; particularly to the coil of vessels which constitutes the Lemnisci of the Acanthocephala lately shown to exist in the true Nematoidea. In the Eudrilids in question the entire integument is riddled through and through with a complex system of tubes, all derived from the branching of the duct, by means of which the paired nephridia reach the exterior. There are, therefore, here as in *Perichæta* and some other forms, many external nephridial pores upon each segment of the body; but this same result is arrived at in a different way in the two cases. The most natural allies of the Oligochæta would seem to be the marine Polychæta; and yet recent researches have not tended to break down to any great extent the barriers separating these two groups. One character, however, hitherto held to differentiate the two has broken down. Cuvier called the Oligochæta as we now know them "Annélides abranches sétigères." the 1855, Grube described, unfortunately too insufficiently, a curious Nev lid from the Nile which he called *Alma nilotica*; the same species genu een lately re-discovered by Levinsen (12), who renamed it sever *branchus niloticus*. The worm has a series of tufted gills on the as terior segments. But it is not yet clear that the worm is not really di Polychæte. Apart from this dubious instance undoubted Oligochæta are now known to possess gills. Bourne was the first to iscover this important fact; he found at Calcutta a Naid having a eries of paired processes of the body-wall in the anterior segments. Later still I found in the "Victoria regia Tank" at the Botanical Gardens a worm allied to the common *Tubifex* of our streams and lakes with a series of dorsal and ventral gill-like processes upon the hinder segments; and more recently still (5) another Tubificid, belonging to a new genus *Hesperodrilus*, in which exactly the same kind of gills are present but lateral in position. Beyond these instances no Oligochæte has been discovered which places the Oligochæta nearer to the Polychæta than they were at the time when Claparède wrote. The phylogenetic arrangement of the Oligochæta must, therefore, still remain the aim of future investigation. The difficulties which surround the solution of the problem are not so much due to missing links in the chain as the inability to decide at which end of the chain was the beginning. Recent discoveries have rendered it in some cases almost an impossibility to distinguish family from family by any salient characters.

We may turn now to matters of special anatomical interest not having so great a bearing upon the inter-relationships and phylogeny

of the group. First among recent publications stands the monumental work of Vejdovsky (15) upon the embryology of *Rhynchelmis* and *Lumbricus*. One of his most important results is a clearing up of the very complicated series of excretory organs which arise at different times in the embryo. The first organ which serves this purpose is a series of tubes excavated in the interior of certain epiblast cells at the gastrula stage or even before; these cells are traversed by clear tubes which open behind into the primitive body cavity, *i.e.*, the space between the epiblast and the hypoblast. Next there is in some forms a pair of long tubes which open on to the exterior in the head region; these are the ciliated larval pronephridia, and they seem to be developed out of, or at least to have some connection with, the last-mentioned larval excretory cells. Quite independent of the last are the embryonal pronephridia; these organs are a series of paired tubes which open into the body-cavity by a "flame-cell." The flame-cell becomes a ciliated funnel, the nephridium grows, and we have the permanent nephridium of the adult. To give a thorough account of Vejdovsky's great book would take up more space than we can give; but one other point of some importance must be mentioned. He found that in the young *Lumbricus* the vascular system consists of a dorsal and of a ventral vessel which are connected by circum-intestinal vessels in each segment of the body; the dorsal vessel... Vejdovsky himself was the first to discover a good many years ...ite-cent is at first double, a state of affairs which is permanently retained in a considerable number of earthworms throughout life. The connexion of the dorsal and ventral vessels by a series of similar tubes in all the segments of the body is precisely what is met with in the simpler forms of Oligochæta such as the Tubificidæ. As still further tending to show that this is the primitive arrangement in worms which no longer show it, I may mention Bourne's discovery (11) that in *Perionyx* the young buds have a full complement of these commissural vessels which are largely wanting in the adult.

Recent investigations into the anatomy of that very remarkable family the Eudrilidæ have progressed so considerably during the last few years, that from being one of the least known they have become quite one of the best known groups of Oligochæta. They have been referred to above as suggesting a link in the direction of the Hirudinea. Other points of interest have arisen. The most marked character of the family is the presence of large, usually unpaired, sacs which open on to the exterior and generally contain sperm. For this reason they have been called spermathecæ, though their anatomical structure is not precisely that of the spermathecæ of other earthworms. The investigations of Rosa (9) and myself (4) appear to have put beyond a doubt that these large sperm-holding sacs are derivatives of the septa which acquire an external pore; they can, therefore, have no morphological relationship to the spermathecæ of, say, *Lumbricus*, though an intimate physiological relation. But the interesting point

about these sacs is that we have a series of stages in the substitution of them for the true spermathecæ. In several genera of Eudrilidæ, *e.g.*, *Heliodrilus*, there is a single median spermatheca, apparently definitely corresponding to the true spermathecæ of other worms. This is wrapped in a sac which also communicates with the ovary by narrower passages. In *Hyperiodrilus*, the true spermatheca—in this case as in the last quite independent of the envolving sac—is greatly reduced in size. Finally, we have in the majority of Eudrilidæ no true spermatheca at all, unless the sac of mesoblastic origin opens on to the exterior by means of its exceedingly reduced representative. It seems clearly a case of the replacement of one organ by another physiologically similar, but morphologically distinct. The spermathecæ are the organs which receive the sperm during congress. It is, therefore, a most inexplicable fact that occasionally they open not only on to the exterior, but into the lumen of the gut. The fact was discovered a good many years ago in the Enchytræidæ, where it often occurs, by Michaelsen. I have found that in the North American genus *Sutroa*, first described by Eisen, exactly the same state of affairs unexpectedly exists. These facts may be commended to those who are still arduously labouring in the hope of finding gill-slits in some ature which is not a Chordate. The singular relationship between res which would appear to have nothing in common, is enhanced in pre rity by the fact that in an Eudrilid, *Paradrilus*, where the In thecæ are of the mesoblastic type, precisely the same inter- re unication is met with.

So much detailed research has been carried on of late by Benham, rst, Rosa, Michaelsen, Eisen, myself, and others, that it is hard to npress into the limits of a short review like the present the results tained. I confine myself, therefore, to such as are of a wider sterest than to specialists. The last matter that I shall deal with, omitting a vast amount of rather less important though highly interesting work, is a curious case of change of function exhibited in the family Eudrilidæ, to which I have so often had occasion to refer. In this family, as in others, there are calciferous glands appended to the œsophagus, which secrete carbonate of lime. In Michaelsen's genus *Stuhlmannia*, and a good many of its more immediate allies, these glands are metamorphosed somewhat. They still open into the gut, but the lumen is greatly reduced, and does not by any manner of means extend through the entire gland. The gland is, in the main, solid, being built up of densely-associated cells, looking like peritoneal cells; the whole mass is permeated by a rich network of blood-vessels, as in the typical calciferous glands. Here and there, however, the cells in question have assumed the appearance of columnar gland cells; and when this modification has occurred, it is the invariable rule that what looks like the lumen of a tubular gland is occupied by blood. If there were not this singular change, one might be inclined merely to comment upon the fact that the glands, instead of being

formed of a much-folded membrane, had, as it were, melted into a solid ball. But the peculiar relation of the cells to blood-vessels seems to suggest a changed function, connected rather with the vascular system than with the alimentary. The whole matter is one of great interest, but would need for its proper discussion some space, which could not be fairly occupied by an article professing only to give a sketch of some of the more important recent advances in our knowledge of the structure of the Oligochæta.

The bibliography which follows is, of course, not exhaustive of the last two years; I have merely quoted those papers to which I have especially referred.

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F. E. BEDDARD.

IV.

Nearctic or Sonoran?

DR. A. RUSSEL WALLACE'S article on the mammals and birds of the Palæarctic and Nearctic Regions in last month's issue of NATURAL SCIENCE,[1] will have been read with the greatest interest by all students of the geographical distribution of animals. The tables of genera common to the two regions, or peculiar to one of them, seem to tell strongly in favour of retaining those convenient divisions of Dr. Sclater, which, by means of Dr. Wallace's own classical work on distribution, have become household words among naturalists all over the world. Yet I venture to believe that the facts re. be so read as to support another view of the zoological regions pre. ex-tropical continents.

In e only alternative to the retention of the Palæarctic and re. ctic Regions of Sclater which Dr. Wallace mentions is their ron into a single Holarctic Realm, as suggested by Dr. Heilprin in work on the "Distribution of Animals."[2] But in a recent memoir n North American mammals[3] (already referred to in NATURAL CIENCE[4]), Dr. C. Hart Merriam advocates a different and more natural view. He would unite only the "Boreal" part of Sclater's Nearctic Region with the Palæarctic, leaving the rest as a separate region, to which he applies Professor Cope's term "Sonoran." This Sonoran Region he considers no mere borderland between the Northern and Neotropical realms, but a primary division characterised by the presence of the most distinctive North American mammals.

The Boreal division of Dr. Merriam roughly agrees with the Canadian sub-region of Dr. Wallace's "Geographical Distribution of Animals." He maps it, however, as extending southward along the three great mountain systems of North America—the Alleghanies, Rockies, and Sierra Nevada, in parts as a continuous tract, elsewhere in isolated patches, some of which reach as far south as Arizona, California, and Carolina. As might be expected, however, there is no decided line separating the northern and Sonoran regions; a

[1] Vol. iv., p. 433.
[2] London, 1887 (Int. Sci. Series).
[3] *Proc. Biol Soc. Washington*, vol. vii., 1892, p. 1.
[4] Vol. iii., p. 288.

"transition" zone is shown on the map, which in places attains a breadth of over 300 miles, wherein the two faunas overlap. This zone is mostly confined to the northern and mountain districts of the United States, but it invades the British territories to the west of Lake Winnipeg, to the north of Lakes Erie and Ontario, along the southern bank of the St. Lawrence, and again in Nova Scotia.

If we now examine the distribution of the seventy genera of mammals given by Dr. Wallace as inhabiting the Nearctic Region, we find that only eight *Canis*, *Otaria*, *Bos*, *Sciurus*, *Sciuropterus*, *Spermophilus*, *Hesperomys*, and *Lepus* can be fairly regarded as ranging indiscriminately over the Boreal and Sonoran districts. Of these only one, *Hesperomys* (the Vesper Mice), is peculiar to America, and, on referring to Flower and Lydekker's "Mammalia," it appears that this should be included in the Old World genus *Cricetus*. The Sea-Lions (*Otaria*), though not Palæarctic, occur in other Old World Regions.

All other genera of North American mammals can be referred to either the northern or Sonoran fauna, even though some of them overstep the limits of the transition zone.

The Boreal fauna of Dr. Merriam comprises thirty of Dr. Wallace's genera[5] — *Sorex*, **Neosorex*, *Urotrichus*, **Condylura*, *Ursus*, *Lu*- **Latax*, *Mustela*, *Gulo*, *Trichechus*, *Phoca*, *Cystophora*, *Cervus*, *Ra*- *Alces*, *Ovis*, **Haplocerus*, **Ovibos*, *Tamias*, *Arctomys*, *Castor*, **Ha*- *Arvicola*, *Myodes*, *Cuniculus*, **Phenacomys*, **Fiber*, **Zapus*, **Erethiz*- *Lagomys*, of which only the ten marked by an asterisk are pecul American. Of the remaining twenty, all but six: *Ursus*, *L*- *Mustela*, *Cervus*, *Ovis*, and *Arvicola* are only elsewhere to be found the Palæarctic Region, while, even of these six, *Ursus*, *Ovis*, a *Arvicola* are characteristic northern genera, which can only be regard as incursors in the tropical regions where they occur. These fact seem to point strongly to the affinity of the northern faunas of the Old and New Worlds, and to require the union of Boreal American with the Palæarctic Region of Sclater. It will be possibly urged as an objection that Boreal America is admitted to be the home of ten distinctively American genera. But if the relationships of these be examined, it will be found that only two, *Condylura* and *Erethizon*, are American in their affinities. The nearest relations of all the rest must be sought in the Old World. And *Condylura* and *Erethizon*, though confined to the Boreal *Region*, should be regarded as belonging to the Sonoran *fauna*.

If the genera of mammals already enumerated be compared with Dr. Wallace's lists, it will be seen there are thirty-two remaining, which are fairly reckoned by Dr. Merriam as distinctively Sonoran, although nine of them range into the southern parts of the Boreal division. Of these thirty-two, only five, *Plecotus*, *Vesperugo*, *Vespertilio*,

[5] The genera adopted by Dr. Merriam are more favourable to his views than those given by Dr. Wallace. I take, however, the latter as the basis for comparison.

Nyctinomus, and *Felis* occur also in the Palæarctic Region. The remaining twenty-seven are peculiarly American, and it will be seen that this number is just over two-thirds of the thirty-nine given in the third column of Dr. Wallace's table as the number of Nearctic genera not to be found in the Old World. The peculiar family of the Pouched Rats (Geomyidæ) is specially noteworthy as coming distinctly into the Sonoran fauna, only one of its genera (*Thomomys*) just entering the Boreal Region. The other peculiar mammalian family, Antilocapridæ (Prong-horn), is also seen to be typically Sonoran. The differences between the two mammalian faunas in North America may now be shown in tabular form :—

	Distinctively American.	Common to America and Palæarctic Region.
Genera common to Boreal and Sonoran Regions ..	2[6]	6
,, typically Boreal ..	10	20
,, ,, Sonoran ..	27	5

And it becomes clear that the affinities of the northern fauna of America are with the fauna of Europe and temperate Asia, while the Sonoran fauna is exceedingly distinct.

When we turn to the birds we find, as might be expected, that the regional divisions are not so closely kept as by the mammals. Nevertheless it is possible, by a survey of the distribution of each genus, to determine to which fauna it belongs. A genus with a majority of Boreal species and but one Boreal may fairly be reckoned as Sonoran. In compiling the following summary I have taken the distribution as given in the A.O.U. Check List, whence Dr. Wallace compiled his genera :—

	Distinctively American.	Common to America and Palæarctic Region.
Genera common to Boreal and Sonoran	30	32
Typical Boreal genera ..	2[7]	14[8]
,, Sonoran ,, ..	81[9]	8[10]

It is not necessary to give lists of all these genera. I give in the footnote their reference numbers in Dr. Wallace's lists, which will enable anyone interested in the subject to check my results.

It will be seen that of the hundred and thirteen genera of birds which, according to Dr. Wallace, differentiate the Nearctic from the

[6] One of these (*Otaria*) is not *peculiarly* American.

[7] Genera nos. 34, 35 (3rd col.).

[8] ,, ,, 18, 20, 21, 22, 23, 25, 26, 27, 31, 36, 37, 47, 53, 54 (2nd col.).

[9] ,, ,, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 15, 16, 17, 18, 19, 23, 26, 28, 32, 33, 37, 41, 42, 46, 47, 48, 49, 50, 51, 52, 53, 54, 55, 56, 57, 59, 61, 62, 63, 64, 65, 66, 67, 68, 70, 71, 72, 74, 75, 77, 78, 79, 80, 81, 82, 83, 85, 88, 89, 90, 91, 92, 93, 94, 95, 96, 97, 98, 99, 100, 102, 103, 104, 105, 106, 107, 108, 109, 111, 113 (3rd col.).

[10] ,, ,, 6, 7, 8, 29, 33, 41, 49, 52 (2nd col.).

The sixty-two genera not enumerated here are common to the Boreal and Sonoran tracts.

Palæarctic Region, eighty-one, or over 70 per cent., are typically Sonoran, while only two are typically Boreal. That as many as thirty genera should range over the whole continent may be urged in support of the retention of the Nearctic Region intact. Yet many undoubtedly Neotropical genera range into the Sonoran; and when we consider the great extent of the North American continent and the absence of any natural barrier to check migration from south to north, our surprise should be that the mixing of the two faunas has not been more complete.

The second column of the summary given above yields highly suggestive facts. With the single exception of *Astur*, all the fourteen typical Boreal genera common to North America and the Palæarctic Region are confined to the latter region in the Old World. This shows most strongly the affinity between the Boreal American and Palæarctic bird faunas. On the other hand, we find that of the remaining forty genera in the second column, only three (*Acredula*, *Nyctala*, and *Archibuteo*) are, in the Old World, typically Palæarctic. The rest are either of very wide range, or occur in both the Palæarctic and Neotropical Regions.

Strong confirmation of the distinction between the Boreal and Sonoran faunas in America can be obtained from other groups. The paucity of Lizards in the former fauna has indeed been well shown by Dr. Wallace himself, as a survey of the tables of distribution of families in the second volume of his great work shows that but one genus of these reptiles enters the Canadian sub-region. This is *Gerrhonotus*, placed by Mr. Boulenger in the latest British Museum Catalogue in the Family Anguidæ. On looking through that Catalogue I find, on the other hand, that the Sonoran Region is characterised by no fewer than 22 genera of lizards:—*Eublepharis*, *Anolis*, **Dipsosaurus*, **Sauromalus*, **Crotaphytus*, **Petrosaurus*, **Callisaurus*, **Uma*, **Holbrookia*, *Uta*, *Sceloporus*, *Phrynosoma*, *Gerrhonotus*, *Ophisaurus*, **Anniella* (forming a peculiar family), *Heloderma*, **Xantusia*, **Cnemidophorus*, *Chirotes*, **Rhineura*, *Lygosoma*, and *Eumeces*, of which the 11 marked with an asterisk are not found elsewhere. The genera which are not peculiar have either Neotropical affinities or are represented in corresponding latitudes in the Old World. The Tortoises tell the same story. A survey of the land and fresh-water genera of Chelonians, as given in Mr. Boulenger's British Museum Catalogue, shows that only two genera, *Trionyx* and *Emys*, neither peculiar to America, reach the Boreal [11] district, while the Sonoran, in addition to these, possesses *Chelydra*, *Macroclemmys*, *Cinosternum*, *Chrysemmys*, **Malacoclemmys*, *Clemmys*, **Cistudo*, and *Testudo*, the two distinguished by the asterisk being peculiar. The Sonoran Region is further to be distinguished by the genus *Alligator*, among the Crocodiles.

[11] The former, indeed, seems not to pass Dr. Merriam's transition zone.

While admitting the great convenience of the Six Regions of Dr. Sclater, it seems that the above-mentioned facts yield strong support to the suggested revision of their boundaries. The addition of the Boreal tract of North America to the Palæarctic Region will render that great division of the earth's surface more natural than it is at present, as the elements of the Northern American fauna are seen to be chiefly Palæarctic in their affinities. To this great realm Dr. Heilprin's name of Holarctic might be applied, or it might perhaps be better known as the Great Northern Region. I need hardly remind students of Geographical Distribution that such a region was mapped thirty years ago in Murray's work[12] on Mammalian Distribution. The Sonoran Region will, indeed, be somewhat less rich than the old Nearctic in peculiar forms of life, but it will retain its most distinctive characteristics and comprise a more natural fauna than did the larger tract. The objection to the new arrangement, which I cannot but feel to be considerable, is that, even outside the broad transition tract of Dr. Merriam, overlapping of the two faunas takes place, especially in the extension of peculiarly American birds to the Arctic Ocean. Still, I cannot think that this overlapping should deter us from adopting the more natural division. No zoological region can be mapped with the hard and fast line of a political frontier, and the zoologist must always think more of faunas than of geographical boundaries. That two distinct faunas exist in North America seems abundantly clear, although the nature of the country has allowed considerable overlapping. It is well to remember that in parts of the Mediterranean sub-region there is a decided Ethiopian colouring to the fauna, in spite of the barrier of the Sahara. Were Africa without that barrier to the northern spread of Ethiopian forms, we should have such a mingling of two faunas as would necessitate a wide transition tract on our own side of the Atlantic.

GEO. H. CARPENTER.

[12] The "Geographical Distribution of Mammals." By A. Murray. London, 1866.

SOME NEW BOOKS.

The Art of Stuffing.

Scientific Taxidermy for Museums (based on a study of the United States Government collections). By R. W. Shufeldt, M.D. Smithsonian Institution. Report of the U.S. National Museum for 1892, pp. 369–436, pls. xv.–xcvi. Washington, 1894.

It was the Rev. J. G. Wood who once said that his earliest attempt at stuffing an animal was made upon a mouse. After skinning the creature, he cut off so many inches of his school ruler as would fill up the interior, and stitching together the margins of the skin under the belly, found a satisfaction in contemplating the success of his creation. The old days of monstrosities are rapidly fading away in all the larger institutions, and especially in our own British Museum of Natural History. No longer do we there see those highly polished cattle in whose sides, rumour has it, it was quite possible to get enough reflection to arrange one's hair; and we may even discount the statement of certain evilly-disposed persons who affirm that within several years there was to be seen one animal in the British Gallery with flanks had been repaired by a process of skin grafting of a some unsuccessful nature. The very beautiful examples of individual stuffed animals, or groups of animals, which are fast displacing their ... and worn representatives in our National Museum are too well-known to need recalling here, and Dr. Shufeldt refers to them as "Perhaps some of the finest groups in the world." Two of the best of these groups (of birds) Dr. Shufeldt has been able to reproduce through the courtesy of Dr. Bowdler Sharpe, and in doing so he pays a high compliment to "that distinguished ornithologist's" interesting paper on these stuffed groups which appeared in the *English Illustrated Magazine* for December, 1887.

In an exceedingly interesting sketch of the history of taxidermic art, prefixed to the paper, Dr. Shufeldt reminds us of Hanno's discovery of the gorilla five centuries before Christ. These were flayed, and the skins taken to Carthage, where they were preserved for many generations, and described by Pliny as Gorgones. He also refers to the robes of the Mexican kings, with their gorgeous ornamentations of trogon and other brilliantly plumaged birds; quotes Shakespeare's apothecary, within whose

> Needy shop a tortoise hung,
> An alligator stuffed, and other skins
> Of ill-shaped fishes,

and finally tells us that the earliest stuffed specimen, as opposed to a mere skin, now known to exist, is a rhinoceros still preserved in the Royal Museum of Vertebrates in Florence, and dating from the sixteenth century.

Dr. Shufeldt next discusses the methods employed by the best taxidermists of the century, and points out the advantages of a liberal education and the importance of a first-rate knowledge of general

biology and anatomy. The taxidermist should be, besides, a good field naturalist, a careful observer of nature and of the habits of animals, a botanist, a topographer, and an artist. The importance of photography is fully insisted upon, and Dr. Shufeldt considers a perfect knowledge of the art and practice of photography more useful perhaps than anything else. He says: "No taxidermist who has any regard for an attainment of excellence in his calling should neglect to make good photographs of all the living animals that he can, and that upon every possible opportunity. This should not be confined to wild animals alone, but to all the domestic ones in their most common attitudes."

Passing on to the true object of his paper, the criticism and explanation of stuffed or modelled specimens of natural history objects, Dr. Shufeldt refers to the beautiful gelatine casts, taken from plaster moulds, of the octopus, squid, and other soft-bodied invertebrates, shown at the Chicago Exposition, photographs of which he reproduces in support of his words. Noting the difficulties of the proper preservation of fishes, he mentions the remarkable collection of preserved fish in the Government Museum at Madras, which Hornaday referred to as the finest of its kind in existence. The lamentable aspect of fishes in alcohol and the further distortion produced by the jars in which they are usually preserved is noted. The advantage of making plaster casts, properly coloured, is insisted upon, and emphasised by photographs of some beautiful examples in the U.S. National Museum. The "Buckland" fishes will be remembered by most readers of this article, and the successful preparations made by Mr. Montagu Browne in more recent years. In our own Museum at South Kensington, we see an attempt to replace the ordinary museum fish with some more lifelike stuffed examples, but the use of plaster or the Browne methods might be more largely employed to advantage in both the Zoological and the Geological Galleries. Dr. Shufeldt says, speaking of rays, that the plaster and gelatine methods are so successful with these fish that, properly coloured, it would be difficult to distinguish between photographs of the living fish, the plaster, and the gelatine cast, if they were all taken on the same scale and under the same conditions. In all his statements, too, the reproduction of photographs of fish-models fully bear him out.

Some half-dozen photographs of beautiful plaster casts of snakes are reproduced, of which the rattlesnake, in a hole in the rocks, is wonderfully true to nature. Indeed, in these animals it is difficult to see why anything else but plaster casts of excellence should be shown in public galleries, so true is the cast, and so difficult is it to properly stuff the creature itself. The reproductions of photographs of stuffed birds have by far the largest space allotted to them, and they form an exceedingly interesting collection. Examples are given of correctly and incorrectly stuffed auks, gulls and penguins, of a scolding parrot, a parrot on the defensive, of game cocks and hens, a pigeon preening the breast feathers, a quail elevating feathers prior to shaking, and also in the act of walking, of owls, and various groups of birds, including, as before mentioned, some in our own Museum, of rare excellence.

In the Mammalia, examples are given of kangaroo properly and improperly stuffed, a group of armadilloes, a porpoise, gazelles, a group of *Bison americanus*, the head of a zebra of remarkable excellence, jack rabbit, head of a tiger, and many more which we are unable to refer to.

In writing this paper, which we specially commend to the notice of the Museums' Association which meets this month at Dublin, Dr. Shufeldt has earned the gratitude of every true student of nature, he has done the taxidermist an incalculable service in elevating his work from that of a mere stuffer to that of an artist, and there is no doubt he will see his closing words realised in "the further encouragement and stimulation of the progress of the art of taxidermy."

More Extinct Monsters.

CREATURES OF OTHER DAYS. By the Rev. H. N. Hutchinson. 8vo. Pp. xxiv., 270 (illustrated). London: Chapman & Hall, 1894. Price 14s.

IN a previous issue we had the pleasure of noticing Mr. Hutchinson's first literary and pictorial attempt at the restoration of extinct animals, and then expressed the hope that the work in question would soon reach a second edition. Not only has this hope been realised, but "Extinct Monsters," as the first volume was entitled, has been so successful from a publishers' point of view that it is now followed by another work of a similar nature. While carefully avoiding all mention of invertebrates in this new venture, the author takes into consideration several groups, such as the fishes of the Old Red Sandstone, the primeval salamanders, and the anomodont reptiles, which were altogether passed over in "Extinct Monsters"; while the treatment of many of the groups noticed in the latter is expanded and amplified. That Mr. Hutchinson has produced a book which forms a worthy companion to his first effort, we are fully assured, and we can only hope that it may obtain as much appreciation from the general public as seems (deservedly) to have fallen to the share of the latter. So far as the illustrations are concerned, we think that the book is almost beyond praise, and we notice that in his restorations of the dinosaurs the artist has considerably improved on his former efforts. Much interest will undoubtedly attach to the figures of the primeval salamanders or labyrinthodonts, as well as to that of the strange and gigantic pariasaur from South Africa. We have too long been accustomed in text-books to see labyrinthodonts restored in the form of impossible frog-like monsters; and it is refreshing to find them now in a guise somewhat near to what was probably their living form. The restoration of pariasaur strikes us as particularly successful, but may we ask the author why, on page 81, he transliterates the name of this creature as *Pareiasaurus*, while, on the preceding page, we find a name which has a similar diphthong in the original spelt *Tapinocephalus*? We may also suggest that he is not quite up-to-date in his treatment of the anomodonts and their allies, among other errors including *Proterosaurus* in that group, whereas it has been pretty conclusively shown to be a rhynchocephalian. Indeed, one of the chief faults we have to find is that the author, both in classification, nomenclature, and anatomy, follows one or two palæontologists to the exclusion of others.

On the whole, the book is written in a pleasant and agreeable style, although sometimes, as in the first paragraph on page 241, some of the sentences appear to have no relation to those which precede them, while in other cases the style is not free from grammatical faults. Perhaps the author will have the goodness in a second edition to inform us whether the sentence on page 107, stating that "like the gavials of India, they [Liassic and Oolitic Crocodiles] doubtless swarmed in the rivers and lakes, and preyed on the fishes that lived in the seas of those periods," is to be attributed to such a slip in grammar, or whether he possesses a

knowledge of the former distribution of Indian gavials which is denied to humble individuals like ourselves. Two pages later, we meet with the astounding statement that a "genus probably attained a length of ten or twelve feet." May we ask how you measure a genus, as this is another point in which we confess to a pitiful state of ignorance? Again, on page 212, we notice that the genus *Paloplotherium* is stated to be on the ancestral line of the horse, and to be very similar to *Hyracotherium*. We have, hitherto, been misguided enough to believe that the animal known as *Paloplotherium* was identical with the one mentioned on page 180 as *Palæotherium minus*, and that it had nothing on earth to do with the descent of the horse. We must, however, presume that the learned author is here also enabled to correct by his knowledge the ignorance in which we have hitherto been enveloped. The statement on page 112, that these and other creatures "are all nearly related to a previously known but larger animal, called by Cuvier *Lophiodon*," likewise reads to us somewhat curiously, seeing that the genus *Lophiodon* includes about a score of species, ranging in size from a sheep to a big rhinoceros.

Although the limited state of our knowledge, in comparison with that apparently possessed by the author, precludes our criticising such statements with the authority which is desirable, we are not hampered by any such disabilities when we point out manifest contradictions in the book. Turning, for instance, to page 130, we read that, "it has sometimes been asserted that birds were derived from dinosaurs in the course of evolution, but this is not a correct statement of the views of leading naturalists such as Professor Huxley"; but a little later (pp. 151, 152) it is written that "we have already alluded to Professor Huxley's theory that birds are descended from dinosaurs." This is unkind, as it would have been a source of great gratification to us to have learnt what the professor's views on this interesting subject really are. Be they what they may, the author is, however, by no means prepared to give them his unqualified assent, and accordingly draws upon his own stores of knowledge of the anatomy of the two groups in question in order to disprove them.

Before leaving the author's style, may we suggest to him that it would be much less irritating to ourselves, and probably to other readers, if, when mentioning the names of the authorities referred to, he would refrain from appending certain mystic letters to their names? Such additions only spoil the look of the text, and, although it is just conceivable that it may gratify a small percentage of the personages thus honoured to see such additions to their names, to the ordinary reader it is a matter of the most supreme indifference whether they are entitled to be called F.G.S., F.R.S., L.C.C., or P.C.L.P. (parish councillor for Little Pedlington!); and on this subject we commend Lowell's "Note to Title-page" of the "Biglow Papers" to the author's notice. We cannot, moreover, pass over the statement on page 18, to the effect that in England "so little is done for the cause of science by our rulers." In the first place, in this democratic age it is not "our rulers" but ourselves who are responsible for the amount spent on science; and, secondly, in our own opinion, considering the very small percentage of the population that takes an interest in Zoological Science, quite sufficient money is expended by Government thereon. Personally, as a taxpayer, the writer would be most unwilling to contribute a single farthing more towards such a subsidy.

Reverting for a moment to the Labyrinthodonts, we confess that



THE HIPPARION (UPPER PICTURE), AND A RECENT WILD HORSE (LOWER PICTURE). (From *Creatures of Other Days.*)



RESTORATION OF THE DINOTHERIUM. MIOCENE PERIOD. (From *Creatures of Other Days.*)

we are somewhat puzzled by the statement on page 13 that in the figure of the tracks on the opposite page only one-half of the track of the animal is delineated. Surely, as the impressions are both right and left they must include those of the feet of both sides; if not, will the author kindly explain his views more fully?

The restorations of the mammals are for the most part well done, and they are probably as near like the originals as we are ever likely to approximate, although it is probable that even the artist himself would be willing to admit that the indications of colouring, which have in some cases been attempted, are more or less fanciful. It is, moreover, a little misleading to find pig-like creatures included in the chapter entitled "Tapir-like Animals, etc." That the book will attract a large number of readers, we have little doubt, and we cannot do more than wish it a success similar to that which has attended its predecessor.

THE CULT OF THE CAMEL.

THE CAMEL: ITS USES AND MANAGEMENT. By Major A. G. Leonard. 8vo. Pp. ix. and 335. London: Longmans & Green, 1894. Price 21s.

IT is a well-known fact that during the last Cabul war and the Nile Expedition the loss of camels through overwork, improper or scanty feeding, and bad treatment, was almost appalling; and although a considerable proportion of this loss was inevitable, there is little doubt that the number of casualties might have been enormously reduced by the bestowal of more attention on the needs and habits of the animals in question. As wars in the same regions or other districts where camels can be employed are inevitable in the future, we heartily welcome Major Leonard's attempt to give Tommy Atkins and his superiors some insight into the habits and needs of these invaluable although unsociable creatures. As the author was both present during the Afghan war and accompanied the Camel Corps during the march to Korti, where he appears to have been connected with the commissariat department, his opportunities of observing the camel at work have been unrivalled; and that good use has been made of such opportunities, the present work affords ample proof. In the book before us we have chapters on the characteristics and temperament, and the instinct and intelligence, of camels, as well as others on their breeding, watering, feeding, loading, management, and ailments, and further one on their capacity and endurance. For the most part, these are well written, and ought to show any intelligent officer how to make the best use of these animals when put under his charge. We notice that, although the stupidity of the camel is admitted, Major Leonard does not give the camel such a bad character in general as is attributed to it by Palgrave and others; and he is doubless right in his assertion that much of the moroseness of its disposition is due to bad treatment and mismanagement. Attention may also be directed to the author's observations on the importance of watering camels at stated intervals whenever practicable, since there is no doubt that during the wars alluded to above a large proportion of the deaths were due to the mistaken belief that these animals are capable of going for an almost unlimited period without water.

If the author had confined his attention to the camel as a beast of burden, we should have had nothing but commendation for his book; but he has unfortunately thought it necessary to say something of the geological and zoological history of the creature, and in the course of his observations renders the statement in the introduction that he is

not a naturalist altogether superfluous. It strikes us, for instance, as somewhat curious to find (p. 1) the camel referred to as one of the oldest living mammals, while a few lines further on it is stated that its prehistoric ancestors are unknown. The context shows that this statement is due to Andrew Murray's belief in the close alliance between the Siwalik and modern camels; but if the author had consulted authorities on the subject he would have found that they are very different, and his confusion between a species and a genus is too obvious to need pointing out. When, moreover, the author (p. 2) states that "it is a curious fact that the former description [? distribution] of the family seems to be much wider than the natural habitat of the forms now extant—a fact which may, perhaps, be attributed to the Deluge, and the sundering of what in those days were vast globes into smaller and insulated continents," he is not only a bit behind the times, but likewise most remarkably enigmatical. Still more startling is the assertion on the same page—made on the authority of a South African friend who has *compared the figures* —that the *Helladotherium* of Pikermi, instead of being, as we have all supposed, a near cousin of the giraffe, is nothing more than a camel! What will our friend Dr. Forsyth Major think of this as the result of all his labours to instruct the public? Other instances of misconception and error might be referred to, but we can only call attention to the assertion, on p. 22, that a camel has twelve molars on each side of the upper jaw, and ten in the lower, as a sample of the anatomical blunders disfiguring what is otherwise an excellent work.

Blind Crustacea.

T. Subterranean Crustacea of New Zealand: with some general remarks on the Fauna of Caves and Wells. By Charles Chilton, M.A., D.Sc. *Trans. Linnean Soc. London*, ser. 2, Zoology, vol. vi., pt. 2, May, 1894.

This paper treats of the curious blind crustacea which inhabit the underground waters of the Canterbury Plains in the South Island of New Zealand. The author leads off with an historical account of previous writings on the subject, and gives a fuller bibliography at the end of his paper. Detailed descriptions of the six New Zealand forms follow, and the author then passes to the more general considerations which we present as interesting to the majority of readers. One of the characteristics of these underground crustacea is the absence of colouration. The integument and greater part of the body is colourless, and though traces of colour (pink or yellow) are occasionally observed, this is the result of the yolk of the egg in females, the liver tubes, or the red colour of the fluid in the vascular system (in worms). In comparing these subterranean forms with deep-water organisms which often show a more or less definite colouration, the author is inclined to believe that the difference in colour is due to some differences of chemical composition rather than to the presence or absence of light. Professor S. I. Smith has previously stated that the darkness existing beneath 2,000 fathoms of water is very different from that existing in ordinary underground caverns.

With regard to eyesight, Mr. Chilton observes that in the New Zealand specimens he has been unable to find any external trace of eyes except in one species, *Crangonyx compactus*, in which the eye is represented by two or three imperfect lenses apparently quite without pigment. He has as yet had no opportunity of making sections to study the conditions of the optic lobes and nerves. On the whole, too, the New Zealand forms give only a modified support to the conclusion

that the subterranean species are more abundantly supplied with sense-organs (other than eyes) than allied surface animals. He is unable to add any fresh information as to the food and food-supply of these crustacea. With regard to arrested development previously noticed among these animals, Mr. Chilton says he has a good example in *Cruregens fontanus*, which has the seventh segment of the peræon small and without appendages, as is the case in the young forms of many Isopods. Of the habits of *Cruregens*, he notes that "they ran backward and forward with equal rapidity, and did not seem particular which way they went; they did not swim, and if they dropped off a plant wriggled helplessly till they reached the bottom." They ran against objects in a way which seemed to indicate that they were totally blind, and he occasionally saw two approach very near each other, apparently without being aware of it, and then suddenly jump apart when they touched.

Speaking of the age of the cave fauna of New Zealand in particular, the author notes that all the places where subterranean forms are found are marked on Professor Haast's map as either "post-Pliocene alluvium" or "recent alluvium" and mostly in the latter, and he suggests that it is quite possible, if thorough search were to be made, that some species of the genera (especially of *Phreatoicus*) would still be found inhabiting fresh-water streams among the Southern Alps of the country. The paper is well illustrated by some quarto plates, and is a valuable contribution to a most interesting subject.

A Criticism and a Theory.

GESTALTUNG UND VERERBUNG. Eine Entwickelungsmechanik der Organismen. By Dr. Wilhelm Haacke. Pp. 337, twenty-six illustrations in the text. Leipzig: T. O. Weigel Nachfolger, 1893. Price 8 marks.

ALTHOUGH Dr. Haacke does not proceed to the display of his own theory until he has spent over one hundred pages in criticising the theories of others, and specially the theory of Weismann, it may be more convenient to attempt at once to give an account of this new panacea for all biological difficulties. His theory is *epigenetic* in the strictest sense. He believes that the plasma of the whole body of each animal and the plasma of the germ-cells are of the same kind. Hence he thinks that the shape and symmetry of the adult must be explained by the shape and symmetry of the elements in the plasma of the germ-cell from which the adult grew. The plasma is made up of ultimate elements, which Dr. Haacke terms "*gemmæ*." These are rhombic prisms, and the process of assimilation consists of the formation of new *gemmæ*. The unformed food of the plasma is built up into the peculiar *gemmæ* of the particular animal, or plant, in some such fashion as inorganic particles grow into crystals under the stimulus of the presence of crystals of their own order. *Gemmæ*, by the adhesion of their faces, are built up into units of a higher order, called *gemmaria*. As out of one set of similar bricks many different castles, depending on the number and arrangement of the bricks in each castle, might be formed, so out of a comparatively small variety of *gemmæ* an indefinite number of *gemmaria* might be obtained. It is these *gemmaria* that are the real morphological units. Out of one *gemmarium* you could tell the whole animal. But as from their size, if from no other reason, the investigator is exceedingly unlikely ever to see the *gemmarium*, Dr. Haacke goes to work the other way about, and endeavours to deduce, from the shape of adult organisms, the shape of their *gemmaria*. This he does in a sufficiently

ingenious manner, and at least succeeds in convincing the reader that the shapes of animals are not altogether unprofitable subjects of study.

The architecture of the *gemmaria*, as one learns from Dr. Haacke's descriptions and figures, is of a simple kind. They form no airy structures with arches and beams and colonnades. The *gemmæ* are superimposed in solid masses as our primeval forefathers might have built monuments to their fathers, or altars to their gods. As there are different numbers of *gemmæ* in the successive tiers, they resemble still more closely the solid staircases which we, as children, formed of our wooden bricks.

These *gemmæ* may adhere together closely or loosely. In each generation individuals with *gemmaria* of which the *gemmæ* are closely adherent are very naturally more stable than those whose *gemmaria*, so to speak, dissolve with every puff of wind. This is Dr. Haacke's interpretation of differences in the strength of individual constitutions. He points out that, in the struggle between individuals, it is not the individual with a claw slightly longer, or with a tooth slightly sharper, that, on the average, survives longer to propagate his species more largely. It is the individual generally better endowed, generally more adapted to resist every blow of fate, to take advantage of good fortune of every kind, that is favoured in Nature's battle. Interpreted into his theory, this means that in each generation those individuals with little adhesion of the *gemmæ* in the *gemmaria* speedily are discomfited; that, in fact, there is a gradual phylogenetic strengthening of the stability of the *gemmaria*.

From this another result ensues. Strengthening of the *gemmaria* implies a resistance to change of any kind. Just as the stable *gemmaria* resist external influences that might be to their hurt, so they resist influences that might be to their advantage. Thus, with age each race becomes hardened. It pursues its own line of progress resisting all other influences, and so a race may die out, as many races seem to have died out, because it persists in a definite line of development after that line has ceased to be of advantage to it, and has even become detrimental.

The plasma, that is to say the *gemmaria*, all over the body of an animal is not only similar in shape and structure, but is in a condition of organic stability. Hence it follows that any external influence acting on a part of the organism, so as to alter the shape of the *gemmaria*, will disturb the equilibrium of the whole body. The disturbed system gradually settles into a new position of stability, a position in which the new force has been registered. Thus it happens that every change of any part of the organism is reflected upon the plasma of the germ-cells of the organism. Thus, acquired characters are inherited, subject, of course, to the limitation that the impacted force must be sufficiently great, or last through a space of time sufficiently long to counteract the stability of the *gemmaria* acquired in ages of elimination of the less stable. Thus, for instance, Dr. Haacke scoffs at the attempt of anyone in a few generations of mutilations, by docking the tails of even 90 generations of mice, to prove or disprove the question of the inheritance of acquired characters. When nature has been building up *gemmaria* that give rise to tails, and hardening these *gemmaria* for a million generations, it is ridiculous for man with his scalpel and his few score years to hope to interfere.

It may be possible that, to some readers, a theory so purely theoretical as this theory of Dr. Haacke's may not be attractive.

Even such we advise to peruse Dr. Haacke's book. For running through this warp of ingenious, but possibly perverse speculation is a woof of actual observation, of shrewd criticism and valuable suggestion. So far as we are concerned we do not think that this theory has the slightest merit even as a provisional hypothesis. We think it the purest moonshine, and regret that a man of Dr. Haacke's evident ability should have published it, even as a jest. This opinion notwithstanding, we think that there are very few pages in the book where a biologist will fail to find a novel handling of novel facts and a most suggestive and stimulating treatment of the most interesting questions of biology. There are among us many given to the translating of strange books. If the idea of translating this book occurs to anyone, we offer the following advice: Let him carefully eliminate every reference to the particular theory of the *gemmaria*; let him curtail a few of the pages of algebraic formulæ, and the volume, so trimmed and lightened, will prove as useful, attractive, and valuable as any book dealing with biological theory published since Darwin died.

Flora of France.

Flore de France, contenant la description de toutes les espèces indigènes disposées en tableaux analytiques et illustrée de 2,165 figures représentant les types caractéristiques des genres et des sous-genres. By A. Acloque. 8vo. 1 vol. of 816 pages. Paris: J. B. Ballière et Fils, 1894. Price f.12·50.

In a laudatory preface, which takes the form of a letter to the publishers, M. Ed. Bureau, professor of botany at the Natural History Museum in Paris, expresses his surprise that anyone should have been found sufficiently bold to undertake a work so considerable as that which M. Acloque has just accomplished. Youth only, he says, could have been capable of so daring an act, but *audaces fortuna juvat*, and fortune has not failed the author in the present case; for M. Bureau finds that the book answers the only sure test which can be applied to one of its kind, namely, to enable the student to run down to their specific names several plants taken at hazard. The object of the work is to assist the field botanist to determine his plants by the aid of dichotomous keys after the manner, for instance, of Hayward's botanists' pocket book, though M. Acloque's bulky volume would make about four of these as regards size. Besides the key the distinctive characters of the families are also given, arranged in sytematic order. In the analytic table of the families the student has the assistance of numerous figures which the author reminds us must be considered as diagrammatic. They will doubtless be useful, though some of them are so extremely rough that they were better left out altogether. For instance, the first four, which are to illustrate vascular and three forms of cellular tissue, will be more of a hindrance than a help to one who is ignorant, while he who knows will scoff at them. The diagrams, however, generally answer well enough to indicate the form of organs or their appearance in section. In the elucidation of genera and species numerous figures are given to illustrate the habit of the plant or inflorescence, and sometimes the form of the leaf, flower, or fruit. Some of these are better than others, but as a whole they compare unfavourably with the plates in Bentham's illustrated handbook. The mere fact that there is in France no similar work will ensure the book a welcome; the text, however, is well arranged and will, except in failing light, when it will be found trying to the eyes, prove a great boon to the student of field botany.

The Natural History of Plants.

The Natural History of Plants. By Kerner von Marilaun. Translated by Professor F. W. Oliver, assisted by Misses Busk and Ewart. (Blackie.)

It is but few years since the General Biology, or Natural History, of Plants became a theme for serious investigation. Nowadays the relations between plants and their surroundings form the subject of a critical scrutiny. The older school of workers in this branch of botany were characterised by their imaginative power and credulity with regard to new theories, rather than by the exercise of any searching enquiries into the truth of their hasty hypotheses. Professor Kerner belongs distinctly to the old and romantic school. It were an exaggeration to say that he is more of a poet than of a botanist. For his book is no mere collection of light fancies; indeed, it includes a rich store of interesting facts which, for the most part, can be gleaned elsewhere only in the scattered papers of specialists. Yet the fact remains that this work, though delightfully fresh and supremely suggestive, is utterly unreliable. With an untamed dogmatism born of a hyperæsthetic imagination, Professor Kerner treats aëry conceits as proven truths. Hence many botanists may doubt the wisdom of translating such a work. Be this as it may, it is easy to appreciate the infinite difficulty involved in the labour of adequately editing an English version. Professor Oliver and his associates have evidently decided to let the work stand as it is in the original. So the theory of the centripetal and centrifugal conduction of water down plants, and the semi-fabulous account of *Lathraea squamaria*, are repeated in the translation. Even if warning footnotes be not put in connection with cases of this sort, surely erroneous, misleading, or heterodox statements should not be left unchallenged. Yet on page 64 we find it indicated that the free nitrogen of the air is not available to the plant save through the assistance of electrical discharges. On page 42, of chlorophyll bodies we are told that "they are produced generally in great numbers in special sac-like excavations in its body, but nowhere except where they are necessary . . ." It would have involved little labour to have given footnotes representing the generally accepted modern views on the subject; and the book could have thus been rendered a less dangerous guide.

But it may seem ungrateful to grumble at the translators' decision to leave the work unaltered, when that which they have attempted—the translation—is such a triumphant success. Professor Kerner's charming German idiom is reflected in clear and graphic English. In its form and get up the English version surpasses the original; in fact it is hard to see how the work could in any way have been improved as a translation. Doubtless this eager and persuasive book will do much towards popularising the study of botany in England.

Percy Groom.

Magazine Articles.

Index to the Periodical Literature of the World (covering the year 1893). [Compiled under the superintendence of Miss E. Hetherington.] 4to. Pp. 224. London: *Review of Reviews* Office, 1894. Price 5s.

Mr. Stead's generous effort to give readers a cheap and ready book of reference to the countless articles appearing in ephemeral literature is a marked improvement on its predecessors. Many suggestions as to additions and revisions have been made, and the majority of them have been accepted. The index proper has grown to 150 pages, 40

pages more than in the volume for 1892, and numerous publications, both British and foreign, have been added to the lists over and above those given and indexed last year. The price of the book is absurdly low in comparison to its value, and there can be no doubt that at present at all events its publication is undertaken at a considerable loss. As before, Miss E. Hetherington and her able staff of women assistants are responsible for the compilation of the index, and the result is worthy of the highest praise.

The first 58 pages are devoted to a list of magazines and other periodicals arranged under countries in four columns, giving the title of the book, the editor, publisher, and explanatory remarks. The remaining 150 pages give a minute and detailed index to the subjects contained in the publications listed. To turn to one entry in this index—Birds—we find no less than 55 articles dealing with the subject were published in magazines during 1893. It may be quite true to say that these are of no particular value and might well be left to perish unrecorded; but, on the other hand, we have the inestimable boon of being able to find out if such and such an article has been written without the wearisome task of searching high and low for it. Turning to another subject quite outside our own, we find under Musicians a record of no less than 100 names of which are given particulars of more or less interest.

We cannot of course enforce the purchase of this book, much as we should like to do so, but we hope few readers of NATURAL SCIENCE will refuse to place upon their shelves a volume so valuable to those who wish to keep themselves in touch with general subjects. W[illegible]ural an increased list of subscribers, moreover, comes a corresp[illegible]ould increase in usefulness, for a return of the expenditure will al[illegible]erable the inclusion of periodicals at present omitted solely for reas[illegible]ly, he expense. [illegible]rtuna [illegible], for [illegible] be [illegible]run

THE COUNTRY MONTH BY MONTH.

SOMEWHAT late in the day a copy has been sent us of F. [illegible]he Knight's "By Moorland and Sea" (Elliot Stock, 5s.); a re-issue o[illegible] is articles that have appeared in various publications. Mr. Knight is, pre-eminently an out-door naturalist, and his bright and brief little chapters, some of them perfect prose poems, are redolent of the fresh air, and are a delightful change after wading through some particularly dry piece of laboratory investigation. Gossipy writings of this class do not, as a rule, come within our province at all; but one chapter, "The Birds'-Nester," is a genuine piece of field natural history, and reveals the true naturalist. It is evident in every line that the writer is a careful observer in close touch with Nature, who feels to the full the truth (as he expresses it), "Happy the man in whose life some room is found to watch the happier children of the air." Some of the process illustrations, too, are strikingly good.

ANOTHER work of the same class is now being issued in monthly parts by Bliss, Sands & Foster, under the title of "The Country Month by Month." It is written not for regular collectors or professed naturalists, but for the crowd of intelligent "general readers" who are now tempted into the country by cheap excursion trains or the cycle. We have received the parts for April and May. Each consists of a short essay on the month, followed by chapters on the plants and animals that may be seen in it. The essay is composed in the main of a series of quotations from poets, good, bad, and

indifferent; the chapters on the plants give a pleasantly worded account of the most interesting flowers that are likely to attract attention. A note on "Wild Life: Furred and Feathered" introduces us to the birds and beasts. An appendix gives a list of the species referred to, with their scientific names. The natural history is written in the sketchy style of Richard Jefferies and the "Son of the Marshes," the latter of whom is the senior editor. Prof. Boulger is responsible for the botanical section of the work. The parts are issued in paper or cloth at one or two shillings respectively. We recommend them heartily to those who are fond of country rambles and wish for help in knowing where, when, and how to look for our most interesting plants and animals. We fear, however, the unsophisticated townsman is not likely to see everything mentioned unless he is ready to rise a good deal earlier than most men do taking holiday in the country.

New Serials.

We have to welcome a new scientific publication from Portugal, the *Annaes de Sciencias Naturaes*, published by Augusto Nobre (8vo. Porto, no. 1, Jan., no. 2, April, 1894). Number one contains, among other matter, a list of plants of the Porto district, by Edwin Johnson; observations on the nervous system of *Arion lusitanicus*, by Augusto Nobre; the birds of Portugal, by W. C. Tait; notes on a large specimen of *Orthagoriscus mola* (the moon-fish), by A. Girard; and numerous notes on matters of local general interest, of which may be mentioned Pisciculture in Portugal, *Narcissus cyclamineus*, and the zoological station of Cascaes. The number is illustrated by three excellent photographic plates from the living plants, showing *Oxalis* [illegible]*rea*, *Senecio scandens*, and *Narcissus cyclamineus*; and by a plate of the nervous system of the *Arion*, described by Senhor Nobre.

Number two contains papers by Dr. Lopes Vieira on the freshwater fishes of Portugal, and upon the habits of *Petromyzon*; continuations of Johnson's, Nobre's, and Tait's papers referred to above, and a note on the molluscan fauna of the islands of St. Thomas and Madeira, with a plate. Illustrations of *Petromyzon fluviatilis* and *P. marinus* are also given.

We hope that this journal, so creditably got up, will meet with the success it deserves. It is to be issued three times a year, the annual subscription being 1 m. 800 (= 8s.). Though Portuguese is the language of the editors, we are glad to observe that other languages are not excluded, as witness several contributions in French.

Another new serial reaches us, from Trinidad, no. 1 of the *Proceedings of the Victoria Institute of Trinidad*, an institute which was founded as a memorial of the completion of fifty years' reign of the Queen. In the articles of association we do not see any evidence of the religious prejudice which characterises an institution bearing a similar name in this country. Three papers are printed, viz., Guppy on "Edible Molluska of Trinidad"; Hart on "Ventilation"; Nabe on "Butterflies." They are more educational than scientific. The *Proceedings* are published at "The Mirror" Office, Port-of-Spain.

OBITUARY.

GEORGE JOHN ROMANES.

BORN MAY 26, 1848. DIED JUNE, 1894.

PROFESSOR ROMANES was of Scotch extraction, and was born in Canada in 1848. He was not a public schoolman, but was educated at a succession of private schools, and by tutors in London and abroad. He had a distinguished career at Cambridge, and being possessed of private means, was able to devote himself to research after his University career. This he did with so great success that in 1873 he was Burney Prize Essayist, in 1875 Croonian Lecturer to the Royal Society, and in 1879 he attained to the fellowship of the Royal Society. He was well known as a lecturer at the Royal Institution, at Oxford, and at Edinburgh, but most people came to know his name by his abundant contributions to periodical literature. His many personal friends speak in the warmest manner of his personal qualities, and everyone interested in biology regrets his premature death. We do not profess to write with any great private knowledge of him; but we would call attention to the handsome tribute paid to his memory in the columns of *Nature* by Professor Lankester, who was at once his friend and his controversial opponent.

Professor Romanes was a fertile and ingenious exponent of the Darwinian theory, a critical and profound reasoner on biological philosophy, and a brilliant investigator. His exposition of the orthodox Darwinian theory, published under the title of "The Scientific Evidences of Organic Evolution" in Macmillan's Nature Series, is by far the most lucid, orderly, and simple publication on the subject. The first volume of his "Darwin and after Darwin" (Longmans, Green & Co., 1892) is a more detailed but equally intelligible account of the same subject. In the multitude of books written about or round about Darwinism these two writings of Professor Romanes stand out by their correctness in detail, by their literary merit, and luminous simplicity.

His most notable original investigations were upon the nervous systems of Star-fish and Medusæ. These, for the most part, were contributed to the Royal Society's *Transactions*, but afterwards a popular *resumé* of them was published as the volume on Jelly-fish, Star-fish, and Sea-urchins in the International Scientific Series. The minutiæ of structure and histology did not attract his attention so much as

the study of function; and he investigated the anatomy of these organisms much less than their physiology. Although his work on them was not sufficiently minute or accurate to rank him among morphological specialists, it led him to the equally important study of mental phenomena in animals. Darwin left to him many unpublished notes on Animal Psychology, and upon these, as well as upon his own investigations, were founded the volume upon "Mental Evolution in Animals," and a later volume on the "Origin of Human Faculty." These books have been the occasion of much controversy, but unquestionably they are greater contributions to Comparative Psychology, and hence to the study of the mind of man, than most psychologists were ready to admit. We prophesy that, as now Psychology has abandoned its concubinage with Metaphysic for a fertile union with Physiology, Professor Romanes's work will receive a place more important than that presently assigned to it.



GEORGE JOHN ROMANES.

His contributions to Biological Theory are perhaps the least important part of his works. His theory of physiological segregation was ingenious, but it was based on far too little observation to bear the superstructure he raised on it. In his controversies in various periodicals, he was more apt at arguing over verbal points, and laying stress on "logical inconsistencies," than at appreciating the facts which gave origin to the theories in question. We have had occasion in NATURAL SCIENCE to point out in several instances what we thought defects in his arguments. But it is just and pleasant to add that he was very ready to modify his own views or his conception of the views of others when the stress of facts lay against him, and that this logical acumen served often to expose concealed weaknesses in the arguments of others.

In later years Professor Romanes was conducting a variety of

experiments on an extensive scale, that, had they not been stopped by his untimely death, would have been of great importance in settling many obscure questions of inheritance. Certainly English science is the poorer for the death of this worker, who gave so much time, talent, and ability to the investigation of its more abstruse problems.

We are indebted to the courtesy of the Editor of the *Illustrated London News* for the portrait here given.

DR. AUGUST VON KLIPSTEIN, whose death, on the 16th of April in this year, we recorded in our last number, was the son of the mineralogist, Philipp Engel von Klipstein, of Darmstadt, and was born on June 7, 1801, at Hohen-Solms, near Giessen. In 1836 he was appointed Professor of Mineralogy at Giessen. He is perhaps best known by his monograph on the geology and palæontology of the Eastern Alps, but has also made valuable communications on the geology of the Kupferschiefergebirge, the Odenwald, the Dukedom of Hesse, Upper Suabia, and the Vogelsgebirge. In 1837 he described, with J. J. Kaup, the *Dinotherium giganteum* from Darmstadt.

DR. JOZSEF SZABÓ DE ST. MIKLÓS, who, as already announced, died at Budapest on April 10, at the age of 73, was well-kno[illegible] for his studies in mineralogy, of which science he held the cha[illegible]tory the University of Budapest. He was also President o[illegible]bited Hungarian Geological Society, and secretary to the Natural Sc[illegible] very section of the Hungarian Academy. Dr. Szabó was an enthusi[illegible] Mr collector, and the University Museum owes much to his energy a[illegible] is exertions. [illegible] in [illegible]er [illegible].

DR. K. A. FIEDLER, Privatdocent at the University of Zürich died on April 2, at the early age of 31. Dr. Fiedler had published, among other things, some interesting experiments on the development of Echinoderm eggs similar to the experiments of Driesch, Herbst, and others.

WE have also to record the deaths of the algologist, ALPHONSE DERBÈS, Honorary Professor at the Faculty of Science at Marseilles; of DR. THOMAS MORONG, Curator of Columbia College, who died on Thursday, April 26. Dr. Morong was especially known for his work on Potamogetons and other allied water plants; he also collected somewhat extensively in Paraguay. And of KNUT FREDRIK THEDENIUS, at Stockholm, on March 4, aged 80. An apothecary and a teacher of Natural History in the Stockholm Gymnasium, Thedenius was the author of several works on Swedish local floras, and also, in conjunction with N. J. Andersson, of a "Svensk Skol-Botanik," containing 250 coloured plates and descriptions of Swedish plants.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

IMPORTANT changes have recently been made in high scientific posts of the United States Government. Major Powell, who for thirteen years has been head of the Geological Survey, has resigned on account of ill-health, due to enlargement of the nerves in his wounded arm. Major Powell is an ardent explorer and ethnologist, and it has been largely through his efforts that there is now so complete an understanding of the Californian Gold Belt and the Rocky Mountain region. Major Powell will not, however, retire into private life, but will become superintendent of the Ethnological Bureau, which was recently severed from the Geological Survey. His successor, as head of the Survey, is Mr. Charles Doolittle Walcott, whose nomination will be heartily welcomed in this country by all who appreciate his thorough and painstaking investigations in the palæontology and stratigraphy of the older rocks of America. There has of late years been some exception taken in certain quarters to the lines on which the work of the Survey has proceeded, but Spe[illegible] understand that the present change has nothing whatever to do with these this li[illegible]s, and that the labours initiated by Major Powell, especially the great topo-[illegible]cal map of the United States, will be continued without alteration.

Professor C. V. Riley, for twenty years chief entomologist of the Department [illegible]send griculture, has resigned his post in order to devote himself to scientific work lec[illegible]tettered by the restrictions that necessarily surround a Government servant. His [illegible]It [illegible]irement will be regretted by both practical and scientific men, whom the work [illegible]bune in his bureau has so greatly benefited. He is succeeded, at his own recom-[illegible]Snendation, by Mr. Leland O. Howard, who has been first assistant in the bureau for fourteen years, co-editor of the *Bulletin* with Professor Riley, and is now President of the International Association of Economic Entomologists.

Professor Milton Whitney has been placed in charge of a newly created Division of Agricultural Soils, in the United States Department of Agriculture, which Division is intended to study the relation of soils to crops and the physics of soils.

Lastly, Professor W. H. Holmes has severed his connection with the Bureau of Ethnology to accept the position of head of the department of anthropology in the Columbian Museum at Chicago.

AMONG recent European appointments we note those of Dr. S. J. Hickson to the Professorship of Zoology in the Owens College, Manchester; Professor E. Kalkowsky of Jena to the chair of Geology in the Dresden Technical School, and not Dr. Credner as previously announced; Dr. L. Will to the Professorship of Zoology in the University of Rostock; and Forstassessor G. Sarauw to be assistant in the National Museum, Copenhagen. Professor R. Semon has resigned his position as assistant at the Anatomical Institute of Jena, and Doctors Braus and Drüner have been appointed in his place. We take this opportunity of congratulating Dr. Pfitzer, the Professor of Botany at Heidelberg, on his appointment to the dignity of Geheim-Hofrath, and the University of Oxford on the fact that Francis Galton is now one of its Doctors.

THE following awards have recently been made:—The silver medal of the Zoological Society of London to Mr. H. H. Johnston, C.B., in acknowledgment of

the efforts he has made to increase our knowledge of the zoology of British Central Africa; the gold medal of the Linnean Society of London to Professor E. Haeckel, who was unable to be present to receive the honour on account of recent illness; the Demazières prize of 1,000 francs, in the award of the Paris Academy of Sciences, to the botanist, M. Sauvageau, while the Montagne prize, of the same value, was divided between two other botanists, MM. Cardot and Gaillard.

AT the Ladies' night of the Royal Society, on June 13, Professor McKenny Hughes had an exceedingly interesting exhibit of ancient horns, illustrating the evolution of the breeds of English oxen. In the Palæolithic age there were three species—*Bison priscus*, *Bos primigenius*, and *B. longifrons*. Only the latter survived till the arrival of the Romans, by whom it was crossed with a larger imported breed, having straighter and more upturned horns—a type which is still seen in the tawny Highland cattle and in the Chillingham cattle. All these are whole-coloured; the parti-coloured cattle are a much later introduction. After the withdrawal of the legionaries, stock soon ceased to be selected, and the cattle reverted to the type of *Bos longifrons*.

The Marine Biological Association had added to their exhibit a sole from the North Sea, in which the eye had not shifted from the left to the right side of the body, so that the head remained undistorted. A similar case was quoted last year in a French scientific journal, but the occurrence is a rare one.

Mr. Hutchinson, whose last book we notice in this number, had some small papier maché models of *Megatherium*, *Dinoceras*, and *Plesiosaurus*, which would have pleased us better had some texture been imparted to their surface. Calvity is the prerogative of the most highly-developed among us.

Professor Stewart, with the aid of three specimens of the common shore-crab, taught us how to walk sideways, and showed what modification of locomotose appendages takes place in the hermit crab.

Miss Edna Walter, B.Sc., is, if we mistake not, the first lady that has exhibited at the Royal, and we must congratulate her not only on her exhibit, but on the lucid explanation she gave of it. It was a goniometer, devised by herself and H. B. Bourne, for the purpose of projecting a crystal on a sphere, and its value rather to demonstrate the fundamental axioms of crystallography than to assist the practical measurement of crystals. Miss Walter has, we understand, further added to the triumphs of her sex by exhibiting this same apparatus at the last meeting of the Mineralogical Society and lecturing upon it *in propria persona*.

THE conversazione given by the Oxford University Junior Scientific Club on May 22 was, in spite of the weather, one of the most successful that we remember, and was attended by over 800 guests. The exhibit that attracted most attention was a block of solid carbon dioxide, weighing 40 lb. This had been sent from the brewery of Messrs. Guinness, in Dublin, packed in a barrel with wool, and had only lost 10 lb. on the journey. The temperature of the solid substance is -76° C., and by its means mercury was solidified and shown to be malleable, while ether was rolled into candles and burnt. Among exhibits of more direct interest to ourselves were a collection showing the adaptation of plants to environment, particularly that of deserts, and experiments showing the distribution of seeds, movement of tendrils, and formation of starch; living marine animals, amœbæ, rotifers, and the like; interesting histological preparations from the human subject; while, despite the Anti-vivisection Society, several physiological experiments were performed on not unwilling visitors, many even shedding their blood in the cause of science. During the evening Captain Lugard gave a very interesting account of East Africa, which evoked the political enthusiasm of the audience.

At a recent meeting of this Society, Mr. A. Vassall read a paper on the Recapitulation Theory, in which he alluded to Dr. Hurst's paper published in our pages. It is, we take the liberty of telling Mr. Vassall, by no means fair to say that "Hurst's opposition simply consists of a direct negative."

In the final school of Animal Morphology at Oxford, Miss Lilian J. Gould, of

Somerville Hall, and Mr. M. D. Hill, of New College, have been placed in the First Class. In the current number of the *Quarterly Journal of Microscopical Science*, there is a paper by Miss Gould on the structure of the fresh-water Rhizopod, *Pelomyxa palustris*, to which Mr. Hill contributes a note. Both have also contributed to our own pages.

THE Conchological Society of Great Britain and Ireland held its annual meeting in Manchester, on June 9, when the President, Mr. W. E. Hoyle, delivered an address on the Classification of the Pelecypoda (Lamellibranchiata).

ON page 315 of our last volume we announced that Miss Brocklehurst had offered some £2,000 to erect a Museum in Macclesfield Public Park. Plans had been prepared for her by a well known architect, embodying the advice of museum experts. Unfortunately, these plans did not meet with the approval of the local surveyor. But the Town Council have now learnt that it is unwise to look a gift horse in the mouth. Miss Brocklehurst has withdrawn her offer.

THE University College of North Wales, Bangor, will, in October, open a Day Training College for the instruction of sixty students equally divided between the two sexes.

A SCIENTIFIC exploring expedition will shortly start for the Macdonnell Ranges in Central Australia. The expedition is at the cost of Mr. W. A. Horn, a member of the House of Assembly of South Australia. All the Australian Colonies, however, have been invited to send scientific men, and we learn that Professor Baldwin Spencer, of Melbourne, intends to take this opportunity of studying the fauna of this little-known region.

MR. W. H. SHRUBSOLE, F.G.S., of Sheerness-on-Sea, has been good enough to own to us a paper announcing that he is prepared to accept engagements to deliver Lectures on the "Curious Dwellers on our Shores," also on "Coal and its Products." It is not our custom to give advertisements for nothing, but, in the intervals of a busy life, Mr. Shrubsole has done so much for Natural Science that NATURAL SCIENCE can hardly refuse so slight a return.

WE learn from the *Botanical Gazette* that the Botanists of the United States are combining for the purpose of making "a compact and complete presentation of the North American flora, so far as it is known." Arrangements have been made for the publication of a "Systematic Botany of North America," under the editorial control of eight well-known botanists, while the co-operation of at least thirty more has already been secured. The work is to appear in seventeen volumes, with about five parts to each volume, and 100 pages to each part. It is hoped that five or six parts will appear annually, beginning with 1895. There will be no illustrations, but full references will be made to published figures. "Special features will be the examination of type-specimens, the citation of type localities, geographical distribution, and a discussion of the economic, palæontologic, and horticultural features of each order." As regards sequence of orders, the editors propose to follow Engler and Prantl in the "Pflanzenfamilien." It is thought that such a work will make information so accessible that study will be stimulated and knowledge extended much more rapidly. Its course of preparation also will doubtless develop many new investigators. The *Gazette* does not state what are to be the geographical limits of the flora, whether, for instance, "North America" includes Canada, which, we notice, is not represented on the editorial board.

A SECOND edition of the concise Guide to the Mitchell Library, Glasgow, has recently been issued. This free public library was provided by Mr. Stephen

Mitchell, a tobacco manufacturer of the city, who died in 1874. It was opened in 1877, in temporary premises in Ingram Street, but since 1890 has occupied the buildings in Miller Street formerly used by the Water Commissioners. Some idea of the size of the Library may be obtained from the fact that no less than 349 magazines and periodicals are placed during currency on the tables in the Magazine Room. Among them Natural Science occupies a prominent position, such journals as the *American Naturalist*, *Annals and Magazine of Natural History*, *Botanical Magazine*, *Entomologist*, *Entomologists' Monthly Magazine*, *Geographical Journal*, *Geological Magazine*, *Quarterly Journal of the Geological Society*, *Journal of the Royal Microscopical Society*, *Mineralogical Magazine*, NATURAL SCIENCE, *Nature*, *Science*, and *Zoologist*, finding a place in the series. One of the excellent rules provided by the representatives of the Founder sets forth that the Library should aim at representing every phase of human thought and every variety of human opinion, and that no book should be refused admission merely because it controverted present views on religion or politics. After all, Glasgow is not so black as she has been painted.

AN International Congress of Applied Chemistry will be held at Brussels, from the 4th to the 11th of August, under the patronage of the Belgian Government. There are four sections, representing (*a*) the sugar industry, (*b*) agricultural chemistry, (*c*) alimentation and public hygiene, (*d*) biological chemistry, dealing chiefly with fermentation. Among the objects to be undertaken by this Congress, we are very glad to note "un ensembles de mesures destinees a faciliter aux specialistes et techniciens l'acces rapide de toutes les publications qui les interessent." Those who wish for further details of the Congress should apply to Mr. F. Sachs, Rue d'Allemagne, 68, Bruxelles.

THE Shah of Persia has instituted a zoological garden mainly for the purpose of introducing the native animals of Europe to his subjects.

THE Vienna Academy of Sciences are preparing an expedition, under the direction of Dr. Steindachner, to explore the deep waters of the Mediterranean.

CORRESPONDENCE.

The Natural History of the Flower.

Mr. J. C. Willis (Natural Science, vol. iv., p. 347), I am glad to see, has arrived at precisely the same result in his experimental study of flowers as I maintained six years ago in my book, "The Origin of Floral Structures," etc., viz., that the phenomena bearing on sex, cleistogamy, etc., "are primarily due to the action upon the plant of external causes." The only point to which I cannot follow him is when he says, "Natural Selection apparently only begins to act later on." I would ask what *facts* are producible to *prove* that Natural Selection acts at all on the maintenance, if not the origin, of any floral and, indeed, other structures? Be it observed that his first conclusion, just stated, is based on positive facts which are accumulative; the second, that Natural Selection plays a part, is an *assumption* for which he supplies none.

Mr. Willis is by no means alone in thus introducing Natural Selection. For example, Mr. Cockerell, in describing the dwarf character of many high Alpine plants, which he attributes to the direct action of the environment, adds: "It appears, however, that Natural Selection may come into play," etc., and gives the hypothetical applications (*Nature*, xliii., 1891, p. 207). It seems to me these pot- ther similar appeals to Natural Selection are simply a sort of unconscious opin to Mr. Darwin; but since the adaptations as described by these authors are frag strations of plants varying in *definite* directions in response to the alte ment, surely it would be a higher tribute to him to quote his key description not only of such an origin of variation, but of its maintenance. not only does he say that "Natural Selection has no relation whatever he primary cause of any modification of structure" (*Anim. and Pl. under a.*, ii., p. 272); but he observes: "By the term definite action I mean a action of such a nature that when many individuals of the same variety re exposed during several generations to any change in their physical conditions of life, all, or nearly all, the individuals are modified in the same manner. A new subvariety would thus be produced without the aid of Natural Selection." (*Anim. and Pl.*, etc., ii., p. 271).

Mr. Darwin, arguing from cultivated plants—in which *indefinite* variation is the rule, so that artificial selection is absolutely necessary—*imagined* indefinite variation to be also the rule in Nature, and *definite* variation to be the exception. Such is, however, not the case. Variation in Nature is always, as Mr. Willis, Mr. Cockerell, and many other writers have of late years maintained, in strict adaptation to the direct action of the environment; in other words, *natural variation is always definite*. Hence Mr. Darwin himself proves, though contrary to the title of his book, that the origin and maintenance of specific characters are without the aid of Natural Selection.

George Henslow.

Sir,—From the fact that our flora resembles the flora of northern Europe in being visited by a larger proportion of flies than has been observed in the neighbouring regions of the Continent, Mr. J. C. Willis infers that "our flora would seem rather more closely allied to these northern floras in this respect than to the nearer Continental flora" (Natural Science, iv., p. 349). Before this inference can be accepted it will be necessary to prove that this resemblance is not merely the result of similar circumstances, such as the natural preponderance of flies over other insects in our own and northern countries.

It may also be pointed out that Mr. Willis's general conclusion, at the bottom of p. 350, implies the inheritance of acquired characters. External causes, he says, produce various degrees of sexual development, on which "Natural Selection only begins to act later on." But Natural Selection, we are told, can only act on characters that are congenital. Does Mr. Willis appreciate the importance of his own conclusion, and what has he to say to this interpretation of it?

ENTOMOPHILOS.

THE HATCHING OF THE OCTOPUS.

MISS AGNES CRANE writes expressing astonishment at the ignorance displayed in our notes on the octopus in last month's "Notes and Comments." She says:—

"As a matter of fact the 'brooding' of the 'hen' octopus and the hatching of the eggs was a fifty days' wonder at the Brighton Aquarium many years ago, and the subject of general comment in the daily and weekly Press. I have watched the female often hovering over the two dozen or more live oysters she had sedulously accumulated in a rocky crevice of her tank near the glass for weeks together. Full descriptions appeared in the *Times*, *Land and Water*, *The Field*, etc., at the time, and Mr. Henry Lee subsequently devoted a whole chapter to 'The Spawning of the Octopus,' recording his observations in his 'Aquarium Notes' on 'The Octopus of Fiction and the Octopus of Fact,' published by Chapman and Hall in 1875.

"Therein he relates that the female octopus deposits her eggs, 'which look like little grains of rice,' in strings, at intervals, for three days, a fair-sized specimen producing ten or twelve long strings, or clusters, or a possible progeny of from forty to fifty thousand individuals, that she broods over them, guards them, and cleanses them, taking but *little food* the while, and becoming quite exhausted by her maternal cares.

"Moreover, your contributor used the word 'incubation' loosely, and seemed to imply that the octopus sits upon its eggs for the purpose of hatching them by the heat of her body. Mr. Lee held this theory not proven, as he succeeded in hatching out a brood of several hundred young octopods from ova which had been removed from the mother in question and placed in a tank by themselves.

"Young octopods are described 'as almost as big as a flea, and, when irritated, of much the same colour,' and as able to assimilate their hue to the colour of their surroundings even before they have left the envelope. The brood referred to lived about three weeks, but ultimately succumbed, possibly to the over-assiduous attentions of their scientific dry nurses: at that time Mr. Frank Buckland, Mr. Henry Lee, and Mr. W. Saville Kent were all professionally attached to the Institution. The eggs of the Sepia and of *Loligo* were also hatched out independently at the Aquarium when removed from parental care. The octopus, therefore, probably guards, cares for, and hatches her eggs just as the nest-building 'fifteen-spined' sticklebacks do, and not in the usual sense of the word incubation. Mr. Lee stated 'that octopods he had known' differed much in their manner of 'brooding' over their eggs. He also recorded the occurrence of a full-grown female octopus and her eggs within an earthen two-gallon carboy which had been dredged in the channel, and of which the neck measured only two inches in diameter. But I have said more than enough to prove that the 'hatching of the octopus' has not been forgotten since the days of Aristotle.

[We protest against the assumption that "incubation" involves heat. The term actually was applied to fish by Owen.—ED.]

TO CORRESPONDENTS.

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NOTES AND COMMENTS.

Vox Populi.

WHATEVER Mr. Arthur J. Balfour says is interesting, whether one agrees with it or not, and most people will therefore have read the remarks that he made when he took the chair at a recent lecture by Professor J. Shield Nicholson. One of Mr. Balfour's points was the necessity for a separation between science and popular opinion. Questions of science cannot be decided by universal suffrage. Science must be allowed free play and room for growth altogether outside the influence of popular forces. Science must be kept from the vulgar, and the vulgar must be kept from science.

Looking at things, as is his wont, through the clear and rarefied air of philosophy, Mr. Balfour could not fail to apprehend the essential distinction that must always exist between the intellectuality of scientific investigation and the sentiment of popular feeling; he could not fail to discriminate between the attitude of mind that pursues knowledge for its own sake, sacrificing the most cherished convictions of the multitude on the altar of a severe truth, and that very opposite spirit which views all things through the distorting glasses of utilitarianism, seeking always for some advantage either in this world or in that which is to come. Now these two modes of approaching Nature are so distinct that it is quite impossible to unite them. Each is bound to live and move independent of the other. So far, then, as Science is concerned, we do not think that the public will ever spoil her investigations. Mr. Balfour's warning is therefore not required.

When, however, we look at things from a somewhat more practical standpoint, we shall discover many advantages that may accrue to both parties from the promotion of a closer alliance between the two than Mr. Balfour seems to favour. Not merely need we have no fear of injuring Science, but we may even gain for her some additional powers. Scientific men have, or ought to have, a wise diffidence in the stability of their own theories; perceiving how far

the unknown still exceeds the known, they would be the last to rashly alter their conduct in obedience to each variation of belief that appears to be necessitated by each fresh discovery. But they are unable, even if they wished, to keep the new discoveries and the new theories from affecting the larger public. Here a danger lies. This public, vaguely expectant of some message or some new rule of conduct, eagerly grasps at such fragments of scientific theory as are conveyed to it in the diluted form of books like "The Ascent of Man." Then, of a sudden, the vessel of science tacks, and the public, seeing itself left behind, cries out that it is deceived and turns its former praise into ridicule or contempt. Our hope of progress then would seem to lie, not in refusing the laity such crumbs of scientific fact as they may have a fancy for, but in giving them to understand what is the true scientific spirit.

Often enough in these pages we have dwelt upon the aid that can be given to science, not merely by the scientific amateur, but by every member of the public. Intimate relations are bound to exist between the two sides, and instead of withdrawing ourselves into some cloistered calm, we desire to promote those relations that they may enter the phase of a cordial and intelligent affection. Still a warning is needed. It is by no means desirable that those without a proper training should attempt to engage in the more technical branches of scientific work. No one has any objection to people amusing themselves with music or painting, but we do not wish to have our ears distracted by immovable piano-organs or our eye affronted by glaring crudities of advertisement. So with scientific work. There is much done by well-meaning people that has merely to be undone by the despairing specialist. What we must aim at is to direct this praiseworthy energy of these good folk into channels of more service to us and of no less profit to themselves.

THE BRITISH ASSOCIATION.

IN extending the brotherhood of Science, not merely among the workers themselves, but also, as just intimated, with the world at large, one of the more important instruments is the British Association. For this Association, unlike some others which have been founded in imitation of it, has never forgotten that one of its chief aims is to introduce the facts and principles of Science to those who would otherwise pass them by as belonging to a sphere other than their own. And this idea is really the explanation of much that some of the more ascetic enthusiasts occasionally object to in the working of this organisation. The popular lectures, the social gatherings, and even the picnics play their own good part in the production of a better understanding between the usually indifferent public and those whom they welcome as dreary pedants, but part from as good fellows like themselves.

At the least it is admitted, even by the grumblers among us, that this annual gathering extracts for Science an amount of nutriment in the shape of golden guineas not altogether to be despised. But what we would rather lay stress upon are such benefits as the fresh impetus given to scientific investigation in each place that is visited, the friendships that professional workers often form with local amateurs, and the general awakening of the public to the fact that, if on the one hand Science helps them, it is, on the other hand, well within their power to offer in return help in many ways previously unthought of. Looking at the matter from these two points of view, there are certain considerations that we may take this opportunity of bringing to the notice of our readers, many of whom will doubtless shortly be attending the meeting at Oxford.

Firstly, as regards the matter brought to the notice of the Sections. It may not be too late to express a hope that contributors of papers to the forthcoming meeting will remember that there is a large body of members who expect entertainment other than the customary highly complicated paper dealing with minute details in an obscure corner of scientific work. Many people bring dry papers which would rapidly empty a learned society, and void them on a respectable hard-working audience which has come to hear some new po-ng that is addressed to their intelligence. The aspect of the opir-ence, if it remain under such an infliction, is generally saint-like frag resigned; in fact, a British Association audience is one of the alt- best in the world, and all rightly thinking and feeling people k-e to see it tortured. One of the things most appreciated in the ast was the formal discussion of some selected subject. This was -rought into contempt, principally, if our memory serves, by a discussion on stays some years ago; and while it might be advisable to refrain from inviting a possible recurrence of such demoralisation, something ought to be found to take the place of the "discussion." The secretaries of Sections have an opportunity here of improving the meetings by bringing together the papers on allied subjects, having them read without comment at the time, and finishing up each group of papers with a general discussion of them. It would save time, give wider scope to discussion, and make things more interesting to the outsider and probably to the scientific Pharisee as well. A time limit should be fixed for each group of papers, and the discussion of them could be announced in the daily journal; thus members would learn not only where they were, but where they wished to be at given times.

Turning now to the finance of the Association, we hope that someone will raise the question of the invested funds at the general committee. A representation as to this matter was recently made to the Council by a body of scientific men of exceptional weight, and it was pointed out that the invested funds greatly exceeded the necessary amount set aside against liabilities to life-members. It was asked

that the balance should be used in the lean years of the Association to supplement the grants-fund. The present position of affairs is unbusinesslike and deserves another description than "cautious," for which quality some praise it. Every penny, beyond the reserve fund to secure liability to life-members, should be spent. The Association has no authority to treasure it up for posterity; the money belongs to the contemporary members, who wish to see it put to useful work.

Among those to whom we have previously alluded as perplexing Science with their unwelcome attentions, a foremost place must be assigned to the British Association "crank," that peculiar type which appears annually for one crowded week of glorious activity and hibernates—no man knows where. The delicate question of dealing with these dear people has always an importance at a great meeting, such as the Oxford one will be. Many things have been tried, closure, etc., etc.; but the most successful is an enterprising, courteous, spirited, and ingenious secretary attached to a Section for this special duty. The system has its drawbacks, since, in one instance of marked success, the zealous official in question had actually to flee from the vengeance that awaited him when his wiles were discovered, and he has not dared to show his face again at a British Association meeting. But the cranks come back.

THE MARINE BIOLOGICAL ASSOCIATION.

AMONG scientific institutions that justly seek for the aid which, as we have said, the public can so well bestow, must be mentioned the above Association, which has just distributed a letter signed by the President, Professor Lankester, and the Hon. Secretary, Mr. G. H. Fowler, accompanied by a brief biography of the Society. As is the case with most of our scientific societies, the Marine Biological Association has not, by any means, too large an income for its needs. Considering that the Government recognises the importance of the investigations, carried out under the direction of the Council of the Society, in economic questions affecting our fisheries, by an annual grant of money, it is to be hoped that the public will individually reply to the appeal of the Association. The cost of membership is not large—one guinea per annum—and a considerable extension of the list of members would remove many restrictions to the usefulness of the laboratory. The researches carried out have resulted in a great deal of information with regard to such important matters as the size at which various food-fishes reach maturity, which should lead to useful legislation on the artificial fertilisation of fish, the history of the sardine, which has been shown to be simply a pilchard one year of age, and so forth. The more purely scientific work cannot be supposed to appeal to the public at large, important though it is. But those persons who appreciate the morning sole and the midnight lobster

might be expected to further the more abundant supply of these dainties. The efforts of the Association in breeding and protecting these and other food-fishes, backed by legislation against their unnecessary destruction, will unquestionably improve our fisheries; but there is a need for increased financial support—the total income being at present only £2,199, as against £70,000 which the United States Fish Commission gets from the American Government.

Natural History as a Recreation for Sailors.

We have mentioned the aid that laymen can render to Science, and few of them have better opportunities for observing natural phenomena and natural objects than they that go down to the sea in ships. And yet we have often been surprised at the small amount of collecting and observing that is done by Naval men in comparison with their brothers of the Army, although in distant parts of the world both alike have time on their hands and freedom to use it. Of course, we do not now refer to those special exploring expeditions like those of the "Challenger" and the "Blake" in recent years, or the long-sustained explorations of the French in the early part of the century, but to the private collection that is or might be done by those stationed abroad; and India in particular comes to our memory. We are therefore glad to see, in the *Nautical Magazine* for a short paper with the title that we have placed at the head of this It is written by Captain D. Wilson-Barker, who, as captain the training-ship "Worcester," has excellent opportunities of tilling into the young mind some appreciation of the beauties and terests of marine animals and plants; and it is to be hoped that ur future race of seamen will be more prolific in collectors than that of the past has been.

We are especially interested in one paragraph in this paper, and that is the following:—"It is curious to see the particular whale which frequents this coast [Brazil], and has very long pectoral fins, gambolling about, waving high in the air those long fins, which, in the distance, glisten like gigantic sword-blades." In our March number, it will be remembered, Mr. Lydekker gave an account of a whale-fight off the same coast. Captain Wilson-Barker says, in a letter he has been good enough to send us, that he had an opportunity of observing these whales for eight months while repairing cables off Brazil. He says the pectoral fins, which nearly reach twenty feet in length, are constantly mistaken for threshers; the outer surface, in common with the whole upper part of the whale is black; the inner surface, in common with the belly, is a glistening white. He therefore suggests that the distinct colouring of the inner and outer surfaces of the fin, alternately exposed during the gambolling of the whale seen by Mr. Lydekker, would give the impression of there being two distinct animals. And this interesting note seems to carry conviction.

THE WORM, THE GASTROPOD, THE CORAL AND THE BIVALVE.

STRANGE as any fairy story is this other true tale of the sea that Mr. E. L. Bouvier tells in *Comptes Rendus* for July, 1894 (vol. cxix., pp. 96-98). Once upon a time it was supposed that certain Madrepore corals, named *Heterocyathus* and *Heteropsammia*, built their skeleton around the shell of a living shell-fish, which prolonged its naturally-coiled shell in the form of a tube through the enveloping coral; and this was the opinion of Deshayes, Milne-Edwards, and Haime. This, however, was not quite correct, as Mr. Bouvier is now able to prove from material collected at Aden by Dr. Jousseaume. The curious facts are as follow.

The polyps of *Heterocyathus* and *Heteropsammia* fix themselves, probably at the close of embryonic life, on the minute but empty shells of various species of Gastropods. As soon as each shell is chosen by a polyp, it receives a new guest in the person of a young worm called *Aspidosiphon*, one of the Gephyreans. The little worm enters the shell and rolls itself in a corresponding spiral. Then the coral outside and the worm inside grow simultaneously. On the one hand, the coral soon completely covers the shell and threatens the imprisonment of the worm; but the worm, on its part, continues to grow in the form of a loose spiral, and thus prolongs the coiled ca[illegible]emed of the shell through the tissues of the coral, passing to the ext[illegible]em by by a round opening. At the same time, the worm secretes a t[illegible]ded in which appears at first to be an actual continuation of the shell, [illegible] d been which differs from it by its less thickness, its more intimate uni[illegible]ated, with the skeleton of the coral, and by the rougher appearance of i[illegible]heir inner surface. In *Heteropsammia* this tube usually reaches the ex[illegible]ed ternal opening; but in *Heterocyathus* it is formed more slowly and does[illegible]us not reach the exterior. This single external opening is, however, not enough to bring aërated water for the respiration of *Aspidosiphon*, so it sets to work, apparently by a secretion from glands in its skin, to dissolve long perforations through the substance of the coral.

The worm has a long proboscis, terminated by a crown of short tentacles, and covered with several rows of pointed hooks. It can thrust out this proboscis to catch its prey, and by sticking its hooks in the ground, can, as Dr. Jousseaume has seen, drag along itself and partner. The worm has also two stout horny shields, one of which is placed near the anus at the base of the proboscis, and, when the latter organ is drawn in, it serves to close the opening of the tube. The other shield is at the other end of the animal, and its particular use is not known. The *Aspidosiphon* that lives with *Heteropsammia* is a different species to that living with *Heterocyathus*.

But we are not at the end of the story yet. A home having been thus prepared by *Aspidosiphon*, a little bivalve mollusc called *Kellia*, which seems given to these lazy habits, comes and takes lodgings in the depressions of the tube, and boards on the food thater

comes in through the respiratory perforations above mentioned. Mr. Bouvier compares the commensalism of the worm and the coral with that existing beween *Parapagurus pilosimanus* and colonies of *Epizoanthus*. Similarly, the commensalism of the bivalve and the worm may be likened to that obtaining in South Australia between *Ephippodonta* and a burrowing shrimp.

Science at a Picnic.

If we have ventured to say a good word for the much-abused garden-parties and excursions of the British Association, we have no wish that such license should be extended to other bodies which are of a somewhat similar character, but which differ in the fact that the primary objects of their meetings are work and discussion.

In our news column we give some account of the work done at the recent meeting of the Museums' Association. Next year the curators are to meet at Edinburgh, and we trust that the local committee of the northern capital will learn a few lessons from the Dublin meeting. Irish hospitality was so generous and overwhelming that it seriously detracted from what should have been the first business of the meeting, the reading and discussion of papers and practical proposals. It will scarcely be credited that, though the members spent the best part of a week in Dublin, the time allotted to the long list of fragers, which included many besides those we have quoted, was only alto hours. Considerable complaint was heard at the way in which ker th papers and discussion were, in consequence, burked; while the insertion of a day's excursion between the two days of meeting proved a vexatious to those who could ill spare their time even for the serious business of the Association. We think we cannot do more good to this hard-working body than by ventilating these complaints, which gratitude to its unwearying hosts would otherwise stifle.

Very similar is the cry that comes to us from the Antiquaries in congress at Burlington House. In a paper that went straight to the point, Mr. St. John Hope protested against the elaborate lunches that waste much of the time at local archæological meetings. Clear explanations of the facts that they went to study were enough to attract an audience without the addition of gratis meals. His experience was that intelligent appreciation was usually shown by an audience composed largely of working-men, which was in marked contrast to the listless inattention and often rude indifference of folk calling themselves ladies and gentlemen. Another objection to too great an acceptance of local hospitality was that they might become a tax upon the places they desired to visit. We ourselves remember an occasion when, out of a large party, only two individuals examined an interesting geological section, which the party had travelled many miles to see, but which they were hindered from studying by the prior attractions of welcoming speeches and lunch.

THE LIFE OF MUSEUMS.

MENTION of the Antiquaries in the same paragraph as the Museums' Association reminds us that the former seem strangely ignorant of the existence of the latter. At all events, the upshot of a discussion on local museums, which they carried on the other day, was the appointment of a committee to investigate the questions of arrangement and financial support and the working of the Museum and Free Library rate. It is an admirable thing that the archæologists should treat provincial museums with a proper seriousness, but all these questions have been discussed over and over again by the Museums' Association; and if these gentlemen are really so interested in the subject, how is it that none of those who spoke at the congress or who have been elected on its sub-committee have, so far as we can gather, with the exception of Mr. Ward of Cardiff, ever attended the meetings of that Association?

A propos of local museums, we have received a copy of the *Yorkshire Weekly Post* for June 23, in which the Science Editor, Mr. G. W. Murdoch, has some very sensible remarks on the better utilisation of those institutions. After alluding to the usual difficulties with which provincial (and we may add other) museums have to contend, such as too great enthusiasm for one section to the neglect of others, the lack of local financial support, government, illiterate town councillors, the vanity of donors, and the utilisation of the museum as a convenient rubbish heap, he proceeds to endorse with much approval several remarks of our own. From his own examination of many museums, both at home and abroad, Mr. Murdoch has, we are glad to find, come to the conclusion that a museum is of greatest value for the cause of Science, and best fulfils the purposes of its being when it is intimately connected with active teaching and original investigation. The account, which we are fortunately enabled to publish in this number, of the University Museum at Oxford will show that a museum which thus co-operates with the laboratory need never fear comparison with museums that confine their functions to those of the store-room and the holiday entertainment. The ideal museum would, in our opinion, be the intermediary between its own collectors in the field and its own researchers in the laboratory, with its exhibition galleries daily open to neighbouring students, and its reserve collections always ready for the inspection of specialists from every part of the world.

WINCHESTER ONCE MORE.

FROM a leading article in the last number of the *Wykehamist* we learn that the new Museum at Winchester College, to which, by the way, Mr. Murdoch alludes, is to be worked on such lines as we here advocate. "We hear talk," says the school journal, "of rooms for photography, modelling, drawing, and practical natural history, quite

apart from the exhibition rooms. It is to be a home not only of art, but of artists; not only of 'bugs,' but of bug-hunters; not only of fossils, but of those who are qualifying for fossilhood." But will so admirable an ideal, which would render this Museum a true temple of the Muses, ever be attained? We sadly doubt. We leave Wykehamists themselves to settle the question of site, as to which they have already begun to quarrel; but we would fain once more express our surprise at the scanty support this memorial of their great founder has met with. William of Wykeham was a scholar and an ecclesiastic, but he was, above all things, a practical man; we can, therefore, imagine no more fitting way of carrying on the very spirit of Wykeham into the centuries to come than by the introduction of this practical intercourse with nature and art into the educational methods of a school that has adhered somewhat too closely to the scholastic traditions of the Middle Ages. £4,319 16s. 2d. is not a large sum for such an object—is, indeed, miserably inadequate; but what is worse is that it has been subscribed by only 402 people, a number barely equal to that of the boys at present in the school. In the interests of Winchester itself, no less than those of modern education, we trust that a few more Wykehamists will come out of the holes into which they have, like the traditional college spider, for the time retired.

Science and Nature.

IN our opening paragraph we spoke of the relation that daily hows more intimate between the scientific specialist in his laboratory pd the ordinary man in the street or in the field. Here is an example bpropriate to the season. For these are the days when tired and nirsty mortals crave above all things the refreshing charms of fruity icids. Were this a Saturday column of the *Pall Mall Gazette*, we would seek the choice word and the apt phrase to do worship before the dear delights of "lemon squash" or those more humble, prepared drinks that owe their flavour and their fragrance to commercial citric acid. An article in the *Kew Bulletin*, published some months ago (1894, pp. 103-108), spread dire consternation among those who grow the lime and the lemon in the south of Europe and in the West Indies; for it related a direct method of producing citric acid from sugar solution by growing therein a peculiar mould. But by the July number of the same periodical the fears of fruit-growers are, for the time, set at rest. On the authority of a distinguished firm of pharmacists, it is said that the practical difficulties of turning a laboratory experiment into a commercial process are so great, that for a long while to come we must continue to look to Nature for our supplies of the fragrant acid. Her Majesty's Ambassador at Berlin writes in the same strain, but with less confidence. The Director at the Manufactory of Chemical Products at Thann, in Alsace, assures her Majesty's representative, through the German Foreign Office, that "certain

difficulties have been encountered which must be overcome before there can be any question of the mercantile utilisation of the process in question." But he also states that the result of experiments "increases the prospect of ultimate success."

This is only another straw showing that the wind is blowing unfavourably to the simple pursuit of agriculture. The sails of farmers and fruit-growers, of gardeners and foresters, must be trimmed scientifically if the ship is to move. In earlier times "to plough and sow, to reap and mow" according to traditional methods was sufficient. In a good season the kindly earth yielded abundantly; in a bad season a hungry population gave a better price. But now, facilities of transport equalise, and therefore minimise, the varying results of the weather all over the globe. And still more, the rapid advance of scientific methods is dogging the heels of slow-moving Nature. Natural products of every kind are being produced more rapidly, more cheaply, and more certainly by scientific aid. "Protection," "Bimetallism," and a dozen other remedies may be beneficial or harmful; it is not within our scope to discuss them. But we lay down an inevitable and indisputable proposition. Unless the agriculturists of England and her colonies choose to train themselves in scientific methods and to banish for ever their easy opportunism, our agriculture will be ruined.

SCIENCE AND ART.

IF Science renders service to those that supply the practical [illegible] of the world, no less can she aid the ministers of its more ethe[illegible] pleasures. The debt that the fine arts owe to Science was dwelt u[illegible] a few years ago by Dr. E. du Bois-Reymond, whose address, recen[illegible] republished in the Smithsonian Report for 1891, we have just had th[illegible] pleasure of re-reading in a copy of that Report sent to us. But it i[illegible] only at the close of this interesting disquisition that the eminent physiologist asks the question, "What have sculpture and painting been able to do for science in return for its various services?" And then he will hardly stay for an answer. In fact, the difference between the artistic and scientific modes of approaching nature is so great, that we fear no very satisfactory answer can be given. Even in so obvious a sphere as the representation of natural objects, artists, working to the end of art, have, as a rule, not produced results sufficiently reliable to serve the turn of Science; while to refer under this head to the more or less accurate drawings intended to elucidate scientific writings would be a gratuitous insult to Art. Such notable exceptions as have existed only make more manifest the general prevalence of inaccuracy on the one hand and absence of artistic feeling on the other. If some return to nature has been shown by our English artists of this century, oddly enough largely in consequence of the teaching of that very Ruskin who deplores the pernicious influence of science, and calls forth the scorn of Professor Du Bois-Reymond, yet the present

tendency seems to be in quite an opposite direction. Some of our *fin-de-siècle* young men, influenced, it would seem, by Japanese impressionism and Japanese grotesqueness, seem to have forgotten the chief ideal of the Japanese artist, who, like Caleb Plummer, "tries to go as near to nature as he can for sixpence."

An interesting example of the assistance which the accurate yet delightful art of Japan may render to the scientific enquirer occurs in a paper by Dr. C. Schlumberger in the last number of the Memoirs of the Zoological Society of France (vol. vii., p. 63, and pl. ii.). The so-called dancing mice of Japan are a breed as to the origin of which there has been some little discussion. The peculiarity of these animals is that they twist rapidly round, apparently in pursuit of their own tails, as kittens or puppies are sometimes made to do with us. This dervish-like habit is supposed to be due to a crook in the brain, a mild insanity perhaps produced by some artificial selection or cross-breeding. Now, Dr. Schlumberger has come across a Japanese *netsuké*, the carved ornament of a tobacco-pouch, which represents a family of these dancing mice. The father and mother and eight little ones are reproduced with the utmost minuteness in characteristic attitudes. The chief interest of this work of art lies, however, in its colouring. The father and mother and four of the young ones thin white coats spotted with black; two of the young are quite black audi wo quite white. The mother and the two white young have and yes, while those of all the others are black. Dr. Schlumberger very fore infers that the dancing mice of Japan are the product of ha cial crossing between well-defined melanic and albino varieties.

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In Old Japan.

A propos of Japan, Mr. F. A. Bather, whose recent report on Natural Science in that country will be remembered by our readers, favours us with the following note:—

"Mr. Kumagusu Minakata, whose knowledge of Oriental science has often been shown in his interesting letters to *Nature*, points out that, in the course of transcribing my notes, I have confused the date of Kaempfer's birth with that of his arrival at Nagasaki. He was born in 1651, and first visited Japan in 1690. Consequently the interval between Kaempfer and Siebold was not 175 years, as stated on p. 24, but 135. . . . Some observations condensed from Mr. Minakata's letters to me may serve to supplement and correct my hasty sketch. So far as the past is concerned, the title 'Natural Science in Japan' should be held to refer to European science, and not to the knowledge that the Japanese themselves had of their own lands and seas. Crude as it was, yet the Japanese, imbued with Confucian maxims, vivified by poetical learning, excited by magical formulæ and anxious for medical knowledge, had a Natural Science of their own, which, though borrowed from the Chinese, was by no means unimproved since its transportation. For instance, Li Shi-Chin's 'System of Materia Medica'—of which the Japanese naturalists were the commentators, as the mediæval scholars of Europe were of Aristotle and Pliny—placed such parasitic phænogams as *Gastrodia*

and *Orobanche* in the division 'Mountain herbs.' The Japanese scholars, though they might be childish admirers of the Chinese authorities, did not hesitate to remove these plants to another division, 'the Fungi.' Though this may seem laughable to modern botanists, still it implies the existence of the idea of parasitism or saprophytism, a conception which must have been based on long-continued observations; and it must be remembered that a European taxonomist so recent as Lindley esteemed physiological functions as of major importance in the arrangement of the vegetable kingdom. Thus the germ of science was not utterly wanting among the naturalists of old Japan. In the days of Kaempfer, it is true, the naturalist, properly so called, was very rare in Japan, the few that existed being rather Confucianist or medical scholars. But when Siebold arrived, the naturalist had already been developed by the efforts of many, among whom Ranzan Ono was conspicuous. This man, eulogised by Savatier as 'le Linné du Japon,' has left as his memorial the great 'Guide to the Study of Materia Medica,' which embodied his researches among Japanese and Chinese literature on botany, zoology, and mineralogy. Iwasaki Tsunemasa, mentioned on p. 25, was his pupil.

"To the causes that I mentioned as hindering the progress of science in Japan, Mr. Minakata adds the inattention of Europeans to what was and is doing in that country. This it was the aim of my paper to help to counteract."

THE HONGKONG PLAGUE.

WE cannot leave the Far East without referring to the *[illegible]* now decimating the Chinese dwellers in our city of Victoria.

Among us modern sanitation seems to have stamped out so *[illegible]* the deadlier pestilences that once prevailed. In London and mo[illegible] our large towns, typhus fever, at least in epidemic form, is almo[illegible] thing of the past, and it may be doubted whether the oriental plag[illegible] could obtain much foothold in this country, even if conveyed hithe[illegible] There is little reason to doubt that this epidemic is identical with th[illegible] Black Death of the fourteenth century, and the Plague of the seven[illegible] teenth. As in the case of typhus fever, the essential conditions for it[illegible] propagation appear to be filth and overcrowding, and in these respects China is probably worse than was our own country in the Middle Ages. That the virus is a living organism few will doubt, and it must be an inspiriting thing to the mind of a bacteriologist to have the chance of studying a mediæval pestilence with appliances and methods of the most approved modern kinds. Professor Kitasato, of Tokio, is already on the spot, and is asserted to have discovered in the glandular swellings a bacillus characteristic of the disease. The details of his observations will be awaited with interest by pathologists, since few could have hoped that the Black Death would ever be traced to its ultimate cause in so modern an organism as a bacillus.

SEWER AIR.

TURNING to similar dangers nearer home, faulty drains and sewer air are among those regarded by the householder with especial

abhorrence. His surprise will probably be no greater than his satisfaction when he learns from the Reports on Sewer Air recently presented to the London County Council by Mr. J. Parry Laws, that this enemy is in many respects less black than has been painted. Mr. Laws' experiments only confirm the results previously obtained by others in this country and abroad, but they will appeal to the Londoner with particular force, as having been carried out in some of the oldest, and, in some instances, the worst of his own sewers. They deal mainly with the number and character of the bacteria of sewer air as compared with those of fresh air taken at the same time in the vicinity, under normal and abnormal conditions. Normal sewage contains enormous numbers of micro-organisms—from four to six millions or more to the cubic centimetre—and it might have been expected that air in prolonged contact with such decomposing and fermenting sewage would have become highly charged with bacteria. In most cases, however, Mr. Laws found, as others had found before him, that fewer micro-organisms were present in sewer air than in fresh air taken at the same time, and, which is more important, that those present were related to and apparently derived from the outside air, and not from the sewage. No effect on their number seemed to be produced by moderate splashing, or, in an experimental sewer, by a considerable increase in the velocity of the air current, or even by flushing air through a sewer which had been kept empty for twelve hours.

A mere knowledge of the number of bacteria present is of less importance than an insight into their nature, and an attempt was made in every case to determine the species present. Not only were no pathogenic species found, if we except one or two feebly pathogenic *staphylococci*, which would not harm us if they could, but the bulk of the species present were of a positively ornamental character, and we read with pleasure that our sewers are inhabited by *Sarcina aurantiaca*, *Bacillus aureus*, and *Micrococcus candicans*. If this be so, it must be admitted that, whatever the injurious effects of sewer air may be, we have no experimental evidence of the presence in it of specific germs derived from the sewage. And in this connection may be noted the well-known fact that sewer men are not more liable to zymotic disease than other members of the community. The baffled hygienist must hence take refuge in hypothetical poisons of a chemical nature if he wishes to explain the deleterious effects of sewer air. The evidence of such injurious action is so strong that it cannot be ignored, and it may well be that the effect is a secondary one, and produced by lowered powers of resistance to specific poisons derived from other sources.

Science and Literature.

Meanwhile we have strayed rather far from Professor Du Bois-Reymond and his views on art. Let us hark back for a moment, for

there were some closing thoughts that caught our attention. A strictly scientific paper need not, he says, be without its artistic aspect; it may "be made as finished a piece of writing as a work of fiction." The qualities demanded are not beauty of phrase and aptness of metaphor, but lucid language, logical arrangement, and a due subordination of the various observations to the conclusion of the whole. "To strive after such perfection will always repay the trouble to men of science; for it is the best means of testing whether a chain of reasoning is faultlessly complete." The popular exposition of science may however, retain the ornamental character of literary art to a greater extent. The literature of knowledge, as a brilliant literary critic of the *Athenæum* calls it, may for these purposes invoke the artifices of the literature of power. The modern rush after scientific and technical education has brought with it an aversion to the other aspects of culture; and the practical pedant of to-day scoffs at literary form as unnecessary, and at the graces of style as "high-falutin." Against this tendency we have always protested, and shall continue to do so. For to see the material world alone is to be blind to one-half of nature.

But if we would ally Science with Literature, let us at least ask that the friendship be mutual; for so then will be the benefits. "Everyone," writes Sir Mountstuart Grant-Duff, "must be cons[illegible]d [illegible]. vii., of the curious effort in much modern writing to supply the a[illegible] of fresh facts and ideas, by saying old things in a new and much difficult way. For a moment the strange contortions of the [illegible] attract our attention; but it is presently found that his perform[illegible] is a mere acrobatic feat, proving nothing more than the presence [illegible], certain cleverness. The mind of the reader is neither enriched [illegible]e soothed. There is but one remedy, and that is to greatly increa[illegible] the number of facts with which literature deals." The new fact[illegible] with which Literature is called upon to enrich her store are those [illegible] Nature and Natural Science. "Literature will, in her turn, repa[illegible] with interest all she gains from a larger commerce with Nature."

Food for Babes.

The above remarks of Sir M. E. Grant-Duff are quoted from an introduction that he has written to "A Handbook to the Study of Natural History for the Use of Beginners," which has recently been edited by the Lady Isabel Margesson, and contributed to by many writers. With the aims of this book we have much sympathy. At the same time, we must confess that it is not a little difficult to discover what its exact aims may be. Taken as a whole, the book would appear to be designed as the Natural History Codex of the P.N.E.U., that admirable body to which we have occasionally alluded. But the contributors to it seem to have had very different ideas of the way in which they were to fulfil their task. Some have clearly written for the parents; others as clearly for the children.

The consequence is a book that we should rather hesitate to put in the hands of our own children. For instance, we find it written that adaptation, mimicry, and the like "afford us beautiful opportunities for teaching both spiritual and moral lessons, without objectionable preaching, which go home to the hearts of children in such a way that the lesson, once learned, is never forgotten. For we must remember that children easily get hard and callous when left to themselves," and so on. It would never do to let a child behind the scenes in this way. On the other hand, if the book is for adults—even for parents—it is hardly necessary to tell them how to degrade the study of sea-weeds by turning them into Christmas cards.

We regret that we cannot speak better of this book, for many of the articles are useful in their way, and are on the whole free from the inaccuracies that disfigure most popular works of the kind; for this, indeed, the names of the writers are in most cases sufficient guarantee. Still the undoubted ability of some of the writers renders their articles the more disappointing. That on Zoology, for example, consists of lists of books far too comprehensive, interlarded with this sort of thing,—"Gradually we must pass from considering the bird as an intact unity like one of ourselves, to see it as a marvellous ... engine, with many parts or organs, as a great web of tissues, ... city of cells—competing and co-operating, and finally as an ...ed whirlpool of living matter, which, though ever changing as ...s of matter and energy pass in and out, yet retains its ...rity till death comes." We don't wish to use the word "high-...tin" so soon after uttering a *caveat* against it, but this really is a ...e "tall." Then again "The Study of Mosses" is doubtless most ...scinating; but who would be attracted thereto by these extracts ...om the description of a Moss plant? "Its base is fixed in the soil by numerous fine filaments, ... some of these may be above ground and green—these are the *protonema*; others are brown, and ramify in the soil—these are the *rhizoids*. The fruit, to which the term *sporogonium* is applied, consists of a *capsule* containing *spores*, placed either at the end of a stalk, the *seta*, or else sessile among the leaves." The chapter on Fungi is nearly as dreadful.

We can only hope that the parents who are expected to read this book will be able to distil from it something that shall be more suitable than the above extracts as food for babes.

The Attack on the Pole.

The present year will be a marked one in the annals of Polar exploration. Never before have so many expeditions been in the field, and never before have the resources of modern science been so called upon to deserve, if not to command, success. The account of the preparations for the Wellman expedition, with its aluminium

boats and bridges, read like a chapter from Jules Verne, and exemplified the imagination of the American journalist. The news recently brought by the "Saide" concerning this expedition is a little contradictory; for while we are told that the "Ragnvald Jarl" was only six days on its passage from Tromsö to Danes Island, Spitzbergen, and that the sea to the north of that island was remarkably free from ice, on the other hand, we learn that, in the opinion of Captain Johannesen, of the "Smeerenburg," the "Ragnvald Jarl" must have been subsequently beset and crushed by heavy pack-ice. This does not augur well for the immediate prospects of the Jackson-Harmsworth expedition, which, in the opinion of Arctic experts, is in any case starting six weeks too late. Since, however, the party are prepared for an absence of two or three years, a slight delay at the outset will perhaps not affect the ultimate result of the expedition. It is, besides, far more the object of Messrs. Jackson and Harmsworth that worthy scientific results should be obtained than that the Pole itself should be seriously threatened. The expedition is very thoroughly fitted out with scientific instruments, and carries besides several experienced observers, although we understand that at the last moment some difficulties arose with regard to the latter.

Almost simultaneously with the starting of the "Windward," the Peary auxiliary Polar expedition left St. John's on board the steamer "Falcon" for Inglefield Gulf, Greenland, to bring Lieutenant Peary's party. The vessel will call at Carey Island, where the unfortunate Swedish naturalists, Björling and Kallstenius, came to grief in their schooner the "Ripple" in 1892. Dr. Ohlin, a Swedish zoologist, accompanies the expedition chiefly for the purpose of searching for his two countrymen or their remains. The party on board the "Falcon" is also charged to explore Jones Sound and to make a chart of the coast. After this it will call for Mr. Peary, and return to St. John's about the end of September.

Whatever else may happen, as all these expeditions intend to observe the aurora on one definite plan, the results to meteorology cannot fail to be of exceptional interest.

THE POLLINATION OF THE YUCCA.

IN MAY, 1893 (NATURAL SCIENCE, ii., p. 321), we described in detail Trelease's studies on the pollination of the Yucca by the larvæ of the moth *Pronuba yuccasella*. In the Fifth Annual Report of the Missouri Botanical Garden (p. 137), Mr. J. C. Whitten has completed the details, previously incomplete, of the life-history of this interesting insect. He finds that the larvæ make their escape from the capsules, and enter the soil, during the rainy weather when the ground is softened and easy of penetration. He also makes the interesting notes that they do this during the daytime or at night, and not exclusively toward the end of the night, and that they drop quickly down to the ground by means of a silken thread.

I.

The Evolution of the Thames.

PROBABLY nothing has gained for geology a greater number of recruits than a love of the scenery and a desire to understand the origin of the physical features of the district in which one dwells. The arrangement of river systems has been an especially useful irritant; for no one provided with eyes and wits can travel about England without being puzzled by the singular anomalies in the courses of our rivers, which often appear to have been planned before the law that water should manage to find its own level had come into operation. The Exe, for example, rises on the north coast of Devonshire, almost within stone's throw of the Bristol Channel, from which separated only by the narrow "Hobby Drive"; nevertheless, it right across the county and enters the English Channel, instead ting its way to the shore near at hand. The fact that no one owed to go through the Hobby Drive without paying sixe, seemed at first to supply a possible explanation, since as river either would not or could not pay the toll, it had to go nd another way. In other parts of the country the rivers do actly the reverse, having cut their way through hills when an easy ourse lay open to them round the flanks. The desire to get at some explanation of these eccentricities which did not assign an element of "cussedness" to the rivers concerned, has given the writer, and no doubt many others, a first interest in geology.

The history of the Thames has been a comparatively simple one, and it has not had to record the sensational revolutions of such rivers as the Rhine or the Hudson, though great differences of opinion still exist as to its age and formation. During the past four years, several very important additions have been made to our knowledge of the subject; but before considering these, it may be advisable to refer to the main geological sequence in the district so as to get a time scale for comparison. The western part of the Thames Basin is formed of a series of clays and limestones belonging to the Oolitic Series. These are succeeded to the east by newer beds forming the Cretaceous system, of which the two most important members in this connection are the Chalk and some beds of chert formed of sponge remains occurring in the Lower Greensand to the south of the Thames.

Above the Chalk is a series of sands, clays, and pebble-beds forming the Lower London Tertiaries and London Clay: the latter is covered in places by sands and pebble-beds forming the Bagshot series. After this there is a great blank in the Thames Basin: when the geological record again commences, it does so only with a series of gravels, clays, and brickearths, which rarely contain any fossils except some derived from older deposits. In consequence, their correlation and methods of formation present problems of unusual difficulty. The following table summarises these beds in descending order:—[1]

I.—PLEISTOCENE.

(*a*) *Post-Boulder Clay.*—Brickearths and gravels at low levels in valleys of existing rivers. Most of the molluscs and mammals of existing species.

High-level terrace gravels and brickearths with remains of mammoth, arctic plants, etc.

(*b*) *Boulder Clay.*—Chalky Boulder Clay with fragments of rocks and fossils from the north of England.

II.—PRE-BOULDER CLAY, OF UNCERTAIN AGE.

Newer Plateau Gravels: Gravels containing many northern fragments and quartzite boulders.

Older Plateau Gravels: Gravels with many small quartz pebbles and vii.,
The "Westleton Shingle" of Professor Prestwich ar
Pre-Glacial Pebble Gravels of the Geological Survey

"*Southern Drift.*"—Partly synchronous with above.

III.—PLIOCENE.

Crags of Suffolk and Sand on Chalk at Lenham in Kent. y,
e

IV.—EOCENE.

Pebble Beds of Bagshot series.

The most important recent attempt to solve the problems of the origin of the Thames Valley is that of Professor Prestwich in a series of three papers entitled "On the Relation of the Westleton Beds . . . . and their Extension Inland," published in the *Quarterly Journal of the Geological Society* for 1890. The papers are the result of over 50 years of work, and describe some sections that were closed from examination before the younger generation of geologists saw the light. They are a summary of a life's work by the Nestor of British geologists, and from this, as well as from their intrinsic importance, demand respectful notice.

Professor Prestwich's theory of the sequence of events in the south-east of England is as follows. In Lower Pliocene times a sea spread

[1] In reference to the above table it should be noted that the only bed positively assigned to the Glacial is the Chalky Boulder Clay. The gravels that underlie this are no doubt closely connected with it, but were obviously deposited or re-arranged by water. To avoid ambiguity in the article the term "Glacial" is not used as a time name; and beds are simply referred to as being earlier or later than the Boulder Clay instead of as Pre- or Post-Glacial.

across from Belgium to Kent, and thence to Suffolk, where it continued till Upper Pliocene times. In Kent, this period of submergence was soon brought to an end, before the Crags of Suffolk had been deposited, by the elevation of the great arch that once covered the Weald. As the land there rose the summit was denuded away, and the material was washed down the northern slope as the "Southern Drift." This began in the Upper Pliocene, or at the time of the Norwich Crag, and continued into the Pleistocene. North of the Thames the Pleistocene series began with a marine shingle beach, which extended from Norfolk into Berkshire, and possibly further. The pebble beds in the valley of the Bure, which were included by Mr. H. B. Woodward in the Norwich Crag, and are generally known as the Bure Valley beds, are taken by Professor Prestwich as the type of this marine shingle. He has traced it from the Bure across Suffolk to Braintree, and regards as its southern extension the high level pre-Glacial gravels marked in red on the maps of the Geological Survey. These occur scattered over the higher hills of south-west Essex, Middlesex, Hertfordshire, Buckinghamshire, and Berkshire. The land then rose, and the erosion of the valleys of the Lea, Colne, etc., commenced; at the same time, the climate became more and more arctic, until it culminated in the "Glacial Period." The Chalky Boulder Clay was then deposited in the district by an ice-sheet from the north. Still later the Chalk escarpment was formed, and the river Thame and the Isis flowed northward along the valley at its foot, before they entered the Wash. After this the Chalk escarpment was breached by the Goring, and the basins of the Thame and Isis were connected with the valley of the Kennet, and thus formed the Thames. So, according to the Professor Prestwich, the formation of the Chalk and Oolite escarpments, and practically the whole of the physiography and river systems of the south-east of England, date from Glacial and Post-Glacial times.

The theory is a brilliant and fascinating one, as it gives a definite historical classification of the drifts, and a working hypothesis with which to determine the relations of a series of gravels otherwise in chaos. Nevertheless, there seems a general consensus of doubt as to the three main positions in Professor Prestwich's argument—viz., (1) The original continuity of the isolated patches of pebble gravel which he has grouped together as the Westleton Shingle; (2) the recent date of the Chalk escarpment and its breaching by the Thames; and (3) the submergence of the whole of the South-West of England below the sea in Pleistocene times. In regard to the first, there can be little doubt as to the former continuity of the Westleton beds from the Bure Valley into northern Essex as far south as Braintree. Similarly, the gravel patches in the London district which Professor Prestwich has referred to the Westleton Shingle all agree in being Pre-Glacial in their general composition, and were doubtless deposited by a common agency at approximately the same period. But the

gap between these and the nearest of the truly marine Westletons is a great one: the most north-easterly of the patches in the London area is at Highbeech, at the height of 370 feet; the nearest of the Suffolk and northern Essex series is 30 miles distant at Braintree, where the gravels are at the level of 240 feet. There seems, moreover, a considerable difference in the gravels of the two areas: those of the northern Essex and Suffolk group are probably truly marine, whereas, in the case of those of the London district, there seems no evidence to refer them to the same agency, except their gradual rise when followed to the south-west. The acceptance of the correlation and original continuity of the two sets of gravels in these two distant areas is essential to Professor Prestwich's hypothesis. It is almost impossible actually to disprove this, but the evidence in its favour is at present very limited.

The view that the whole of the South-East of England has been submerged below the sea in Pleistocene times is also one that has not found much favour. Messrs. Monckton and Herries, for example, certainly do not accept any submergence of the district in Essex around Brentwood and Warley since Eocene times (no. 3, pp. 22, 23; nos. 4 and 5). It may be objected that the patches of sand on the North Downs of Kent and Surrey, which are referred to the base of the Pliocene series, prove a subsidence at this period; but as far as I am aware, the only one of the patches certainly marine is at Ightham, in the neighbourhood of Lenham, east of Maidstone. This may indicate merely a gulf running from the Pliocene sea up the valley of the Swale. There is no proof whatever for the Pliocene age or marine origin of the sand patches elsewhere on the North Downs, such as that at Coulsdon.

Nevertheless, in spite of the general hesitation expressed, it seems impossible to get any one piece of direct evidence that is final against Professor Prestwich's conclusions. His theory is founded mainly on a series of inferences rather than direct proof, and its inferential evidence is all we have against it. Dr. Hicks's (6) demonstration that the Hendon Chalky Boulder Clay mantles the surface and slopes of Pre-Glacial hills and valleys, and Mr. T. V. Holmes's discovery of Boulder Clay at Upminster (8), showing that at least two-thirds of the Thames Valley had been eroded before the Chalky Boulder Clay was deposited, are neither of them final; for Professor Prestwich fully admits a considerable amount of erosion before the Chalky Boulder Clay, but later than his early Pleistocene Westleton Shingle. Both the cases described by Dr. Hicks and Mr. Holmes are to the south of the Westleton line, and far from the great Chiltern escarpment, on which the point in dispute must turn.

There are, however, two areas which give strong *à priori* evidence against the truth of Professor Prestwich's theory. These are the Chiltern Hills and the Downs of western Berkshire, the structure of which we will now proceed to consider.

The most instructive area in the Chilterns is that in the neighbourhood of Tring, Prince's Risboro', the Wycombes, and the valleys of the Missbourne and the Chess. It is all included in sheet no. 7 of the maps of the Geological Survey, of which the "Drift Edition" admirably shows the complex series of gravels in the district. These are also described in two of the Survey Memoirs (1). The map shows that the Chilterns consist of Chalk, covered by a varied series of drifts, intersected by a number of valleys which cut across the ridge from north-west to south-east. The most extensive of the drifts is a series of gravels and brickearths that are coloured pink on the map and marked as "Glacial." There is no Boulder Clay in this immediate district, but the gravels, though somewhat different in composition, appear to be continuous with those which pass under this deposit further to the east. They are, therefore, earlier than the Boulder Clay. Though marked as "Glacial," no one would be likely to maintain that they are Glacial in the same sense as is the Boulder Clay. They are certainly due to water action in some form, instead of to the direct agency of ice like a moraine. To avoid the ambiguity involved by the use of the term "Glacial," it may be advisable to call these the "Newer Plateau Gravels." They must be distinguished from another set, which may be called the "Older Plateau Gravels," which are the "Pre-Glacial Gravels" of the Geological Survey and the Westleton Shingle of Professor Prestwich. Pebbles of these occur in this area capping the outliers of Eocene beds at Penn and Lane End, near High Wycombe. The former existence of these two plateaux, each capped by its own set of gravels, was first demonstrated by Professor T. McKenny Hughes in 1869 (9).

A third set of gravels occurs on the floors of the set of valleys running through the Chalk ridge; as these occur right up to the watershed, it is clear that they must have been deposited at a time when the valleys continued further to the north.

The fact that the valleys of Bradenham, Hampden, and the Miss cut through the plateau and contain none of the "newer high level plateau gravels" shows that the valleys are later than the gravels. The valleys themselves, though now dry in their upper parts, breach the Chalk escarpment, and it is impossible to examine them without feeling that they are due to erosion by rivers that once rose some distance to the north. The breaches made by the heads of the valleys are most impressive; thus, that on the road from Wendover to Aylesbury crosses by a pass the summit of which is 503 feet high, while the ridge on either side rises to 790 and 800 feet. Similarly, the road from West Wycombe to Prince's Risboro' crosses through a breach at the height of 427 feet between points of the ridge that are 700 feet only three-quarters of a mile to north-east and south-west. The deposits on the floor of the valley are also suggestive and tell no less strongly in favour of the formation of the valleys of

former streams, for beds of gravel lie along them. Sections in the gravels on the floors of the dry Chalk valleys are scarce and unsatisfactory; there is a small one within the fork of a lane and the main road from Missenden to Prince's Risboro', just to the north-east of Hampden House. The hole shows a coarse gravel of unrolled flints with but little sand and without any oolitic material. Mr. Whitaker records a note by Mr. Jukes Browne upon the gravels at Saunderton in the next valley to the west, which show the same characters (1, p. 447.)

The nature of these gravels is important, as it is to their constitution that we have to trust to know what was the former course of the rivers that eroded the valleys in which they occur. There are two possible explanations: either these streams rose on the Oolites to the north, and are earlier than the Chalk escarpment, or they were confined to the Chalk (as the Chess still is), and existed when this formation extended much further to the north-west than at present. That this latter view is correct appears the more probable, as the gravels of these dry valleys are composed solely of material derived from the Chalk and the "pink" gravels which overlie it. If the head-waters of the rivers had been situated on Oolitic rocks, it is about certain that some fragments of these would occur in the gravel. It might be suggested that these have been eliminated owing distance of the valleys from the Oolites; but when we rememl fragments of these rocks occur in the Thames gravels on th side of the Chalk, and much further from the Oolites tha Saunderton—West Wycombe valley, this explanation seems in cient. That the valleys are later than the so-called "Gl Gravels" or Newer Plateau Gravels, we have already seen; bu very irregular nature of the floor of the valley, from which all tra of any former channel have been destroyed, shows that it is of so considerable age. To learn whether the valleys were earlier or late than the Boulder Clay, we must turn to a member of the series which cuts this deposit. The Lea serves our purpose very well; in its upper course it presents identical features with those of the Miss and Chess, while we know that at Hertford the Lea Valley was eroded in the period that intervened between those of the Newer Plateau Gravels and the Boulder Clay, deposits of the latter of which occur within it. If therefore, as seems highly probable, all the north-west to south-east valleys through the Chilterns were formed simultaneously, then the Chalk escarpment was breached in the period between the formation of the Newer Plateau Gravels and the Boulder Clay.

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The next point for consideration is as to the evidence to show the age of the escarpment itself. The distribution of the Boulder Clay gives us a good hint in this direction. This deposit was apparently formed by the southernmost portion of the East Anglian ice-sheet, which travelled from north-east to south-west: towards its end it appears to

have split into two tongues, one of which kept to the south of the Chalk ridge, as far as St. Albans, Hendon, and Finchley, while the other flowed along the foot of the Chalk escarpment past Hitchin and Dunstable, whence it trended off to the north-west towards Leckhampstead. The Chalk hills of this district thus rose above the level of the ice which lay upon their lower flanks.[2] This tongue of the glacier seems to have had but little erosive power and to have flowed only along lines of low land previously in existence. Thus in the valleys of the Brent and the Lea it simply occupied pre-existing valleys without eroding fresh ones. As, moreover, it was apparently unable to surmount the Chalk ridge, it does not appear likely that this thin lobe of ice eroded the valley it occupied on the north side of the escarpment. It is much more probable that it there extended so far south owing to the former occurrence of a valley along which it flowed. That is to say that the valley at the foot of the Chalk escarpment, and the escarpment itself, are both older than the Boulder Clay.

We have next to consider the question whether the Thames ever flowed up the valley of the Thame, across the watershed, and down the Ouse into the Wash. Conclusive evidence against this can only be obtained by a detailed study of the distribution of the gravels upon the watershed between the Thame and the Ouse. Until such evidence [illegible] pi[illegible]thcoming we are thrown back on two general considerations: in Chi[illegible]st place, the great irregularity and sinuous course of this watershed (Z[illegible]rs it very improbable that it could have been formed by a line De[illegible]evation which broke across the former course of a river. Further, G[illegible] only way in which the Thame could have surmounted the barrier [illegible]uld have been by its waters having stood at a higher level; there is M[illegible] evidence of any great lake which discharged to the north or of [illegible]ovements of the country which have since lowered the valley of the Thame. Apparently Professor Prestwich's only reason for suggesting this connection between the Thames and the Wash was the difficulty of explaining the formation of the gorge through the escarpment between Moulsford and Pangbourne. But this gorge is simply the southernmost of the series of parallel valleys through the Chilterns. The level of the floor of the gorge is lower than that of the passes through the escarpment further to the north-east, because the height both of the ridge and the passes descend as the former is followed to the south-west. The Thames gorge is so closely analogous to the series of valleys parallel to it, that it appears most probable that it was formed at the same time—viz., before the period of the Boulder Clay. We may summarise, then, the sequence in the Chilterns as follows:—

[2] Mr. Worthington Smith has recently described some drift deposits on the higher parts of the Downs near Dunstable as a "Boulder Clay." This has not been generally accepted as such; and even if it be glacial in origin, as it contains no northern boulders, and is exclusively composed of local materials, it does not prove that the northern ice-sheet ever occurred on the higher parts of the Downs.

(1.) Existence of a high plateau; formation upon this of the "Older Plateau Gravels" of Lane End, Penn, &c. (Coloured red on the Geol. Surv. Maps.)

(2.) Prolonged period of denudation, resulting in the formation of a second plateau, lower than the last, and interrupted at places by hills left undenuded. Deposition on this plateau of the "Newer Plateau Gravels." (Coloured pink on the Geol. Surv. Maps.) The Chalk escarpment was then in existence, but it was much higher and further to the north.

(3.) Erosion of the series of north-west to south-east valleys, including those of the Thames at Goring, and of the Miss, Chess and Loudwater.

(4.) Cutting back of the Chalk escarpment to approximately its present position by the erosion of the Thame and Ouse. This cut off the head waters of the Miss, Loudwater, etc.

(5.) Advance of the ice-sheet which deposited the Boulder Clay.

The only remaining question is whether it be possible to fix any definite date for this series of events. Everyone will admit that no. 5 was early Pleistocene, as it is generally considered that the arctic facies of the fauna in the Norwich Crag was due to the refrigeration of the climate, which culminated in the formation of our English glaciers. This gives us the latest date for the last member of the series. Professor Prestwich includes no. 1 also in the Pleistocene; but if we do not accept his correlation of these drifts with the Westleton beds, then these gravels may be of any age between the Eocene and the early Pleistocene. When we remember how little the structure of the country appears to have altered since the time of the Boulder Clay, it is a rather heavy order to crowd such a long series of events into the short time between the end of the Pliocene and the beginning of the deposition of the Boulder Clay. This affords further reasons against the acceptance of the Westleton age of the "Older Plateau Gravels," which may be allowed an extension backward into the Upper Tertiary, the exact amount of which cannot now be determined.

Let us now turn to the western end of the Berkshire—Chiltern escarpment, where valuable evidence of the Pre-Pleistocene age of the Chalk escarpment is afforded by the distribution of the Sarsen stones. These are huge masses of siliceous rock, now lying scattered over the Downs. Whence they came is not quite decided: they unquestionably represent some former bed of sand, which has now been denuded away and these boulders only left. They vary greatly in composition. They are believed to have been derived either from the Woolwich and Reading or the Bagshot beds, but probably came from more than one horizon. Some of them have been found *in situ* in the former, and thus unquestionably belong to them, while others occur on the London Clay and on Bagshot beds, and thus with equal certainty are far more recent than the Woolwich and Reading beds.

Some, moreover, occur in districts where it is probable that this formation never extended.

The distribution of the Sarsens enables us to approximately determine the original extension of the Tertiary Sands that yielded them. The Sarsens are abundant on both sides of the Kennet valley, on the Marlborough Downs, and thence along a line past Avebury north to Swindon. The blocks here are small, as they are also at Highworth and Hannington. They occur on the dip slope of the Chalk of the Berkshire Downs as far as Aldbourne and Lambourne, but they appear to be entirely absent from the higher part of the Downs. In a walk from Ashbury to Aldworth, along the edge of the Chalk escarpment past Uffington Castle and White Horse Hill, not a single Sarsen stone was seen, except those forming the cromlech and circle of Wayland Smith's cave and the famous "Blowing Stone." These are certainly not *in situ*: the Blowing Stone has been moved to its present position. Nor can I find any record of Sarsens on the escarpment. The highest point at which they occur is one mile from the edge of the Chalk escarpment. Mr. W. Cunnington, who knows the Wiltshire and Berkshire Downs better than anyone, says that their absence from the ridge is certainly not due to their having been removed by man; and my colleague, Mr. Andrews, has recently [illegible]rched the downs around Aldbourne and can find none nearer the [illegible]rpment than on Kingstone Down. Their absence from the [illegible]rpment seems to show that the Sarsen-yielding horizon thinned against the higher part of the Chalk, so that this apparently rose [illegible]ove the level of the water in which these sands were accumulating. [illegible]he estuary probably extended up the Kennet Valley and then over [illegible]e lower slopes of the Chalk Downs to Abury, and north of this till it [illegible]eached the Jurassic plain about Swindon. A further continuation may be indicated by some Sarsens which occur in the valley that runs from Swindon to Hannington and near Highworth. But in any case the Sarsens in the Swindon district are sufficient to prove that at least a part of the Berkshire Chalk escarpment was in existence in the time of the later set of Sarsen-yielding sands, *i.e.*, probably Upper Bagshot.

That the Berkshire escarpment took up its present position earlier than that of the Chilterns is shown very clearly by a comparison of their structure. (Figs. 1 and 2.) Thus, while the Chilterns is jagged and irregular and breached by numerous former stream courses, the Berkshire escarpment is regular and unbroken. The whole drainage system of the Chilterns has been altered since the date when the escarpment was formed, while in the Berkshire Downs the drainage is entirely resultant on the present slope of the country; thus the drainage of the latter is of the type known as subsequent, that of the former as antecedent.

There is a third line of argument which there is only space here briefly to indicate, viz., that the Thames and its tributaries

are essential members of the river systems of England to the south and east of the Humber and the Severn, and that the main outlines of this system were determined long before Pleistocene times. If we examine a map of this portion of England (Fig. 3), we see that all the rivers rise in a central area included within a radius of about thirty miles from Rugby. Hence rivers flow north-east into the Wash and the Humber, south-west into the Severn, and south-east into the Thames. If we examine a sketch-map showing these rivers, two facts are clearly brought out: first, that the drainage lines are dependent on the ridges of the Oolites; and, secondly, that they appear to be independent of the arrangement of the highlands of the chalk in the Chilterns. Thus, while the Thames and the valleys of the Loudwater, Miss, Chess, and Colne run right across the Chalk at right angles to the ridge, and in a south-easterly direction, the rivers of the Oolitic country, on the other hand, flow in the main to

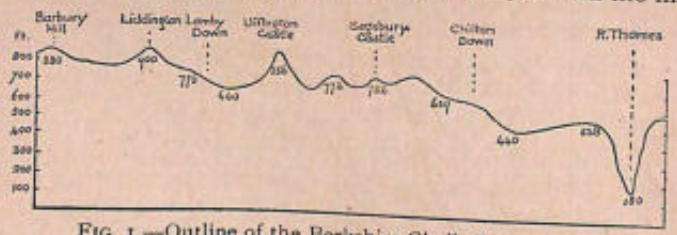


FIG. 1.—Outline of the Berkshire Chalk Escarpment.

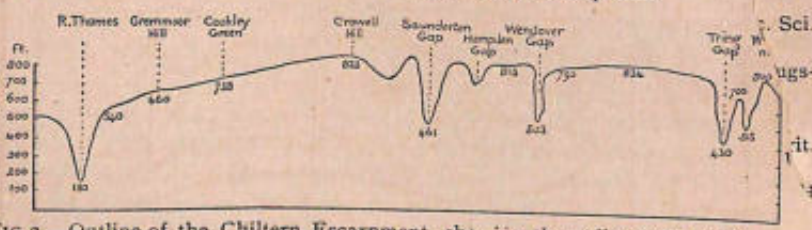


FIG 2.—Outline of the Chiltern Escarpment, showing the valleys of the Thames, Loudwater, Miss, and Colne.

north-east or south-west, parallel to the ridges. That is to say, the river systems of the Chalk country were antecedent to the hills, while in the Oolitic country they were subsequent or simultaneous. This admits of the simple and probable explanation that the rivers rose on a central plateau and radiated thence in every direction, flowing along the lines of least resistance. Those rivers whose course was parallel to the strike of the beds cut valleys through the soft clays, and left the harder limestones standing out as ridges between them. Those which drained the south-eastern slope had to cut their way at right angles to the strike, and thus formed deep narrow valleys through the Chalk ridge. At first these were probably all good sized streams, but the Thame and the Ouse which flowed in the valley between the Chalk and the Oolites cut the escarpment further and further to the south, and thus drained off the headwaters of the Buckinghamshire tributaries to the Thames.

The dates of these various episodes cannot be fixed very precisely. There are two outside limits: the valleys are earlier than the Boulder Clay. They are later than the Newer Plateau Gravels, but it is not possible to determine the exact age of these, for if we reject Professor Prestwich's hypothesis of their correlation with the Westleton beds, then they may extend back indefinitely into the Tertiary. The nearest approach to a determination of a maximum age of the great

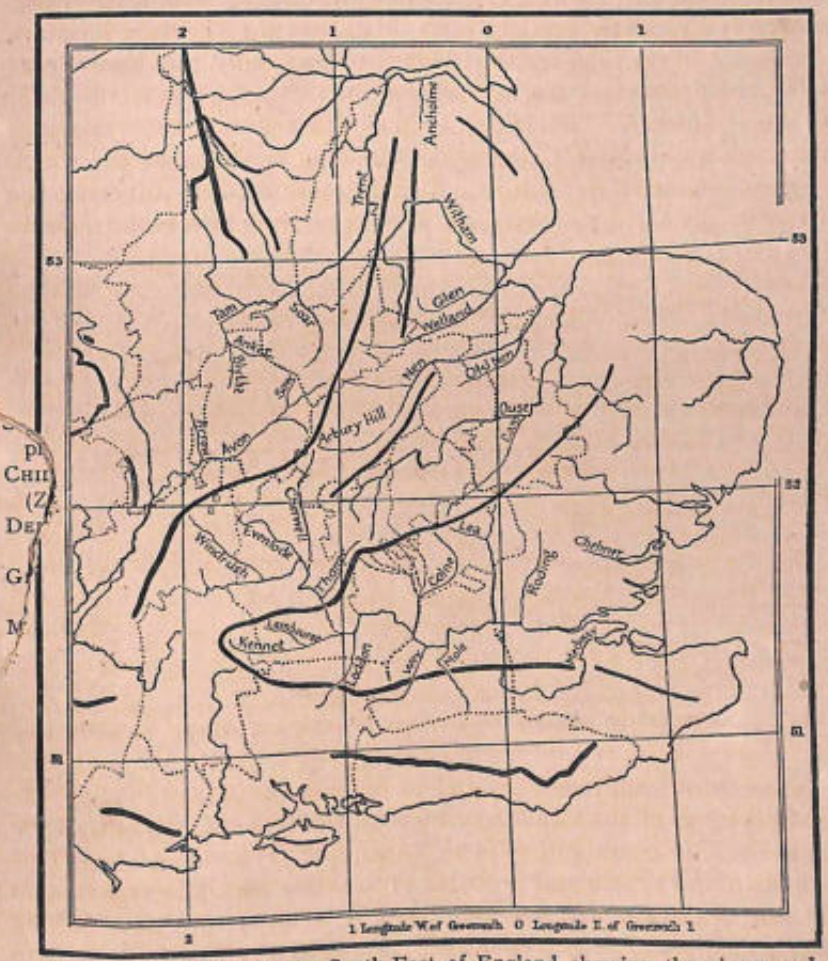


FIG. 3.—Sketch-map of the South-East of England, showing the rivers and principal escarpments (black lines); the dotted rivers are now dry.

central plateau which fixed the direction of the rivers of the south-eastern quarter of England is that it is Post-Eocene; how much later, one cannot be sure. The evidence quoted from the Berkshir area shows that the erosion of the main valley between the Chalk and the Oolites had commenced before the period of the later set of Sarsen stones, which are probably of Upper Bagshot age. So that one can only conclude that the main outlines of the river systems in

question were laid down in the long recordless period which followed the close of the Eocene, when England for the last time "arose from out the azure main." As no deposits of any value as a time scale have been left in this district for this period, it is unfortunately only too possible that we may never be able to arrive at a more exact determination of the date of the Thames and its northern tributaries.

PRINCIPAL REFERENCES.

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2. **J. Prestwich.**—"On the Relation of the Westleton Beds or Pebbly Sands of Suffolk to those of Norfolk and on their extension Inland; with some observations on the period of the final elevation and denudation of the Weald and of the Thames Valley, etc." *Quart. Journ. Geol. Soc.*, vol. xlvi., 1890, pp. 84–181, pls. vii., viii.

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9. **T. McK. Hughes.**—"On the Two Plains of Hertfordshire and their Gravels." *Quart. Journ. Geol. Soc.*, vol. xxiv., 1868, pp. 283–287.

J. WALTER GREGORY.

II.

Some Account of the Gall-Making Insects of Australia.

THE very interesting paper on the formation of vegetal galls and their inhabitants in the November number of NATURAL SCIENCE has induced me to offer some notes of my observations on the galls produced in this country, and the conclusions I have arrived at as to their origin after close study extending over some years.

The researches of many of our entomologists have shown that the insect fauna of most countries is regulated by, and adapts itself to, its surroundings in a most remarkable manner. Though, in so [illegible] a mass of land as Australia, we have a great range of climate [illegible] a diversified vegetation, the bulk of this great island is covered [illegible] stunted, scrubby forest trees, broken here and there along the [illegible]astal mountains and alluvial flats by thick forest lands, and in the [illegible]rth-eastern parts by dense jungles and tropical bushes.

One can safely say that the trees prevailing over nine-tenths of Australia are "gum trees," Myrtaceous trees chiefly belonging to the genus *Eucalyptus*, and smaller shrubby trees belonging to the order Leguminosæ, genus *Acacia*, commonly known under the names of wattles, myal, mulga, etc.

In the genus *Eucalyptus* some 140 species are described, of which the extreme forms are the small, bamboo-like, slender-stemmed mallee-scrub gums, growing up in clumps from a common base, with thin, long, slender roots just below the surface of the dry, sandy country they cover in many of the inland districts, and the giant gums of Tasmania and South Gippsland, whose roots strike deep down into rich alluvial soil, and whose great rounded trunks shoot up 400 feet in height.

The Australian gum trees may be considered as taking the position in our forests that the oak trees occupy in those of England and Europe. As, therefore, on the oaks of the Old World the larger proportion of the most interesting galls are found, so in this country the bulk of ours are produced upon *Eucalypti*. On the western plains and uplands, but also in a less degree in the denser scrubs clothing the coastal ranges, bush fires that devastate miles and miles

of country are of annual occurrence. These, for the most part, do not destroy, but simply char or scorch the foliage of the larger trees, promoting a vigorous growth of young suckers shooting up from the base of the trunks or roots, the tender foliage of which forms a dainty lodgment for hundreds of insects, and is especially suitable for gall producers.

Before proceeding with a brief account of some of our most characteristic galls, we will take a glance at their food-plants, as certain groups seem to have a decided preference for allied species. The Cynipidæ are found upon Acacias and Eucalypts; the galls of the Psyllidæ upon the Eucalypts; the Cecidomyia (gall gnats) attack *Eucalyptus*, *Melaleuca*, *Leptospermum*, *Omalanthus*, *Acacia*, and *Frenela*; while though the greater number of our gall-making coccids confine themselves to *Eucalyptus*, some of the smaller groups also attack *Casuarina*, *Leptospermum*, and *Melaleuca*.

Both the form and the structure of the dipterous galls, all of which belong to the family Cecidomyia, are very variable. One of the commonest, *Cecidomyia Frauenfeldi*, Schurer, about the size of a small filbert, is composed of a number of papery scales overlapping each other like the petals of a rosebud, while the larva lies curled up at the bottom close to the point of attachment to the twig. Very different are the round, hard, shot-like galls of *Horotom Omalanthi*, Skuse, often covering the leaves with their rounded red warts. The structures of *Diplosis frenelæ*, Skuse, are very remarka Sci being subglobular, fruit-like galls, formed of three thin valves grow ngs upon the slender branchlets of the desert pine (*Frenela Endlicheri*).

Those that infest Acacias produce curious tubular gall *Cecidomyia acaciæ-longifolia*, Skuse, aborts the young seed pods, alterin it them into irregular masses of tubular cells, each containing a larva, and occurring in such numbers that every seed pod on one of these trees is thus transformed.

Hardly anything is known about the Cynipidæ belonging to this country, but I have described three species (placed provisionally in the genus *Cynips*) all of which are found upon the Acacias, though I know several *Cynips* galls upon the Eucalypts.

Cynips acaciæ-longifolia attacks the flower-buds, aborting them into large rounded soft masses which, when mature, are handsomely tinted with red and yellow, from which they obtain the not inappropriate name of "Acacia apples"; *C. Maideni* produces galls upon the branchlets and twigs of the same Acacia, often altering them into swollen gouty excrescences six or seven times their natural size; *C. acaciæ-discoloris* deposits its eggs in the leaf-buds, which change into oval hollow galls ornamented with three short prongs or horns at the apex.

In the Homoptera, the family Psyllidæ contribute a number of rounded woody galls upon the leaves of the Eucalypts. In one species the larvæ form soft fleshy galls of a brilliant red tint, so

plentiful, sometimes, that the trees appear at a distance to be covered with crimson berries; another simply punctures the leaf and lives in the depression formed round it. This often swells out into an elongate, rounded, bubble-like gall still growing with the leaf after the Psylla has emerged.

I now come to the most remarkable group of our gall insects, namely, the gall-making coccids, belonging to the sub-family Brachyscelinæ. In the typical genus *Brachyscelis*, both the male and female coccids produce some of the most singular and fantastic growths ever found upon a tree. The members of this genus, if not all the Brachyscelids, confine their attentions to the Eucalypts. The male galls are generally small tubular growths from one to three lines in length, very slender, with a dilated rim or bell mouth at the apex; in some species they are very rare or unknown, while in others they cover the leaves in thousands. In *Brachyscelis munita*, Schrader, the male galls grow together, forming a curling, twisted mass of slender truncated tubes without an enlarged apex. I have received specimens of these from country correspondents under the name of "vegetable coral."

In *B. pharatrata*, Schrader, and in several allied species, the male galls are produced from the side of the fully-developed female gall, forming a flattened mass of coalescent tubes enveloped in a smooth [illegible]ay covering, often of a brilliant red tint, ten or twenty times the [illegible] of the female gall from which it springs.

The larvæ are pale, yellow shield-shaped little creatures, with short stout antennæ surmounted by long bristles of irregular length; [illegible]e segments are distinct and the outer margin of the whole insect is [illegible]inged with short feathery cilia, truncated at the apex, while there are two long hairs or filaments at the anal tip.

The male coccid is a delicate, pale yellow or bright pink, two-winged little insect, with long plumose antennæ and slender hirsute legs. The abdomen is very long and slender, and wonderfully adapted for reaching down to and impregnating the virgin females imprisoned in their woody coverings.

The female galls are produced upon the twigs or branches (exceptionally upon the leaves, as in *B. pharatrata*); they are round, conical, cylindrical, sessile, or stalked, some quite flat on the sides, others angular with apical extremities, produced into long straight or curved horns often several inches in length. *B. duplex*, Schrader, is four-sided, three inches long, an inch in diameter, with two sides prolonged into leaf-like horns from six to nine inches in length, gradually tapering to a point; the apical orifice in this gall is key-hole shaped, and lies between the bases of the horns. *B. munita*, Schrader, is smaller and much more variable; the apical orifice is a minute circular hole with a long, slender, straight or curled horn shooting out from each of its four angles. *B. ovicola*, Schrader, is always a symmetrical oval over an inch in length, while *B. pomaformis*, mihi, known in Queensland as

of country are of annual occurrence. These, for the most part, do not destroy, but simply char or scorch the foliage of the larger trees, promoting a vigorous growth of young suckers shooting up from the base of the trunks or roots, the tender foliage of which forms a dainty lodgment for hundreds of insects, and is especially suitable for gall producers.

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the "bloodwood apple," has a height of two inches, and a diameter of three.

There is a very curious double growth in *B. pileata* Schrader, and *B. variabilis*, mihi, unknown in any other species we are acquainted with. In the former, a long fleshy spike shoots out, under which the female gall is formed. As it increases in size the spike cracks round at the base, turning into an inverted cup-like covering protecting the young gall; this becomes brown and dry, falling off as the gall beneath reaches maturity. In *B. variabilis* the gall, which is very globose and woody, has a hollow dome-shaped false cell growing around and above the true solid gall. In the latter there is a very small round apical orifice, while in the dome above there is a ragged irregular aperture. Both these species appear to present a case where the outer bark of the stem throws off another gall on its own account.

The female coccids are top-shaped, round at the head, and sharply attenuated towards the anal tip or "tail"; they are yellow or semi-transparent "grubs" with no visible mouth or eyes when mature; short, rudimentary, three-jointed antennæ are situated on a small knob (evidently the head segment) between the fore legs; the middle and hind legs are slightly larger, and all terminate in a simple curved claw; the abdominal segments are distinct, covered lightly with fine hairs, and on the dorsal side carrying irregular rows of sh[illegible]r for sharp spines (which form very good guides for determining spe[illegible] differences). From the tip of the anal segment stand out the [illegible]. Geol. South appendages, consisting of two black horny processes lying c[illegible] together, but tapering and generally cleft at the tip.

The mature coccid is simply a bag of semi-transparent jell[illegible] which changes into a thin transparent fluid, in which the whitey-opaque eggs can be seen, through the skin, floating about; they are extruded in long strings, each egg sac bursting a few seconds after its emergence, when the new-born larva, after a few kicks and struggles, shakes itself clear and crawls down under its mother, remaining inside the gall and often filling the cavity between the wall of the gall and her. After some days they crawl through the apical orifice and escape on to the tree.

If all the thousands of larvæ that escaped from a single gall were able to make good their footing upon the tree, that would succumb speedily to their united attacks; but besides the numbers that never succeed in producing a gall, many that do are destroyed at a very early stage by the attacks of hymenopterous and other parasitic insects.

There are two other genera belonging to this group, *Ascelis* and *Opisthoscelis*. In the former the adult female has no signs of legs or head, but is simply a mass of yellow jelly armed with a remarkable three-fingered anal appendage, which plugs up the apical opening in the flat blister-like or globular gall. The males of *Ascelis* do not produce galls, but remain in the parent gall until full grown.

In *Opisthoscelis* the adult female has the segmental divisions well defined, and though both the fore and middle legs are wanting there is a well-developed hind pair, in which the tarsal claw is produced into a long thread-like joint sometimes much longer than the whole coccid.

Another remarkable genus, *Cylindrococcus*, forms scaly fruit-like galls (which are frequently mistaken for seeds or cones) upon the "she-oaks" (*Casuarina*), while the members of the genera *Sphærococcus* and *Frenchia* form several curious galls very distinct from each other. It is doubtful if the last three genera can be included in the sub-family Brachyscelinæ, but they are mentioned here on account of the gall-making habits of most of their species.

From their minute size it is difficult to observe the larvæ of many of these coccids when upon the plants, but taking one as a type, *Ascelis præmollis*, Schrader, a species rather common about Sydney, found upon the "bloodwood" (*Eucalyptus corymbosa*), we may illustrate how the larvæ of Brachyscelinæ attack the foliage. Upon leaving the mother gall they scatter all over the leaves, but only those fortunate enough to come across the younger growth towards the tips of the branches gain a foothold. They attach themselves to the epidermis, simply sinking into the leaf, the larvæ disappearing in a very short Mon. leaving the apical orifice, like a pin prick, and a minute blister Norfc only indications of their presence.
Oxfor
Rutl n the case of a parent insect (as in the Cynipidæ) forming an Shrc ion in a growing woody tissue, it is quite possible that she might Som ct some acid or irritant secretion that would have a good deal to Suf with the ultimate form of the gall; but where the larva itself Sta ectly attacks the plant it is quite evident that there can be no Su ritant matter of any consequence introduced with it, and the final shape of the woody excrescence must be altogether influenced by the mode of feeding or working of the insignificant-looking little coccid.

It is, therefore, very remarkable that a number of white grub-like coccids, with aborted appendages, differing so little from each other in specific characters, should each have the power of forming a distinct and well-defined form of gall, which never varies in size or shape unless attacked by either outward or interior inquilines. The question then arises, What first causes a gall to be oval in one species, and in another species to put forth branching horns?

Their growth is very rapid, and the form of the gall is determined and apparent at a very early stage of formation on the tree. It is quite evident, in the genus *Brachyscelis*, at any rate, that the after-shape of the gall must be anticipated, because the inner skin, or wall of the gall, in contact with the female coccid, is smooth, hard, and sapless some time before its full development. Where the larva is a free-moving creature, it can no doubt materially direct the accumulation of vegetable matter either at the base, sides, or apex of the excrescence by feeding upon or neglecting one portion more than other, and

through an inherited instinct, each insect of a species forms a gall the exact counterpart of the other. This would be marvellous enough, but where the enclosed insect can apparently have no connection with her cell for a considerable portion of the latter part of her existence, it is even more astonishing, and the fact that the form of the galls is influenced at an early stage is borne out by my observations that many attacked by coleopterous and hymenopterous inquilines,[1] which have destroyed the coccid, still grow and increase in the uniform typical shape distinctive of its species.

The excessive flow of sap caused by the intrusion of the larva into the tissue of the leaf or twig would naturally cause an after-growth where the sap-circulation was altered, and naturally would form an excrescence of aborted, diverted, woody fibre, but this cannot account for a mass of galls with long curled horns or other decided characteristics being evolved from such a simple cause. The final shape of the gall must be directed by this tiny organism in some wonderful manner as yet unknown to us.

REFERENCES.

[The full descriptions and figures of the insects and galls referred to by Mr. Froggatt in this article will be found in the following paper by him.—ED.]

Notes on the family Brachyscelidæ, with some account of their parasites, and descriptions of new species. Part i. *Proc. Linn. Soc. N.S.W.*, ser. 2, vol. Hütt. pp. 353–372, pls. vi., vii., 1893.

Part ii. *Ibidem*, vol. viii., pp. 209–214, pl. viii., 1894.

Part iii. *Ibidem*, vol. viii., pp. 335–348, pls. xvi., xvii., 1894.

WALTER W. FROGGATT

Technological Museum, Sydney, N.S.W.

[1] Inquiline: an animal that lives in an abode properly belonging to another, either at the expense of the latter, as in the present case, or merely as a co-tenant.—ED.]

III.

Books of Reference in the Natural Sciences.

OF all the services that can be rendered to the systematic naturalist, the compilation of books of reference is the most useful and perhaps the most thankless. The general value of these books as time-savers is so great, that it is wise in a journal that endeavours to assist scientific as well as general readers to devote a few pages to a list of those works in the various departments of Natural Science which are found by experience to be of service.

To anyone studying a particular group of animals or plants, the loss of time, to say nothing of temper, in hunting up references and getting special information is often considerable, and it must be a satisfaction to know that there is in existence a book in which we can find the greater part of the information for which we are seeking, easily by turning over a few of its pages. It may also, occasionally, create in the searcher feelings akin to gratitude. Such books should be perfect to be of service; a wrong quotation shakes one's faith in the compiler, and if such often occur, the worker is apt to put aside the book with a sigh. Despite the difficulty of attaining this perfection, there are instances of accurate reference books—*e.g.*, Bronn's "Nomenclator Palæontologicus"—laboriously passed for press, and compared item by item with the original sources, after the manuscript is put into type; for this is the only way to approach exactness. In other cases, the compiler has been known not even to have seen a proof of his work, which has been allowed to go forth into the world with all its errors uncorrected. There can be little use for such books, though they do occasionally put one upon the track of something previously overlooked.

In the following list, no distinction has been made between good and bad books of reference; they have been classified under subjects as far as possible, and notes have been added whenever it has been considered necessary. The list, though obviously incomplete, has been compiled with the help of those most qualified to speak from experience and is offered to those who, working mainly in one field, occasionally wish to stray into another.

GENERAL LITERATURE OF NATURAL SCIENCES.

British Museum.—Catalogue of Printed Books. This is now nearly complete for the following letters: A—P, U—Z. It is still going through the press, and will be completed in about ten years.

BRITISH MUSEUM.—Academies. An excerpt from *the* Catalogue, and containing all the publications of scientific societies entered under the name of town where published, contained in the British Museum up to 1885.

BRITISH MUSEUM.—Periodical Publications. An excerpt from *the* Catalogue, and containing all the serial publications, other than those of scientific societies, contained in the British Museum up to 1885.

ROYAL SOCIETY.—Catalogue of Scientific Papers. Up to 1883. This contains references to papers which appeared in scientific serials.

ENGLISH CATALOGUE OF BOOKS.—Yearly. Contains titles of all books published in Great Britain and the United States since 1835.

KAYSER.—Index Completissimus Librarum. 1834–1891. A list of all books published in Germany between those years.

LORENZ.—Catalogue général de la Librairie Français, 1840–1890. A list of all books published in France between those years.

BRUNET, J. C.—Manuel du Libraire. 8 vols. Paris, 1860–80. General.

TIELE, P. A.—Nederlandsche Bibliographie van Land- en Volkenkunde. Amsterdam, 1884.

ANDERSON, J. P.—The Book of British Topography. 1881.

LIDEN, J. H.—Catalogus Disputationum in Academiis et Gymnasiis Scandinaviæ et Finlandiæ. 1778–9. Continued by G. Marklin under similar title up to 1819. Upsala, 1820. [Most valuable for the older Scandinavian dissertations.]

DRYANDER, J.—Catalogus Bibliothecæ Historico-Naturalis Josephi Banks [Banks had a remarkable collection of early books on Natural History: this collection is in the British Museum (Bloomsbury), and is now incorporated in the general Catalogue.]

BÖHMER, G. R.—Bibliotheca Scriptorum Historiæ Naturalis. 1785. Valuable for early publications.

REUSS, J. D.—Repertorium commentationum a societatibus litterariis. Gottingen 1801. Gives papers on zoology, botany, and mineralogy published by academies as arranged under subjects.

NATURÆ NOVITATES (Friedländer & Sohn, Berlin).—This is a booksellers' fortnightly list, giving the titles of all publications on Natural History and the exact scientific as they appear.

HUTH & KLITTKE.—Societatum Litteræ. Yearly since 1887. List of current literature on General Science. Frankfurt a. O.

VALLÉE, L.—Bibliographie des Bibliographies. Paris, 1883.

LIST OF BIBLIOGRAPHICAL WORKS IN THE READING-ROOM OF THE BRITISH MUSEUM. British Museum, 1889.

SCUDDER, S. H.—Catalogue of Scientific Serials. 1885. Includes all publications of academies and societies.

BOLTON, H. C.—Catalogue of Scientific and Technical Periodicals, 1665–1882. 1885.

POOLE, W. F.—An Index to Periodical Literature, 1882; and supplementary volumes. All papers in magazines and similar periodical publications.

[HETHERINGTON, E.].—Index to the Periodical Literature of the World. Yearly since 1890. ("Review of Reviews" Office). Incomplete, but growing.

YEAR BOOK OF SCIENTIFIC SOCIETIES.—Yearly (Griffin & Co., London). Contains complete list of all British Societies with their officers, and details of publications issued during the year.

LEFEVRE-PONTALIS.—Bibliographie des Sociétés Savantes de la France. 1887. Published by the French Government.

MÜLLER, J.—Die wissenschaftlichen Vereine und Gesellschaften Deutschlands. 1883–1887. Comprehensive, but imperfect.

CATALOGUES of the libraries of the following institutions:—Royal, Linnean, Geological, Zoological, Museum of Practical Geology, Science and Art Department, South Kensington Museum, Royal Institution, Patent Office, Radcliffe, York Gate, and all such Catalogues, British and Foreign.

GENERAL ZOOLOGY.

THE ZOOLOGICAL RECORD.—Yearly since 1864. In the volume for 1893 will be

found a list of zoological serials with a note of the libraries where they can be seen.

ZOOLOGISCHER ANZEIGER.—A fortnightly serial containing original matter, but giving a list of new papers and books on zoological subjects as published. An index published in 1887, for the ten years then concluded, gives practically the whole literature of zoology during that time in one volume.

ANATOMISCHER ANZEIGER.—Similar to above, but dealing more especially with vertebrate physiology and anatomy.

ZOOLOGISCHER JAHRESBERICHT.—Yearly since 1879. Similar to the "Zoological Record," but deals with Morphology rather than Systematic Zoology.

ZOOLOGISCHES CENTRALBLATT.—Leipzig, 1894. In progress.

JOURNAL OF THE ROYAL MICROSCOPICAL SOCIETY.—This contains a series of short reviews on recent microscopical work in all branches.

THE NATURALIST, London, publishes classified annual Bibliographies of Natural History for the North of England.

ANNALS OF SCOTTISH NATURAL HISTORY, Edinburgh, contains Bibliographies of current literature on Zoology and Botany of Scotland.

WIEGMANN.—Archiv der Naturgeschichte. Yearly since 1835. Gives a record of zoological literature.

ENGELMANN, Bibliotheca Historico-Naturalis.—From earliest times until 1846; continued as Carus and Engelmann, Bibliotheca Zoologica, 1846–1860; continued as Taschenberg, Bibliotheca Zoologica, 1860–1880 (in progress). This contains a reference to biological literature, either separate or serial, exclusive of botany, and is by far the best and most convenient book ever published.

AGASSIZ, L.—Bibliographia Zoologiæ et Geologiæ. 4 vols. Ray Society. 1848–54. A general catalogue of all books, tracts, and memoirs on these subjects.

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ANNUAIRE GÉOLOGIQUE.—Yearly since 1885. In progress. This book gives not only a list of the papers on geology published during the year, but also a careful digest of the contents.

QUARTERLY JOURNAL OF THE GEOLOGICAL SOCIETY.—In November of each year is published "Additions to the Library." This gives not only separate books, but titles of papers in serial publications.

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C. DAVIES SHERBORN.

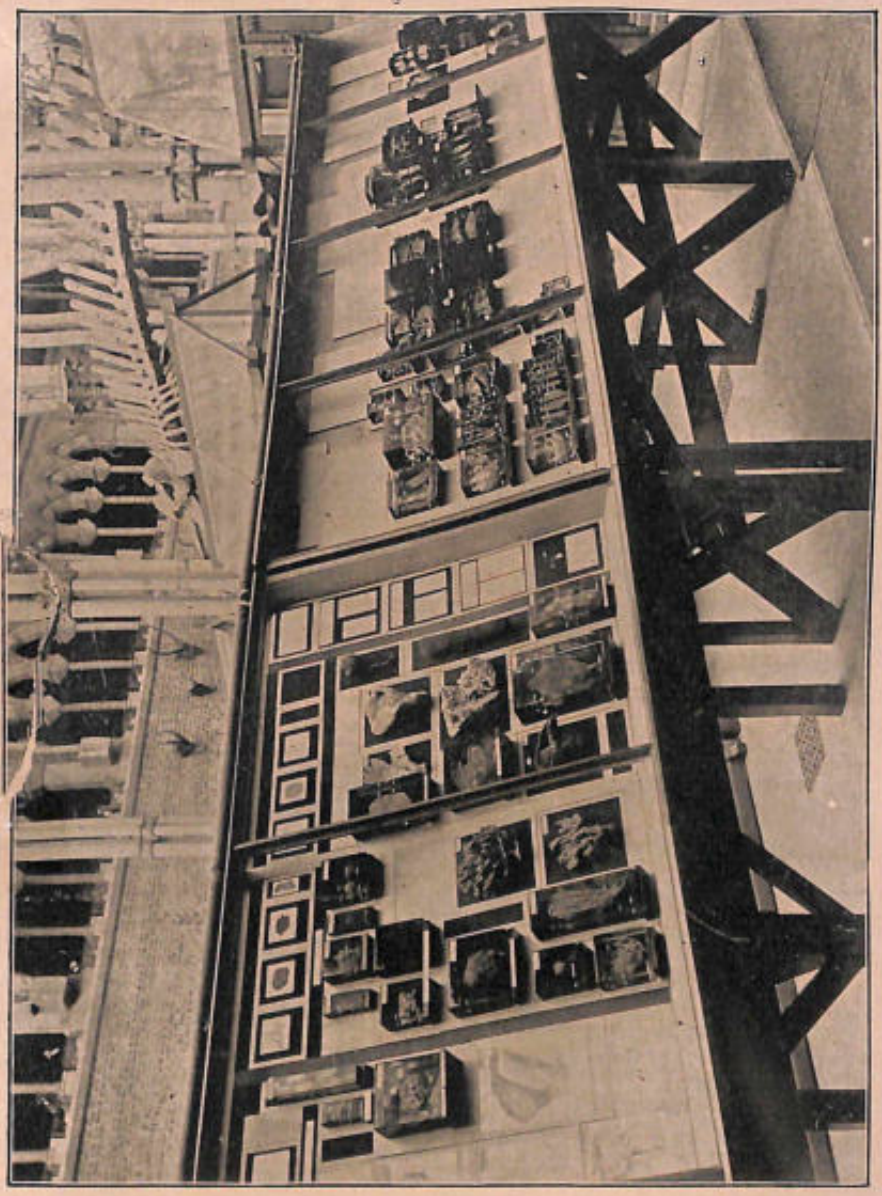
IV.

Some Reforms in the Oxford University Museum.

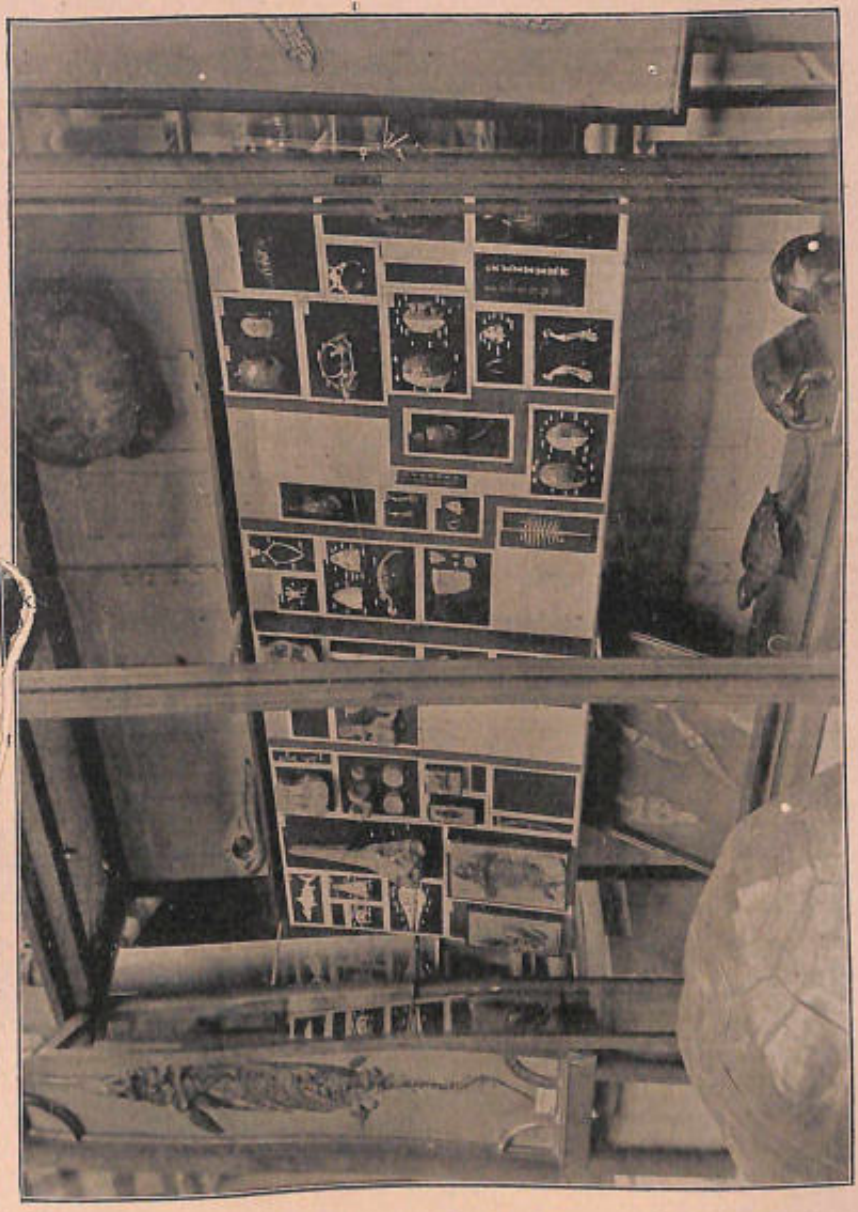
IT is now, I believe, some ten years since Sir William Flower inaugurated the reform of Museums on a truly scientific basis at the Natural History Museum; the cases in the Central Hall, gradually filling with excellent preparations, have been a valuable object lesson for all the scientific Museums in England. On somewhat similar lines, Professor Ray Lankester is reforming those exhibition cases containing zoological collections in the Oxford University Museum which are under his control.

So much has lately been said about the object and aim of Museums that I need only point out those particular conditions wherein this Museum differs from other similar public institutions as regards the kind of collections to be exhibited. Here one need seek neither to attract the nursery-maid nor to amuse children, nor again need one trouble to satisfy the idle curiosity of the sightseer. There is, then, no necessity for tragic groups of stuffed animals, for birds perched on cardboard rocks among artificial flowers. On the contrary, the exhibits are to be strictly scientific, forming series at once instructive and interesting to the general educated public, and more especially to the real student of zoology. Surrounded as it is by the various chemical, physical, and biological laboratories, the central court is in the first instance a place of study. In such educational collections it is essential that each object should be exhibited for a definite purpose, should show what it is meant to show as clearly as possible, and should be fully labelled in language technical so far as is necessary for accuracy. The observer is not to be bewildered by a number of specimens, but rather impressed by a few well-chosen examples.

For more than 30 years a large and valuable quantity of material (chiefly vertebrates) has been accumulating in this Museum, which, enriched with portions of the Ashmolean, Christ Church, Hope, and other collections, forms a considerable store of specimens to draw upon for exhibition. Hitherto the cases belonging to the Department of Comparative Anatomy had been used rather for storage than for display. The specimens were placed on shelves,



One of the Invertebrate cases; showing, on the left-hand, the arrangement of the Porifera.



One of the Reptile cases; containing *Ichthyosauria*, *Plesiosauria*, and *Chelonia*.

and taken out when needed in the laboratories or elsewhere. A comparatively small number were shown in table cases. Now, two collections only are kept: one, the larger, for educational use in the laboratory and for research, the other, and smaller, for public exhibition. It is with the latter that we have to deal in the present sketch, and with the system adopted for its display. The specimens are placed in their proper systematic order to illustrate not only their structure from a general morphological point of view, but also those important characters of systematic value whereby animals are classified—in other words, not only to show the anatomy of animals and to state that they *are* divided into classes, orders, and families, but also to show *how* and *why* they are so divided. Extinct forms are placed next to their living relatives,[1] and treated in precisely the same manner, thus breaking down that artificial barrier some people are apt to erect between fossil and living animals. When neither specimens nor casts can be obtained, semi-diagrammatic black-and-white figures are set up in their place. Similar drawings are shown of important objects too small to be visible to the naked eye.

Each object is placed on a rectangular tablet covered with black in the centre, with a half-inch margin of soft greenish blue colour all [illegible]nd. The specimens are provided with printed labels, and narrow [illegible] pointers," or lines; when in spirit, they are placed (labels and [illegible]ttached to a glass plate within square glass jars, blacked behind. [illegible]eover, each tablet has a small label at the top right-hand corner, [illegible]ing the scientific and popular name of the specimen, its date, [illegible]alogue number, locality, and source (by whom presented, or from [illegible]om obtained by purchase or exchange). As already stated, each [illegible]ecimen is placed in its systematic position, and the families or larger groups are marked off by dark red broad lines of separation. The whole presents a *tout ensemble* pleasing to the eye, yet not distracting the attention from what should, of course, be its chief object —the specimens themselves.

It is evident that the arrangement of the whole animal kingdom in this fashion is a task of no small magnitude, which will require some years to complete. Fortunately, in some instances, the help of specialists has been secured, as, for instance, of Dr. W. B. Benham for the Chætopods, Mr. E. A. Minchin for the Sponges, and Mr. G. C. Bourne for the Corals.

A set of six show cases, about twenty-one feet long, is devoted to the Invertebrates. These cases are provided with sloping backs and glass fronts on either side about four feet high above the ordinary table level, giving in all some 900 square feet of exhibition space (see Plate I.). Of the Invertebrates, a considerable number of Polychætes and Molluscs has been set up; but the Sponges are the only group the arrangement of which has approached anything like

[1] Professor Green has kindly lent many excellent fossils for this purpose.

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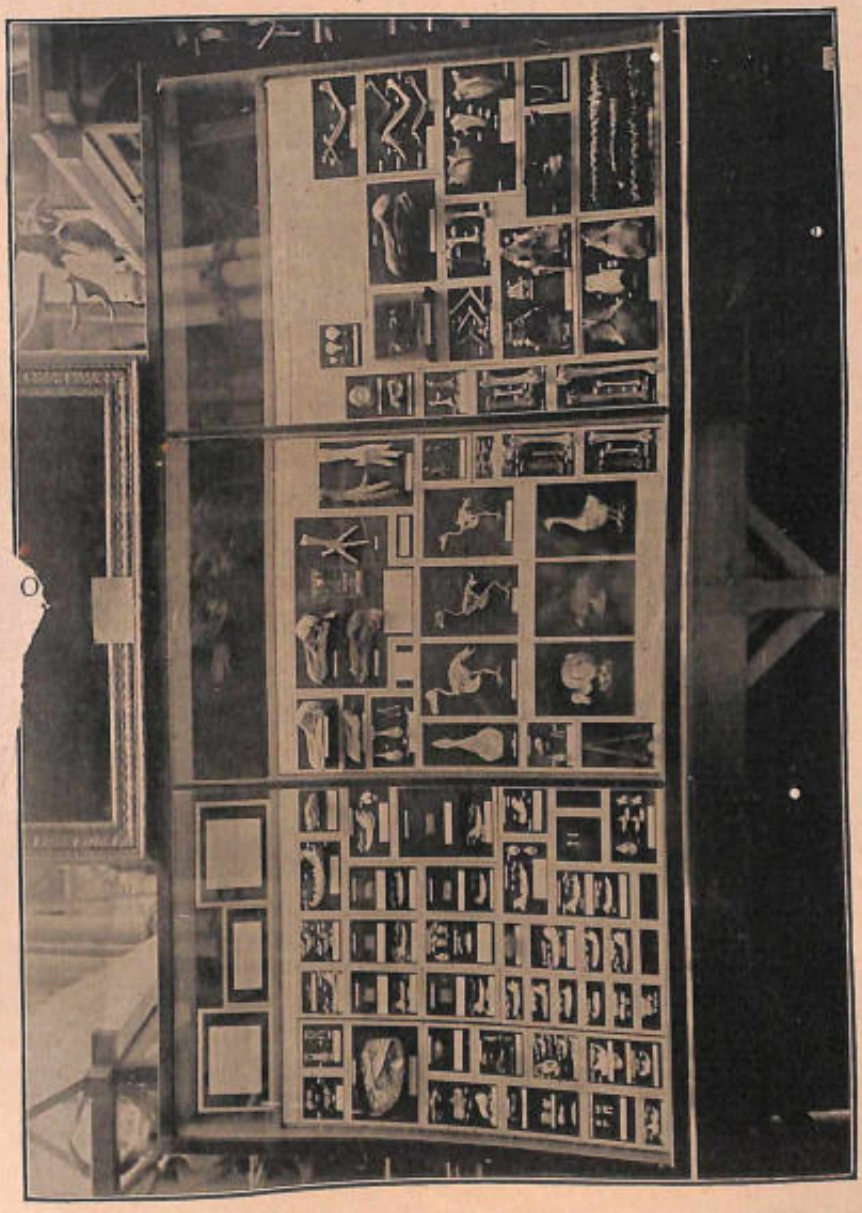
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completion. Mr. Minchin drew up the outline of the plan adopted, and selected the specimens. The Porifera, unfortunately, are a very unsatisfactory class to deal with in a Museum, their most important morphological and systematic characters being microscopical. This is, however, to some degree compensated by the very instructive and plainly visible modification of form assumed by individual sponges. A striking and very complete set of the modifications both of the Sponge Person and of the Sponge Colony has been prepared. Above these have been placed a series of coloured sketches drawn from life at Jersey, showing the brilliant colours of sponges which cannot be preserved. Then comes a series of fully described diagrams of the types of canal system, next to which are the specimens illustrating the classification of the group. (These do not appear in the photograph, being still in another case, where they were placed provisionally.) In this systematic part, short descriptions and figures of the characteristic spicules, etc., are placed at the head of each division.

Turning now to the Vertebrates. These are exhibited in the old high upright cases, within which sloping backs have been fitted. The Reptiles, the class to which most attention has been given, occupy one side of one of the main avenues, appropriately facing the famous *Megalosaurus* and *Cetiosaurus* remains. The tablet space devoted to this group alone is of about 240 square feet. Plate I. shows the case containing the Ichthyosauria, Plesiosauria, and of the Chelonia. Below are put objects too bulky to be set on tab while above will be placed the outline classification of each ord with large and carefully-executed diagrams of the skull. T descriptive labels have not yet been written.

Besides this general collection, a few things of exception interest or rarity have been set up in special cases. Such are a series illustrating Craniometry, and another showing the principal points of structural difference or resemblance between Man and the Anthropoid Apes. Lastly, there is a case (Plate III.) containing the Mesozoic Mammalia from the Stonesfield Slate, and the remains of the famous Oxford Dodo, the last Dodo seen living in Europe. This case, which was the first of the new ones completed, is arranged in great detail. In the portion devoted to the Mesozoic Mammalia, we have on the left the Allotheria (Multituberculata), among which we may notice the long-lost fragment of *Stereognathus* belonging to Professor Lankester, which was only recently recovered. On the right are the six jaws from the Stonesfield Slate, with an enlarged drawing, history, and description below each, together with figures of all the British and the most important American genera of Mesozoic mammals. Alongside are placed specimens of those living forms which resemble them most. The largest portion of the case is occupied by the Dodo, the Solitaire, and their living allies. Most of the parts of *Didus* and *Pezophaps* are compared bone for bone with those of pigeons, and their affinities are fully illustrated.



Case containing the Mesozoic Mammalia and the Dodo.

In view of the approaching visit of the British Association to Oxford, it may be useful to mention some of the specimens, besides those already treated of, which are of particular interest to zoologists. Such are, among the mammals: The brain of the chimpanzee "Sally," the jaws of the Ziphioid whales, *Mesoplodon bidens* and *M. layardi*, and a cast of the large extinct American Ungulate, *Dinoceras mirabile;* a Great Auk's egg figures among the stuffed birds; among the reptiles we may mention the Leathery Turtle (*Dermochelys coriacea*), the structure of whose carapace is shown in the reptile case described above; among the interesting fish are *Lepidosiren* and *Chlamydoselachus*, *Palæospondylus* and *Pleuracanthus* of the fossil forms. In the Geological Department there is an excellent collection (that of the late Dr. Grindrod) containing type-specimens of *Pteraspis* and *Cephalaspis*. Among the recent invertebrates we may notice a large series of Cephalopods, some of which are unique in England (*e.g.*, *Thysanoteuthis* and *Dorataspis*); a specimen of the sponge *Crateromorpha meyeri* containing the branching Polychæte, *Syllis ramosa*; a remarkable earthworm with two long penial appendages (of the genus *Siphonogaster*), and some very fine specimens of the coral *Heliopora*. Many dissections and other objects, though mounted, have not yet been set up in the new cases.

E. S. Goodrich.

Oxford Museum, July 14th.

V.

Hertwig's "Preformation or New Formation."[1]

Part I.

DR. OSCAR HERTWIG has worked so long at embryological subjects, and has produced results so valuable, that his opinion on this burning question of to-day is of unusual value. It is proposed in this paper to give an uncritical account of the views expressed by him in his most recent publication.

Biologists of the seventeenth and eighteenth centuries observed the process of embryological development as the growth of a miniature but visible embryo or bud into the adult form. They regarded it, therefore, as a process in which the minute particles of a formed organism became bigger by the absorption and transformation of food-substances. By analogical reasoning they carried these visible occurrences back to invisible beginnings, and came to hold the germ of an organism as a miniature image of the adult, invisible to us on account of the imperfection of our eyes, and by reason of the extreme minuteness and opacity of the germ. They had got beyond the idea of spontaneous generation, and had formulated a theory of the continuity of life in the phrases "omne vivum e vivo," and "omne vivum ex ovo." And so, in addition to the theory of "preformation," they adopted the theory of "evolution." The germ of an animal was a miniature of the animal, and within this, germ within germ, were enfolded the germs of all future descendants of the animal. The succession of the generations of animals was an "evolution," or unrolling of a series of germs arranged by the Creator at the beginning of the world.

In the last hundred years improvement of methods and instruments has carried the boundaries of the visible much further into matter. The embryos and buds which appeared then simple organic substance are now resolved into millions of cells, while these again are

[1] Zeit- und Streitfragen der Biologie. By Professor Dr. Oscar Hertwig. Pamphlet I. Präeformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen. Pp. 144, with 4 illustrations in the text. Gustav Fischer, 1894. Price 3 marks.

ordered aggregates of physical substances. Although in this minuter investigation no trace has been found of enfolding of germ within germ, yet as a logical possibility this theory could only fail when the actual limits of matter had been reached. It remains to-day as it was at origin, unfounded on observed fact, but in the limits of pure reason logically tenable.

Wolff's "Theoria Generationis" was the foundation of epigenetic views. He perceived that preformation and evolution formed a closed system where there was no room for the progress of research. He tried to bring back embryology from pure reason to the realms of observation, and held that the germs of animals and plants were structureless organic matter, which in the process of development by new formation or epigenesis became the organised adult. His theory found little favour in his own day, for he could offer no rounded interpretation of nature in which the mind of man could dwell. He had no explanation of that "vis essentialis," inherent energy, with which he endowed organic matter; on his system the growth of a new organism remained as much a miracle as he declared the theory of germ within germ to imply.

Since his time, succeeding generations have done something to place a real meaning within his inspiring but empty phrase, and in the slow progress of science step after step has been taken in tracing the forces and methods by which organic forms arise. But to-day, as formerly, the same question is answered in two ways. In the words of Roux: — "Is embryological development epigenesis or evolution? Is it a new formation of structure? or is it the becoming visible of structure previously invisible to us?"

Weismann is the great modern instance of those who respond for preformation and evolution. According to him, each cell or set of cells of the adult that vary independently have an actual representative in the germ. The germ is a microcosm in the strict sense of the word, and this microcosm is a miniature of the adult invisible to our eyes. In two respects, Weismann's germinal microcosm differs from that of the older evolutionists. They held it to be a simple and direct miniature of the adult. For us, separated from those times by a century of embryological work, the actual events of embryological development, the visible changes of cell-layers, and the metamorphoses of organs, make this view untenable. And so in Weismann's microcosm the elements representing structures in the adult are arranged quite differently from the adult arrangement. Secondly, the older evolutionists explained the chain of life by their theory of germ within germ. Weismann attributes to his microcosm the power of division. But in method and tendency this modern theory of preformation, like the older theory, stays rather than satisfies the desire for causal explanations, and is on the intellectual level of an "explanation" of the origin of the world by supernatural means.

For many years Dr. Oscar Hertwig has been studying both the

actual occurrences of embryology and their theoretical significance. Since he takes a more epigenetic view, he proposes to attempt to formulate his ideas. This he will do first in a criticism of Weismann's theory of preformation, and then in an exposition of his own epigenetic theory. An account of these two sections will appear in succeeding numbers of NATURAL SCIENCE.

P. CHALMERS MITCHELL.

SOME NEW BOOKS.

MARSUPIALS.

A HANDBOOK TO THE MARSUPIALIA AND MONOTREMATA (Allen's Naturalists' Library). By R. Lydekker, F.R.S. 8vo. Pp. xvi., 302, with 38 coloured plates. London: W. H. Allen & Co., 1894. Price 6s.

IN this volume the author gives a concise account of all the known species of the Marsupialia and Monotremata. Under each specific name we find (1) a list of synonyms, (2) a diagnosis of the species, (3) distribution, (4) an account of the habits and mode of life. Moreover, each family and genus is the subject of a short descriptive account, and the whole is prefaced by a general introduction. The design of the book, which is based avowedly upon Mr. O. Thomas's Catalogue of the Marsupialia and Monotremata in the British Museum, is, indeed, excellent, and renders reference to any point easy. There are both systematic and alphabetical indexes.

In the introduction the author remarks that "it is not to the credit of the present generation that the working zoologist has for the most part to rely for his knowledge of the habits of the greater number of Marsupials upon observations—admirable in their way—published many years ago"; but in spite of this deficiency most readers will be thankful for the very interesting accounts here brought together of the habits of the various species, and we may echo the hope of the author that the appearance of this book may stimulate those who have the opportunities to add to our knowledge in this direction.

The book is thoroughly up to date, and includes descriptions of the more recently discovered species. The most interesting of these is the curious Marsupial Mole (*Notoryctes typhlops*)[1], of which a complete description and a good figure are now rendered easily accessible to the general reader. The affinities of this remarkable animal have been the subject of some discussion, but it is now definitely proved to be a true marsupial, having, however, some striking points of resemblance with the Cape Golden Moles (*Chrysochloris*), which belong to the Insectivora. This similarity is particularly marked in the case of the teeth, and in this connection the author makes a statement that appears open to objection. He remarks that "the exact similarity between the molar teeth of the two is somewhat difficult to explain, although it may probably be accounted for by both having retained this primitive type of tooth from early ancestors." Now, it seems highly improbable, to say the least, that two animals belonging to two totally different groups, both highly specialised in relation to a peculiar mode of life, should have retained the primitive character of the teeth, which so readily undergo modification. It appears far more likely that in these cases, as in many others, the tritubercular teeth are by

[1] *See* NATURAL SCIENCE, vol. i., pp. 37 and 247.

no means primitive, but highly specialised in relation to a particular diet, and that the resemblances between the two animals in question are simply the result of convergence, due to similar environment and mode of life.

The last part of the volume is occupied by a brief account of the fossil forms of the two orders, concerning which Mr. Lydekker can speak with authority. It is to be hoped that a similar addition may be made to the other volumes; the divorce between descriptions of recent and fossil forms has lasted too long already.

Most of the plates were originally published in Jardine's Naturalists' Library, but have been recoloured for the present issue, while figures of some of the recently discovered species are added.

BRITISH PASSERINES.

A HANDBOOK TO THE BIRDS OF GREAT BRITAIN. Allen's Naturalists' Library. By R. Bowdler Sharpe, LL.D. Vol. i. 8vo. Pp. xix., 342, with coloured plates. London: W. H. Allen & Co., 1894. Price 6s.

THE additional interest which nowadays attaches to most natural history topics has naturally resulted in a rapid multiplication of zoological works. This in itself is desirable enough, since students always wish to keep their knowledge abreast of the times. The misfortune is that some of the works which are most loudly trumpeted by the public Press possess little real utility, being, in fact, merely popular compilations of the harvesting of genuine workers in the wide fields of Science. Happily, no such drawback attaches to the charming little volume on British Passerine Birds, designed by Dr. Sharpe to inaugurate the new "Naturalists' Library" which Messrs. Allen propose to launch under his very competent editorship. It is true that books on British birds are legion; nevertheless, there is always room for first-class work, and Dr. Sharpe's exceptional experience stamps his pages with that *imprimatur* of authority which we should look for in vain in many other popular works. Dr. Sharpe has the resources of the national collection upon which to base his conclusions. He is a good field naturalist himself, and possesses a knowledge of modern ornithological literature second to none. It is not, therefore, surprising that his new book has been carefully planned and executed with the highest skill. It is well printed, and misprints are hardly to be found. There are a few trifling slips in Dr. Sharpe's definitions of the ranges of certain birds, but none of these happen to be of any consequence. A more serious drawback is the somewhat inadequate treatment meted out to *nestling* birds. In this respect alone Dr. Sharpe is arbitrary and irregular. For example, he describes the nest-dress of the young Creeper (*Certhia*), but he says nothing about the first plumage of the true Flycatchers (*Musicapa*). The first plumage of the Thrushes (Turdidæ) is fully explained, but nothing at all is said about the first plumage of the Larks (Alaudidæ), which is quite as noteworthy. We should like to see the nest-dress of every species fully explained in the volumes which are to follow. When we turn to Dr. Sharpe's descriptions of the habits of birds, we find such frequent references to Mr. H. Seebohm that a stranger might suppose that this distinguished traveller was our only insular field naturalist. We do not wish to detract from the merits of Mr. Seebohm's labours, which we value very highly; but Mr. Seebohm's field notes have been served up again and again in his "History of British Birds," in his books on Siberia so-called, in the Ibis, in Dresser's "Birds of Europe," while popular com-

pilers have appropriated whole pages of Seebohm's writings in order to use them as padding in their own books. Dr. Sharpe would have done well to lighten his text with a brief extract from Warde Fowler's excellent *brochure* on the habits of the Marsh Warbler, or Gurney's graphic account of the Bearded Titmice that nest (in sadly diminished numbers) in the Norfolk Broads. A long series of excellent papers might have been drawn upon with advantage. After all, we must not be too exacting. It is something of a privilege that Dr. Sharpe should lay aside his important monographs in order to instruct our youth in such a limited subject as "British Birds," and we might seem ungrateful if we found serious fault with the execution of his task. It is only fair to say that Dr. Sharpe has taken great trouble to anticipate the requirements of beginners, as well as the points that advanced students are likely to raise. The characters of each genus are tersely and comprehensively described. The various problems of distribution are discussed and explained with judgment. Every variety of information seems to be afforded by one or other of the separate paragraphs into which Dr. Sharpe has wisely divided his text. The changes of plumage receive full and lucid treatment. The size and colouration of the egg-shells of British Birds are fully described, apparently from the National Collection, which Mr. Seebohm has recently arranged and classified. Altogether, we may say that Dr. Sharpe has been exceedingly successful in his first venture, which augurs well for the quality of the numerous volumes which are to follow. The most grudging critic would find it hard to withhold a high meed of praise from the letter-press. From beginning to end it is nothing less than admirable. Would that it were possible to speak in the same terms of the coloured plates. They are numerous—surprisingly so for the price of the book—and we hope that they may prove useful; but they are not up to the mark by any means. If the series arranged by Messrs. Allen is intended to have a large circulation, we incline to think that the publishers would do well to sanction a more liberal expenditure on illustrations.

American Eggs.

Smithsonian Institution. Comparative Oology of North American Birds. By R. W. Shufeldt. 8vo. Pp. 461–493. Washington, 1894.

Dr. Shufeldt is always interesting, and every one of his numerous papers possesses the value which independent and original views are sure to command. The Comparative Oology of North American birds is an attractive subject, far too comprehensive, in fact, to be satisfactorily dealt with in a pamphlet of only thirty octavo pages. The most prominent defect in Dr. Shufeldt's essay is that he has given his facts in so condensed a form, and in such intricate sentences, that it is often difficult to follow his arguments or even to comprehend the conclusion at which we are expected to arrive. The chief portion of the information here cited is borrowed direct from well-known sources, notably, from the writings of Messrs. Coues and Ridgway, which have long been accessible to everyone at home and abroad. Occasionally Dr. Shufeldt ventures upon statements which are certainly wide of the fact: as, for example, when he asserts that the American buzzards "never defend their eggs by direct attack." Indeed it would be easy to enumerate a number of points upon which Dr. Shufeldt's opinion should be modified by the experience of his American confrères. But there is much that is good

and accurate in his text; not that we can agree with his dictum that, "from a scientific standpoint, avian oology has accomplished much in the past," for the study of egg-shells has always appeared to us to be singularly barren of important results, however pleasurable it may be to its devotees. The biological laws, which Dr. Shufeldt formulates to explain variations in the colour of the shells of eggs, seem to us to be very unsatisfactory where they are not the exposition of self-evident truths. The fact of the matter is that Dr. Shufeldt has attempted to perform an impossibility, in compressing so large a subject into such narrow limits. That he has dealt with it at all is a matter for congratulation. Cursory and superficial as his review of American oology must be admitted to be, we feel grateful that he has endeavoured to generalise at all on this difficult theme. It is to be hoped that he may find leisure to develop his oological impressions; in which case we shall, no doubt, welcome a substantial addition to the literature of the subject at some future date.

H. A. MACPHERSON.

LINNÆUS'S "SYSTEM OF NATURE."

CAROLI LINNÆI SYSTEMA NATURÆ. Regnum Animale. Editio decima, 1758, cura Societatis Zoologicæ Germanicæ iterum edita. MDCCCXCIV. Lipsiæ sumptibus Giulielmi Engelmann, 1894. Pp. iv., 824. Price 10 marks.

As the Bible is to the theologian, so is the tenth edition of Linnæus's "Systema" to the systematic zoologist.

The foundation of all binomial nomenclature, the whole scientific naming of existing animals, dates from this tenth edition of Linnæus's "Systema." In the tenth edition for the first time, the immortal naturalist was consistent in giving two names, a genus-name and a species-name, to everything he described, and therefore the tenth edition represents the sum-total of the Linnæan labours as regards founding a definite and simplified system of labelling organic nature.

For many years, and chiefly at the instance of the Committee formed by the British Association in 1842, the twelfth edition of the "Systema" was taken as the base-line, but the tenth is slowly replacing the twelfth in this country,—it has long done so in America,—and this replacement smooths over not a few difficulties that exist from works published between the dates of the two editions (1758 and 1766).

The original edition is now so scarce as to command a high price, and the thanks of all students of zoology are due to the German Zoological Society, and to Mr. Engelmann, for the publication of this reprint, at a price which brings it within easy reach of anyone who takes the smallest interest in his subject. We specially recommend it to the notice of all museum curators and librarians.

SEX.

UEBER DAS VERHÄLTNISS DES MANNLICHEN UND WEIBLICHEN GESCHLECHTS IN DER NATUR. By Dr. Georg Klebs, Professor of Botany in Basel. Pp. 30. Jena: Gustaf Fischer, 1894. Price m. 0·80.

THIS little pamphlet does not call for much comment. It was delivered as the Rectorial Address in the University of Basel last autumn, and for publication, says the author, has been altered and enlarged in a few points. It forms a readable and quite uncontroversial account of the general phenomena of sex in the animal and vegetable kingdoms. The language is simple, and—rare occurrence

in modern scientific pamphlets—the references to all the writers mentioned are pleasant and appreciative. Starting with the discovery, in August, 1677, of spermatozoa by Leeuwenhoek's pupil, Hamm, the writer traces the gradual advances in knowledge which have led to our modern conception of the sexual cells in animals and plants. He shows the general parallelism existing between animals and plants in respect both of the sexual cells and of the male and female organisms containing them. He shows how, in both, the male cells become adapted to reach the usually larger and more passive female cells, and how the sexual characters of the male and female organisms are adapted in association with these diverging capacities. He touches lightly upon the determination of sex, the influence of crossing, and of in-and-in breeding and kindred topics. We recommend this book readily—not to biological specialists, who would find in it nothing very new, but to anyone who wishes an agreeable and instructive hour's reading.

Biological Homilies.

Biological Lectures and Addresses delivered by the late Arthur Milnes Marshall. Edited by C. F. Marshall. 8vo. Pp. viii. and 363. London: David Nutt, 1894. Price 6s.

"Homilies," says the Judicious Hooker, "are plain and popular instructions." Justly, then, may we apply this term to the essays comprised in the present volume. The exposition of the advanced teachings of modern biology in language that shall be lucid yet not misleading, intelligible yet not inaccurate, increases in difficulty with the increase and extension of knowledge itself. Along whatever path of zoological specialisation we proceed, we are speedily confronted by a bristling array of technical terms, erected as necessary waymarks for the pioneers themselves, but presenting a well-nigh impenetrable barrier to their merely curious followers, while they are also unintelligible to their fellow workers on other paths. We, who month by month endeavour to lay the results of special research in an intelligible form before our readers, well understand the difficulties of such a task, and readily appreciate how successfully they have been overcome by the enthusiastic biologist whose loss we still so deeply mourn.

Most of the lectures contained in this book are reprinted from the *Transactions of the Manchester Microscopical Society* or from other publications. Five, however, are now printed for the first time from such manuscripts as were left in a fit state. These five were mostly delivered thirteen years ago, and include "The Modern Study of Zoology," "The Influence of Environment on the Structure and Habits of Animals," "Embryology as an Aid to Anatomy," "The Theory of Change of Function," and "Butterflies." The remainder deal chiefly with Cell-division, the theories of Weismann, experimental Embryology, and above all with the Recapitulation Theory, which indeed is a regular King Charles's head.

The merit of these addresses is to be found rather in their clear and vivid style than in any great originality of idea. Perhaps the earlier pages have occasionally too strong a platform flavour, which the author himself would probably have edited out. In other respects the volume contains admirable models for the populariser of biology, refreshing reading for the amateur scientist, and reliable synopses for those whose special studies have not left them time to follow the intricacies of other branches than their own.

THE BRACHIOPODIST'S VADE-MECUM.

AN INTRODUCTION TO THE STUDY OF THE BRACHIOPODA, intended as a Handbook for the use of Students. By James Hall, assisted by John M. Clarke. The First Part, issued in *Report of the Regents of the N.Y. State Museum*, xlv., for 1891, pp. 450-616. Albany, dated 1892, but published in 1894.

IN our first volume (pp. 628–629) we noticed the important work by Messrs. Hall and Clarke, entitled "An Introduction to the Study of the Genera of Palæozoic Brachiopoda." The present handbook is practically an attempt to bring the facts and conclusions of that advanced treatise into a form more suited to the pecuniary and intellectual capabilities of the student. Hitherto English-speaking students have either had to struggle with the ponderous tomes of Davidson, issued by the Palæontographical Society, or, if they wished for some more concise, elementary, and, at the same time, up-to-date account, they have had to overcome the difficulties presented by a foreign language, and to rely on the admirable summaries by Von Zittel in German, and by Oehlert in French. Although, therefore, Professor Hall tells us that "the work has been prepared for the use of American students," we take upon ourselves to assure him that, so soon as it is issued in completed book form, it will win the gratitude of all those English-speaking zoologists and geologists who share with the authors an interest in the Brachiopoda.

The present part begins with a general account of the group, which follows in the main the lucid model furnished by Oehlert in Fischer's Manuel de Conchyliologie, differing therefrom by the inus's poration of the fresh knowledge acquired during the last seven yortal After a synopsis of the habits of the Brachiopoda and of nd a bathymetric and geographical distribution, the latter illustrated hth detailed map, there follows a full description of the shell with rds external and internal markings. A closer attention to the facts e. growth has recently led zoologists to distinguish between the variope methods in which the passage for the stalk or peduncle has become more or less closed by shelly matter or stereom in different genera o. Brachiopoda. Those who wish to learn the meanings of the terms *delthyrium*, *deltidium*, *deltidial plates*, *chilidium*, *listrium*, and *syrinx*, will here, and here only, find them fully explained. Next to this comes a clear and succinct account of the anatomy of the soft parts of the animal and of those internal skeletal structures connected with the so-called "arms" or lophophore that are of such importance in classification but so hard to investigate and comprehend. We find herein full use made of the recent researches of Joubin, Oehlert, Beecher, and others, of which accounts have from time to time been given in NATURAL SCIENCE, while, in the description of the development, ample notice is taken of the growth of the shell as well as of that of the soft parts, and it is recognised that developmental change does not cease at the close of embryonic life, but continues through the brephic, neanic, and succeeding stages.

With the way thus cleared for him, the student passes next to the study of all the genera, both recent and fossil, beginning, of course, with the Inarticulate Brachiopods, and then advancing through the Orthoid, Strophomenoid, and Productoid groups of the Articulata. The order that is followed in the descriptions of the genera is precisely the same as that of the larger "Introduction" to which we have already referred. The only differences appear to be the admission of Beecher's *Paterina*, which for some reason was excluded

from the previous work, and of Matthew's *Trematobolus*, which dates from 1893. This part of the work is illustrated by numerous woodcuts; at the same time it will undoubtedly be necessary for those who wish to gain a clear idea of the various genera, now so minutely divided, to refer to the excellent plates of the larger work, and better still to such specimens as they may be fortunate enough to find showing the details of deltidial, muscular, and brachidial structure, which specimens, we need hardly say, are rarely to be found even in our best appointed museums. Owing to the great difficulty of determining from ordinary specimens to which of these revised genera a species should be referred, we cannot help regretting that the authors have limited themselves under each genus to instancing merely the type species, which of course is not necessarily a typical or common species, instead of giving a list of the chief American and European species that they would refer to the genus in question. We may add, also, that considerable use of the larger work has brought to light a defect, which is naturally intensified in this smaller book, namely, a want of clearness and definiteness in pointing out the characters that separate one genus from another closely allied to it. It is, for instance, very difficult, in many cases, to know whether to refer a species to *Rafinesquina*, *Stropheodonta*, or *Orthothetes*. The diagnostic characters are no doubt given, but they are mixed up with other characters not strictly diagnostic. This defect might easily be remedied by giving a key, like that published a short time ago by Mr. C. Schuchert, and we trust that some such step will be taken in the second part of the present book.

As in their previous work, the authors do not divide their genera into families, a course which may be warranted by the insufficiency of our knowledge, but which hardly commends itself to the text-book compiler, the museum curator, or the examinee. We all admit that our classifications probably do not tally with nature; but that even a bad classification is better than none. The authors, however, promise to discuss the question of classification at the close of the work, and meanwhile we may refer to the very workable classifications by Schuchert (*American Geologist*, xi., p. 141, and xiii., p. 102). We are more inclined to quarrel with our authors for the arrangement that they have in some instances adopted. They need not commit themselves to a cast-iron classification, but surely they might follow an arrangement more in accordance with the facts of geological history and individual development. They appear to accept the main arguments of Beecher, but for some unexplained reason they do not admit the natural conclusions. Thus, they begin with the somewhat advanced Lingulidæ, then proceed through the Lingulellidæ and *Lingulasma* to the Trimerellidæ, from which they come back to the Obolidæ, and so to *Paterina*. Now if they accept Beecher's views on development, they should also accept his conclusion that *Paterina* is the most primitive form of Brachiopod known to us, and they should lead off with that genus. But perhaps this will be explained in their final chapter. Anyhow we know exactly what Messrs. Hall and Clarke themselves think about the Trimerellidæ, and we know that they derive this assemblage from two distinct stocks, one typified by *Lingulasma* and the other by *Obolus*, a view which may be correct, but which is at any rate incompatible with the peculiar arrangement they adopt. This, however, will not render their book of less practical use to the student, who will look forward with much anxiety to the second part, which we are promised in the Annual Report of the State Geologist for 1893, and which we may, therefore, expect to receive somewhere in 1895 or 1896.

When the work is complete it is sincerely to be hoped that it will be bound up together and placed on the open market.

F. A. B.

ANOTHER TEXT-BOOK OF ZOOLOGY.

LEHRBUCH DER ZOOLOGIE FUR STUDIRENDE UND LEHRER. By Dr. J. E. V. Boas, Lector der Zoologie an der Kgl. Landw. Hochscule Kopenhagen. Pp. 503, 427 illustrations. Second and enlarged edition. Jena: Gustaf Fischer, 1894. Price 10 marks.

IN the increasing multitude of foreign and English text-books, it is not always easy to see any but local pretext for the issue of the books. It is natural and right that where a teacher is honoured in the country in which he teaches, his students should have his written as well as his spoken word. This text-book is written specially for medical students, students of veterinary science, and so forth. Those of us who have taught or examined similar students in England will wonder with a great admiration at the range and thoroughness of the course of zoology mapped out for these in Copenhagen. In a general part of eighty-eight pages Dr. Boas gives an account of the animal body on the old Hunterian plan of division into tissues and systems. Then he treats rapidly but briefly the special problem of Morphology, the plans on which animal organisms are built, the embryological growth of organisms, and the relations of the diverging types of adult structure to each other. Next he treats of various biological problems, such as the influence of sessile life, the duration of life, adaptations, the relations of animals to their environment, and so forth. All this is as simply and as philosophically done as we have seen in any book, and Dr. Boas's students are to be congratulated on their teacher. The systematic part is equally well written. In invertebrates the most typical or the best known species only are referred to. In vertebrates the enumeration of species is carried out much more fully.

The illustrations are all good. There are many old friends and many desirable additions. Altogether we have nothing but praise for this text-book. While it is not conspicuously better than the other leading books of the same kind, it is thoroughly satisfactory for its purpose, and much better than any English text-book or translation.

A GEOLOGICAL TEXT-BOOK.

GEOLOGY. A Manual for Students in Advanced Classes and for General Readers. By Charles Bird, B.A. Lond., F.G.S. 8vo. Pp. 429. London: Longmans, 1894. Price 4s. 6d.

THE illustrations constitute the feature in which this book differs most markedly from most works of the kind. These fall into three groups. First, and most important, a new set processed from photographs, some from those by Messrs. Wilson, chiefly of coast scenery, mountains, rivers, and old volcanic tracts, which are admirable in selection and in method of reproduction; the rest, of rocks, minerals, and fossils in the Jermyn Street Museum, often of figured or type-specimens, among which special attention should be given to figs. 145, 214, 231, and 237; both of these sets of illustrations are excellent and will give more than a passing value to the pages they adorn. Secondly, there are illustrations, for the most part good ones, borrowed from earlier works, some with adequate acknowledgment, as in the case of those from the works of Green and Bauerman, others without, like figs. 170, 211, 239, 259, 261, 287, which appeared first in Woodward's "Geology of England and Wales." Thirdly, those which have been recovered from some forgotten talus heap, or

else specially drawn for the work, as we do not recollect having seen them before, and fervently desire never to see them again. For example, the drawing of the quartz crystals on page 13 is preposterous, of hornblende on page 30 useless, while the fancy glacier on page 115 has no excuse, when good photographs, which might really teach something, are so easily obtainable. Why should a ridiculous and imaginary creature be made to masquerade as a trilobite on page 194, when a very good figure of *Ogygia Buchii* is wasting away in the uncongenial air of the Cambrian a few pages further on? Impossible graptolites, undesirable plesiosaurs, and shells and sponges standing on their heads should not have been tolerated amongst the other illustrations, and more satisfactory photographs or drawings of some of the rock-specimens should have been secured. It is tantalising not to be told where a conformable sequence from Carboniferous to Keuper is to be found (p. 273), or where the Lias rests direct in the Permian conformably and unconformably within a short distance, while the Oolite is unconformable to the Lias (p. 284).

Leaving these and other errors in the illustrations and turning to the text, it is clear that this has suffered by being cast in the rigid mould of a South Kensington Syllabus, so that an attempt has to be made to put into the book far more than can be *taught*, though it may possibly be *crammed*, in a work of this size. What is the use of attempting to teach anything about systematic crystallography without at least a drawing to show the relations between crystal axes and faces, or to simply state, without any explanation, that a crystal has or has not an effect on polarised light, unless these misements are merely intended to be committed to memory? The the logy is worse than the mineralogy; after learning that mere of zo tegration will convert granite into "gravel," that phonolite is a b dic," syenite basic, felsite and pitchstone plutonic, we are not for ch surprised to hear that the last class of rocks is often amygbaloidal. Slips and misprints abound: Marlsto, Brokenhurst, u rodorite, *Hippurites ponderosum*, Flanborough, Coraline, kim-clay, n *nio* (p. 320), Landeilo, Geographical Survey, *equivalvus*, while some of the definitions, doubtless accurate, do not help very much in understanding the application of words, like asbestos, unquenchable, and eggstone for roestone.

The latter part of the book is rather better done, the physical part decidedly so, and among the good features are vertical sections of the systems, showing their characters, fossils, and economic products. There are disfigurements, however, here and there. We read of trilobites with only two segments, that there is a Rhætic fish still living, that the "characteristic ammonites occasionally show a tendency to become a little mixed," and that belemnites are the "internal shells of a kind of cuttlefish." It is a pity the author has not cleared things up for the student by adopting the tripartite division of the older Palæozoic rocks now everywhere adopted, and that he has omitted all reference to the Ordovician and Silurian rocks of the Lake country, while giving a careful description of the foreign equivalents. Scant space is devoted to modern theories and views in the origin of the foliated rocks, and the minute zoning of the Upper Cretaceous rocks has been omitted.

In conclusion, while the author has introduced some important and novel features into his book, he would do well to prevail on his publishers to make a bonfire of certain venerable illustrations and to let the proof sheets of a new edition pass under the eyes of those who are familiar by actual field work with their several branches of the

science, in order to eliminate errors and supply omissions in a subject too wide to be adequately covered by a single individual, however varied his attainments.

The Lower Oolites in England.

The Jurassic Rocks of Britain. Vol. iv. Lower Oolitic Rocks of England (Yorkshire excepted), by H. B. Woodward, F.G.S. 8vo. Pp. xiv., 628, with plates i., ii., and woodcuts. Memoirs of the Geological Survey of the United Kingdom. London: Printed for Her Majesty's Stationery Office, 1894. Price 10s.

This is the companion volume to the one reviewed in Natural Science, vol. iv., p. 69, and it is by the same author. Whether the strictures then passed on the "get-up" of the volume have led to improvement, we will not presume to say; at any rate, the present volume shows a pleasing contrast to the earlier one, though it is still much below the standard befitting such a publication. The volume deals with the Lower Oolitic rocks of England, Yorkshire being excepted. Such a division of the geographical extent of the Lower Oolite may be convenient; but it seems to give undue prominence to Yorkshire, and to throw an unfair burden on the author of the present treatise. It is an immense, and, if we may say so, too ambitious a task for one man to cover so much ground. Such a proceeding may indeed possess the advantage that if the author be a strict palæontologist his readers know that, by whatever names he alludes to species, the same form receives the same name in differ[illegible]s's districts. This is, of course, not the case with different observers [illegible]ortal hence much of the difficulty of geological correlation results. U[illegible]nd a tunately, even this advantage is thrown away in the present insta[illegible]nth The weakest part of the whole work is the palæontology, not becaus[illegible]rds any want of ability on the part of those responsible for it, but becau[illegible]. they cannot bring themselves to appreciate the needs of modern wor[illegible]e At the same time, the wisdom of expecting two or three men to grapp[illegible]e with all the present intricacies of palæontology, until separate mono[illegible] graphs placing the subject in available form have been prepared, may well be doubted. In consequence of this policy the result is, as the Director-General says in a somewhat apologetic preface, "A general memoir which is intended to present a broad but detailed picture." We have no fault to find with this intention; but the detail should be accurate.

The scope of the volume is from the zone of *Ammonites jurensis* to the Cornbrash. The zone of *A. jurensis* was treated in the companion volume of the Lias. It is here included to form, with the *A. opalinus*-zone, a division, "Midford Sand," which is parted between the Lias and the Inferior Oolite—the term Midford Sand being qualified by the words "Yeovil" or "Cotteswold," according to locality, a concession which will be understood by those acquainted with the strata and with recent literature. Besides the zone of *A. opalinus*, only three other zones are admitted in the Inferior Oolite. The labour of further subdivision has been shirked, under the plea that other zones are only local—a curious plea, when some of those not admitted were traced across Europe thirty and forty years ago.

The whole of what is called "Great Oolite Series"—from Fuller's Earth to the Cornbrash—is divided into two zones, of which the Cornbrash takes one, that of *Ammonites macrocephalus*. Something seems to require explanation here. *A. macrocephalus* has usually been

regarded as indicating the zone above the Cornbrash. The author puts it (in the Appendix) as a fossil which ranges above the Cornbrash; and this admission seems to call in question the wisdom of the division adopted—a division not accepted by Continental authors. At the same time, it excites a doubt whether *A. macrocephalus* belongs to the Cornbrash, or whether the Cornbrash may not be only a lithological series of different palæontological age in different districts. We are inclined to ask, "What is Cornbrash?", well aware that every geological student preparing for his first examination would think himself able to instruct us.

In the "Great Oolite Series" we are introduced to a new term, "Fullonian," an impossible word from *fullonius*, which we hope will not be adopted. Only one Ammonite zone comprises the "Fullonian," and the greater part of the Bathonian. This is an extraordinary inversion of the value of terms, and it shows where work is required in this country. It also forcibly illustrates the lithological tendencies of the author. He can appreciate and recognise subdivisions where they are based on lithological characters, because these are so striking and catch the eye so readily that it would be impossible for anyone to overlook them. When, however, it comes to the more difficult task of looking beneath the surface, and understanding divisions made upon palæontological grounds, the author shows himself unable to perform it. Although the volume does not profess "to enter into minute particulars [which] belong to the domain of the specialist in some restricted field of palæontological research," yet it does enter into such particulars where lithology is concerned. Thus, as lithologist the author is able to recognise eleven subdivisions of the Inferior Oolite; as palæontologist he can only see four. In our opinion, the above name marks the specialisation of the lithologist, and though we do not find fault with it on that account, for that is where its value lies in a scientific work, yet we take exception to the unequal treatment accorded to palæontology.

Perhaps the incorrectness of the palæontology is best exemplified in the woodcuts which illustrate the work. We confess to a difficulty in understanding why the woodcuts are given, because if the general geologist can identify his specimens therefrom he possesses skill denied to any specialist. Even if he do succeed he is no better off, for he will be wofully misled as to names. Why, when the original types were figured in British works, and when many of them are accessible in the British Museum, the woodcuts of what purport to be the same species should have been prepared from foreign figures, we are unable to understand. The result is certainly disastrous. Page 46 shows woodcuts of *Ammonites sowerbyi*, *A. murchisonæ*, and *A. humphriesianus*, all after d'Orbigny, whereas these fossils were named and figured by Sowerby. But d'Orbigny's *A. sowerbyi* disagrees in many respects with the type. His *A. murchisonæ* is not admitted to be that species at all, and his *A. humphriesianus* was named as a different species forty years ago by Oppel. Again, p. 48 shows a woodcut of *A. parkinsoni* after d'Orbigny, where a copy of Sowerby's type-figure might just as well have been given, and it would have saved the author the mistake of depicting as *A. parkinsoni* what has been separated by another name for some years. Worse, however, remains than this. Among the "Cornbrash fossils" shown in page 432, fig. 119 gives *Ammonites discus*, after d'Orbigny. This is another of Sowerby's species and the type is from the Cornbrash; but d'Orbigny figured as his fossil an Inferior Oolite shell. With unfortunate

temerity the author has taken upon himself to say that the Inferior Oolite species figured by d'Orbigny under a wrong identification is a Cornbrash fossil, and he has called it "*Ammonites discus*, Sowerby." A comparison of the suture lines shown by d'Orbigny and indicated by Sowerby ought to have put him on his guard. We fear, however, that such little details as suture lines do not accord with "individual views about species"—a phrase we find more than once in this work.

In the catalogue of fossils given in the appendix palæontological inaccuracy also reveals itself. It is not, however, by any means so conspicuous as was the case in the Liassic volume, and this, we take it, results from detailed work on Lower Oolitic species having been done by specialists in various recent publications. Thus the greatest and therefore most suspicious zonal ranges to be found in the tables are shown among the Lamellibranchiata, with which no British specialist has yet engaged himself, and we confidently expect that when one does these zonal ranges will be very much restricted. Outside the Lamellibranchiata very few species are given with wide zonal range—a marked contrast to the Liassic volume, while the majority are recorded from a single zone. A few exceptions occur, noticeably among the Brachiopoda. Thus *Rhynchonella concinna* and *R. obsoleta*, *Waldheimia anglica*, and *W. bullata* are given wide ranges. We venture to say that those who are making a study of Jurassic Brachiopoda—a by no means small band of naturalists in the West of England—will be unable to endorse these "finds" of the author. Nor will they wish to take from the Survey the credit of recording *Rhynchonella ringens* in the *Parkinsoni*-zone. We can understand some of the other zonal records by ascribing them to "individual years. about species," but we confess to being entirely at a loss to their what *Rhynchonella* of the *Parkinsoni*-zone can have been mistaken by a *R. ringens*, a species so remarkably distinctive in every way. its

The stratigraphical work is also not free from error. Thus, of the section of Leckhampton Hill (p. 124), a considerable series us sandy beds, which should be between the Gryphite and lower *Trigonia* grits, are not shown at all. On page 110 something seems to have gone amiss with the zonal arrangement; for, while in the early part of the volume (p. 45) "the zone of *Humphriesianus*" is placed in the "upper division," here it is put to overlap a portion of the "lower division." Then the upper *Trigonia* grit is left out of account for zonal purposes altogether. The recognition of the zone of *A. humphriesianus* at all in the Cotteswolds may be regarded as an error founded on misconceptions of Wright and Witchell, and in making a quotation to support it (p. 111) about there being "no break of a marked character between the upper *Trigonia*-grit and the Gryphite grit," the author has done a little special pleading: the context insisted on there being a chronological break.

Although we have thus pointed out some of the pitfalls which have entrapped him, we hasten to place on record our conviction that, considering the immensity of the task upon which he has been engaged, the author has produced a work of great value to all those who may be studying the Lower Oolitic rocks. As a record of what has been done among these strata, accompanied by many new and important contributions from the author himself, as a guide to the localities and the rocks which they exhibit, and as a starting point for all new work, the volume is one of great interest. Our thanks are due to Mr. H. B. Woodward for the amount of information which he has rendered so readily accessible to the student.

Fossil Plants.

Catalogue of the Mesozoic Plants in the Department of Geology, British Museum (Natural History). The Wealden Flora. Part I.—Thallophyta–Pteridophyta. By A. C. Seward, M.A., F.G.S. Pp. xl., 179, with 17 woodcuts in the text. Plates i.–xi. London, 1894. Printed by order of the Trustees.

This, like some other British Museum Catalogues, is not a mere list but a detailed description of the specimens, with numerous critical remarks on their systematic position, and a discussion of the present state of our knowledge of individual groups. In an introduction of some five-and-twenty pages, the author summarises the results of previous work on the Wealden flora, and gives an account of records of fossil plants from other countries, but of presumably the same or similar age. Some recent collections of Mr. P. Rufford from the neighbourhood of Hastings have supplied much of the material for the present volume, but reference is also made to the collections of Mantell, Dawson, and Beckles. Three genera and twelve new species are instituted.

While commending the thoroughness of the work, and the care with which facts previously known are considered in the light of fresh knowledge, we would at the same time draw attention to one or two points affecting mainly the principle of the book or its arrangement. Thus, while it is of the utmost importance to quote synonymy, it is well also to avoid such a waste of space as is entailed by the chronological arrangement which has been adopted. For instance, under *Lychselia Mantelli*, we have an account of the annual variation in the into ing, from 1825 to 1890, occupying nearly two pages (pp. 114–16), of o which one synonym, *Lonchopteris Mantelli*, occurs fourteen times. con ely it would have been better to lump all the references to tha s name in one paragraph. Similarly, on p. 27, under *Equisetites* ev *urchardti*, this name occurs a dozen times. And why are we made pr to seek in the obscurity of the text for references to the plates? wi The description of individual specimens is, we think, sometimes carried to an excess. For instance, under the species just mentioned, we find entries such as these:

V. 1070 and V. 1070a. A short piece of stem with three tubers, and another piece with four.

V. 1070b. Fragments of stems and several tubers. Two tubers shown in contact.

V. 2818 and V. 2819. Fragment of the same species.

As each item means a paragraph, no little space is thus occupied, whereas a bare quotation of the register numbers would have served the purpose.

A far more important point, and one affecting a great deal of recent palæontological work, is the foundation of species on material, the insufficiency of which the botanist shudders to contemplate. To give one instance: *Equisetites Yokoyamæ*, sp. nov., is founded on some tubers and fragments of stems, the tubers differing from a species previously described "in their smaller size and narrower elliptical form." It is well to note such differences, but to erect a new species without the support of other evidence, save locality, is to prejudice the undoubted value of the work as a whole. By the description of new species from often badly defined fragments of fronds, or other vegetative organs, specific names are reduced to marks of identification of individual specimens, and a comparison of facts in recent and fossil botany is rendered extremely laborious or practically impossible.

METEOROLOGY IN MARYLAND.

THE CLIMATOLOGY AND PHYSICAL FEATURES OF MARYLAND. First Biennial Report of the Maryland State Weather Service. Director, William Bullock Clark. 8vo. Pp. 140, 5 folding maps. Baltimore, 1893.

IN our February number (vol. iv., p. 146) we reviewed an Outline of the Geology and Physical Features of Maryland, which, it will be remembered, contained much information as to the climate of the State. In the present Report the various meteorological data are given in much more detail, with careful analyses of the weather in different parts of the State during the years 1892 and 1893, and with tables showing the daily rainfall at 27 stations. The agricultural and practical value of this portion is greatly enhanced by a detailed account of the state of the various crops as dependent on the changing weather during the period in question. The general interest of the work, however, lies in the relations which it establishes between the leading features of the climate and the topography, and therefore also the geology, of the State. There is naturally a great difference between the western part of the State, or Appalachian region, and the lower lying Piedmont Plateau and Coastal Plain (see our previous review). Thus the average annual temperature of the extreme western region is 50° Fahr., that of the eastern border 58°, while even more pronounced variations are seen when the comparison is made by seasons. Similarly the average yearly rainfall of western Maryland is about 38·5 inches, while that of the remainder of the State is nearly 44 inches. A more detailed comparison brings out still more interesting facts. The lie of the isotherms, for instance, both annual and seasonal, is direc on traceable to the configuration of the country. Variations of rai ver- too can, as a rule, be explained by similar reasons. "Thus red broad Frederick Valley, lying as it does on the eastern flank of ich Blue Ridge, receives an abundant rainfall from the moist sou easterly winds." At the same time there are many striking loc d variations in precipitation that have not yet received their explan e tion, and Professor Clark believes that they cannot be due f topographic differences.

Of the value of this work, which has only been in progress since May, 1891, we cannot speak too highly, and we are the more inclined to give good words since so much of the labour is voluntary. It is not merely to the Maryland farmers that these reports will prove of service: their educational value for schools, as showing in a comprehensible manner so many natural sequences of cause and effect, is not to be exaggerated; while we have no doubt that their usefulness to foreign meteorologists as records of fact will fully rival the services they will render to the State itself by attracting immigrants to a country so highly favoured of the clerk of the weather.

CONGRÈS GÉOLOGIQUE INTERNATIONAL. Compte Rendu de la 5me Session, Washington, 1891. 8vo. Pp. x., 530, maps and illustrations. Washington: Imprimerie du Gouvernement, 1893.

THIS ponderous volume of the proceedings of the fifth Geological Congress, held at Washington in 1891, reached England on the 23rd June. It contains reports of the various discussions on Pleistocene and other strata, the colouring of geological maps, the international bibliography of geology (which is in the press, and will be distributed by the author, M. de Margerie, as soon as ready), and a full report of

the excursions made by the Congress, dealing somewhat extensively with the geology of the western United States. The excursions, three in number, comprised the following: (*a*) The neighbourhood of Washington; (*b*) the Rocky Mountains; (*c*) Lake Superior, and the reports occupy some 330 pages. Maps, sections, and illustrations are given in quantity, as well as a sketch of the literature of each district, and the volume thus possesses the usual permanent value of these reports. It is a pity, however, that so long a time has lapsed before publication.

The Journal of Marine Zoology and Microscopy (May, 1894) continues to grow apace both in size and interest. This new number contains papers on "Sexual Colouring in the Cuckoo Wrasse," "The Significance of Dual Opercula in Serpulids," "Abnormal Muscular Bands in Salpa," as well as descriptions of the beautiful microscopical preparations sent out to the subscribers. The slides sent out this time contain the Phyllosome stage of *Scyllarus*, and four sections illustrative of the structure of the Anemones. That the Jersey Biological Station is progressing is only what we should expect, the situation being especially advantageous for the collection of marine objects. That it has many friends is evident by the ready response that has been made to a request for donations to the Library. We heartily wish Messrs. Sinel and Hornell the success they deserve.

Mr. G. S. Perrin sends us a reprint of a paper on Australian timbers, read before the Royal Victorian Institute of Architects. The species of *Eucalyptus* supply the greater number of the useful woods, of the commonest and best being the Blue Gum (*E. globulus*). The "ac[illegible]n or Macquarie Harbour Pine (*Dacrydium Franklinii*) is said to [illegible]e precedence of all Tasmanian timber for lasting qualities, beauty, [illegible]d easiness of working, while Western Australia has a large source [illegible] wealth in her immense *jawah* forests (*Eucalyptus marginata*). The Author lays stress on the great importance of a rational conservation of forests, suggesting that when the reckless waste in America has caused a serious shortening of the timber export from that country, Australia will be looked to to supply the deficiency.

The Jura of extra-European countries is receiving attention. The *Zeitschrift der Deutschen Geologischen Gesellschaft*, Bd. xlvi., Heft. 1, contains a paper by Dr. K. Futterer on the Jura of East Africa, with six plates chiefly devoted to the illustration of Ammonites. The fossils obtained from Mombassa, Tanga, etc., are considered to indicate beds of Callovian, Oxfordian, Kimeridgian and Tithonian age.

Mr. J. T. Cunningham has two interesting papers in the last number of the journal of the Marine Biological Association. The first, on "The Life History of the Pilchard," traces the development of this fish from the egg until nine days old, when the observations were stopped by the death of all the specimens. The second paper deals with the ovaries of fishes, and refers to those of the plaice, dab, flounder, sole, turbot, and brill.

From Mr. David Nutt we have received Schröter's "Coloured Vade-mecum to the Alpine Flora," fourth edition, price 7s. This is a handy guide to alpine flowers, and will be of considerable service

to tourists in enabling them to identify their specimens. The serious student will not, of course, forget his Gremli, so indispensable to minuter study.

MM. J. B. BAILLIÈRE ET FILS, 19 Rue Hautefeuille, Paris, write to tell us that they have nearly ready for publication a Bibliography of French Flora, containing the titles of nearly one thousand books and pamphlets dealing with the subject. These are classified according to geographical regions. The publishers will send it free to anyone writing for it.

THE *Nuovo Giornale Botanico Italiano* commences a new series with the present year. The original publication, which dates from 1869, was edited till 1872 by Beccari, but for the last two-and-twenty years has appeared under the direction of Professor T. Carnel. Henceforth it becomes the special organ of the Italian Botanical Society, and will contain those papers which are too large for publication in the Society's Bulletin. This is indicated by the addition to the title, "Nuova Serie," "Memorie della Società botanica italiana."

UNDER the title of "The Land of Viti," Mr. J. P. Thomson's paper on the Fiji Islands, read before the Adelaide Meeting of the Australian Association for the Advancement of Science (September, 1893), has been printed in the *Scottish Geographical Magazine* for March. Mr. Thomson deals with the various geographical divisions of the group, the coral reefs, geology, products, fauna, flora, natives and climate. The paper is long and interesting.

WE have received from Messrs. Williams and Norgate the issue their foreign book circular for June, 1894, being no. 59 of their Scientific Series. It contains a classified list of recent foreign scientific publications, with the net cash price for which they can supply each book. Purchasers of scientific books will find it very convenient.

THE July number of the *Scottish Geographical Magazine* contains articles and news of considerable interest and value. Dr. Otto Pettersson continues the account of recent Swedish hydrographical research in the Baltic and North Seas. In the last number of NATURAL SCIENCE, vol. v., pp. 5–7, we gave an account of the first part of Dr. Pettersson's review. This new part contains much detailed information, and is illustrated by many valuable charts of the degrees of salinity, the direction of currents, the contours of the sea-bottom, etc., of the areas investigated. Mr. E. Delmar Morgan writes of the mountain systems of Central Asia, and a useful coloured map is annexed. Mr. D. R. Urquhart continues his interesting notes upon the Bolivian *Altiplanicie*.

OBITUARY.

BRIAN HOUGHTON HODGSON.

BORN 1800. DIED MAY 23, 1894.

MORE than a third of a century has passed away and a new generation has arisen since the last contribution to Natural History from Brian Hodgson's pen was originally published, and it is more than half a century since he ceased to take an active part, as an official, in the work of the Indian Government. To appreciate the services that he rendered to Natural Science in the first half of the present century, it is necessary to recall the change, both in our knowledge of India and in our means of communication with it, that has taken place in the interval. One instance will suffice. At the time when Mr. Hodgson investigated the religions and languages, described the mammals and birds of Nepal, so little was known of the Himalayas that their peaks, which exceed by many thousands "acieet all other mountains on the earth, were supposed to be inferior in elevation to those of the Andes.

A few words will be sufficient for a sketch of Mr. Hodgson's career. He was the son of a banker, and was born at Macclesfield; he entered Haileybury in 1816, and landed in Calcutta to join the Indian Civil Service in 1818. In 1820 he was appointed secretary to the Resident at Katmandu, in Nepal; he became Resident in 1831, and occupied the post till 1843, when he was replaced by an even better known Indian official, afterwards Sir Henry Lawrence. Mr. Hodgson resigned the Indian service, and for a time returned to Europe, but in 1845 he took up his residence at Darjiling, a hill station that had only been occupied for about five years, and he remained there, partly engaged in studying the birds and mammals, but chiefly occupied with linguistic enquiries, until he finally left India in 1858. After his return to England he entirely relinquished the scientific pursuits on which his widespread reputation was founded.

There is no richer mammal and bird fauna in the world than that of the Eastern Himalayas, and if, when Mr. Hodgson arrived in Nepal, he entered on a zoological Eldorado, he made superb use of the opportunities he enjoyed. For many years the *Asiatic Researches* and the *Journal of the Asiatic Society* were filled with descriptions of new birds and new mammals from his fertile pen, and many of his communications appeared in the *Proceedings of the Zoological Society*

and in other periodicals. His papers, as a rule, were far from being mere descriptions of new forms; they contained numerous notes on anatomy, affinities, and habits. He published no large work, although for many years he proposed to bring out an account of the Nepalese bird and mammal fauna, with coloured illustrations. A large number of coloured drawings that had been prepared for this work by native artists under his direction were presented by Mr. Hodgson, together with the types described by him and the bulk of his collections, to the British Museum. In addition to these, he presented numerous specimens to the Museums of Calcutta, Paris, Leyden, Edinburgh, Dublin, etc. As a collector, indeed, he was at the time unrivalled. In the "List of the Specimens of Mammalia in the Collection of the British Museum," published in 1843, Mr. Hodgson's name is attached, in the "Index of Donations," to a larger number of references than any other donor's, and at this time only his first contribution to the national collection had been received. Subsequently, two separate catalogues of his presented collections were published, one in 1846, and the other in 1863.

A better idea of Mr. Hodgson's energy than any that can be derived from lists of specimens or even from a perusal of his papers is afforded by the drawings presented by him to the British Museum, or, still better, by the original copies that have found an appropriate resting-place in the Library of the Zoological Society of London. These drawings represent many hundreds of mammals and birds, and fill several large folio volumes, the same species being sometimes drawn three or four times. Each sheet, besides the figure of the whole animal, generally contains drawings of details of the external and internal structure, and the paper is crowded with manuscript notes on the localities, habits of life, breeding, nidification, and measurements. No better example of the care with which Mr. Hodgson collected facts could be cited than his paper "On Various Genera of Ruminants," published in 1847 (*J.A.S.B.*, xvi., p. 685). In some respects he was in advance of the science of the day. He was fully alive to the importance of geographical distribution, and was the first to attempt a demarcation of the zones of life, resulting from differences of elevation, in the Himalayas. Unfortunately, his collections, when incorporated with those of the British and other Museums, were indiscriminately labelled Nepal, whether they came from that country, from Sikhim, or from the plains of India. Serious confusion has resulted from this mistake and from others of a similar kind.

Mr. Hodgson's researches in Natural History were by no means his only claim to recognition. He was even better known for his enquiries into the languages, literature, and religion of Nepal than for his study of the fauna. He has been regarded as almost the discoverer of Tibetan Buddhism, and he certainly was the first to call attention to its literature. During the years that he lived at Darjiling, in the dwelling so admirably described in Hooker's

Himalayan journals—one of the most beautiful sites to be found, even in that magnificent range—by far the greater part of his time was devoted to the collection of the unwritten languages spoken by the scattered and rapidly disappearing aboriginal tribes of India, from the Himalayas to Ceylon. Nor were his diplomatic services small; among them it may be remembered that he was the first to advocate the enrolment of Ghurka sepoys, and to appreciate the merits of one of the most gallant races of Asia.

Owing to the interest excited in Paris, then a great centre of Oriental studies, by his researches in ethnology and literature, Mr. Hodgson became, as long since as 1838, a corresponding member of the Institute, and received the Cross of the Legion of Honour and a special medal struck by the French Asiatic Society. In England the recognition of his services to science was tardy and incomplete. He became a fellow of the Royal Society in 1877, and when he was in his ninetieth year the University of Oxford conferred honour on itself by investing him with the degree of D.C.L. It is to be regretted that no place has been found for him in the long roll of honours conferred by her Majesty for services in India.

CARL JOSEPH HYRTL.

BORN 1811. DIED JULY 17, 1894.

AMONG others who have passed away recently is CARL JOSEPH HYRTL, the anatomist, of Vienna. Dr. Hyrtl was almost as famous a teacher as an investigator. He aroused an extraordinary enthusiasm in his students, his classes being attended not merely by those qualifying for medical work, but by those already advanced in years and eminent in their profession. He was also celebrated for his anatomical preparations, which, scattered in all directions, bear eloquent testimony to his energy and powers. Dr. Hyrtl had a remarkable knowledge of languages, speaking Greek and Latin as well as several modern languages. He died at Perchtoldsdorf, near Vienna, where he had retired when the affliction of blindness closed his earthly labours. He is best known to the public by his "Anatomy of Man," and "Topographical Anatomy." His other works dealt with the anatomy of mammals, reptilia, and fishes.

FERDINAND HEINE, the founder of the Ornithological Museum which bears his name, died on 28 March at Halberstadt; LUCIEN FRANÇOIS LETHIERRY, the well-known student of Hemiptera, died at Lille on April 4, at the age of 64; EDWARD NORTON, the entomologist and specialist on Hymenoptera, on April 8, at Farmington, Conn., U.S.A., at the age of 70 years; Dr. F. QUIROGA Y RODRIQUEZ, Professor of Crystallography at the University of Madrid, on June 3.

Dr. Quiroga also contributed to our knowledge of the geology of the interior of Africa.

Carl Theodor Liebe, the geologist and ornithologist of Thuringia, passed away at Gera on June 5; and Edouard Lefèvre, the Secretary of the Société Entomologique de France, and a well-known Coleopterist, died at Paris on June 18, aged 55 years.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. J. Walther has been appointed Professor of Geology and Palæontology at the University of Jena; Dr. A. Tenne, Professor of Mineralogy at the University of Berlin; Dr. F. Altmann, Professor of Botany at the University of Freiburg i.B.; Dr. A. König, Professor of Zoology at the University of Bonn; and Dr. R. Scharizer, Professor of Mineralogy at the University of Czernowitz.

Mr. P. Chalmers Mitchell, M.A., F.Z.S., lecturer in Biology at the Charing Cross Hospital Medical School, has been appointed lecturer in Biology at the London Hospital Medical College.

Mr. R. Lydekker, F.R.S., is again in the Argentine Republic. He is engaged on the further study of the palæontological riches of the La Plata Museum.

Dr. C. J. Forsyth Major, accompanied by Mr. A. Robert, starts for Madagascar at the end of this month under the auspices of the Royal Society. Dr. Major will endeavour to complete our knowledge of the extinct animals that have inhabited in w land, and his special qualifications, both as a zoologist and an explorer, allow th hope for considerable and valuable results. Dr. Major is a Scotsman, and was n in Glasgow; as now he belongs to every country, we hope he will have little uble with any of the powers that be.

Mr. F. W. W. Howell proposes to make a third exploration of Iceland. The district to receive attention is the Northern Glacial district of the Vatna Jökull. The funds necessary Mr. Howell proposes to raise by promising a lecture on his return to any society or institution for a fee of five guineas.

Our statement that Dr. R. Semon had left the Anatomical Institute at Jena was gleaned from an erroneous source; although Drs. Braus and Drüner have become assistants at that establishment, it will not lose the valued services of Professor Semon.

The following eminent botanists have promised to attend the meeting of the British Association:—Dr. Strassburger, Dr. Zacharias, Dr. Errera, Dr. Pfeffer, and Mr. Douglas H. Campbell.

Mr. George Holt, of Liverpool, has given £5,000 to the University College f that town, which is to be invested to maintain the pathological and bacteriologi- al laboratories. Professor Boyce, of University College, has been elected by the ouncil to fill the newly-created post.

The generosity of Ludwig Mond, F.R.S., has placed at the disposal of the Royal Institution the freehold of No. 20 Albemarle Street, to be held for the purpose of a laboratory, to be named "The Davy-Faraday Research Laboratory of the Royal Institution." Mr. Mond further offers to make all necessary structural alterations and to equip the laboratory with apparatus, appliances, etc.

A GOVERNMENT Museum for Natural History and Ethnology has been established at Para, in North Brazil, and Dr. Emil A. Goldi has been appointed its first Director.

THE Library and Reading-Room of the Linnean Society will be closed during the month of August for cleaning.

THE Zoological Society of Dublin have received, on deposit from Mr. Cross, of Liverpool, an Orang Utang. The attendance at the Gardens during the month of May reached 11,000.

THE *Deutsche Fischerei-Verein* offers a prize of 1,000 marks for a paper embodying the best researches on the pathological and anatomical influence exerted on fish by the presence of the following substances in the waters that they inhabit:—(1) Free acids, (2) free bases, especially lime, ammonia, and soda, including the soluble carbonates of potash and soda, (3) free bleaching gases (chlorine and sulphurous acid). The determination of pathological characters accompanying the death of fish from asphyxia is also desired. As appropriate subjects the Salmonidæ and Cyprinidæ are suggested. Papers, which may be in German, French, or English, must be sent in by 1st November, 1896, to Professor Dr. Weigelt, Zimmerstrasse 90/91, Berlin, S.W., and from him further details may be obtained.

AT its meeting in Munich, the German Zoological Society drew up the final programme for the great systematic work to which we have alluded (NATURAL SCIENCE, Nov., 1893, p. 383). The work, which will be published by G. Fischer of Jena, is to be entitled "Das Tierreich. Eine Zusammenstellung und Kennzeichnung der rezenten Tierformen." Though excluding animals properly described now fossil, the work will take cognisance of such forms as have become extinct during historic times. Each species will be accompanied by a diagnosis, as brief and concise as possible. We are glad to find it recognised that such a work can only attempt to be a synopsis of actually published knowledge, and that no corrections or new descriptions can be admitted. The direction of this gigantic task is entrusted to Professor F. E. Schulze, of Berlin, who will be assisted by a committee of seven and a number of sub-editors.

THE Geological Society of London has raised its Composition Fee. We give the new bye-law which was passed at a special general meeting held on June 20, 1894. "A fellow may at any time compound for future annual contributions, that of the current year inclusive, by payment of thirty-five pounds, or if elected before the 1st November, 1894, by a payment of thirty-one pounds ten shillings, or if elected before the 1st November, 1877, by a payment of twenty-one pounds. If he has already paid the contribution for the current year, or any part of it, such payment shall be reckoned as forming a portion of the composition."

THE Geologists' Association of London holds its long excursion this year in Shropshire. The programme, which is elaborate and lengthy, speaks well for the energy of the leaders and the excursion secretary, Mr. Thomas Leighton. Members will have an opportunity of inspecting in the field the whole sequence of rocks below the Bunter Sandstone, and that under the leadership of such men as Lapworth, Watts, Blake, Callaway, and La Touche. The excursion lasts from July 28 till August 4. The headquarters will be Shrewsbury.

WE learn from *Science Gossip* that a Geologists' Association has been formed at Bristol, which holds meetings and makes excursions on Sundays. The honorary secretary is Mr. Frederick Ellis, 22 Senier Street, Bristol.

THE Hundred-and-tenth Meeting of the Yorkshire Naturalists' Union was held at Knaresborough, on Saturday, July 14, 1894, for the investigation of the Nidd Valley from Nidd Viaduct to Goldsborough Mill.

THE meeting of the French Association for the Advancement of Science will take place at Caen, from the 9th to the 15th August; the Geological Society of France will hold their extraordinary meeting at Lyons from the 19th to the 26th August; and the Botanical Societies of France and Switzerland at Geneva and the Valais from the 5th to the 14th August.

THE meeting of the Museums' Association at Dublin called out some interesting papers. Mr. H. B. White of the Dublin Museum described certain of the fittings and appliances there used. Mr. W. E. Hoyle, of the Manchester Museum, described some beautifully arranged cases, illustrative of the structure of the Foraminifera and the classification of the Pelecypoda; the latter explain the two different systems of arrangement according to shell-teeth and gill-structure. Messrs. Hoyle and Bolton explained an elaborate system of cataloguing fossils on a modification of Dewey's decimal method. Mr. G. H. Carpenter showed some attractive cases that he has arranged in the Dublin Museum to illustrate the chief features in the struggle for life, evolution, and geographical distribution of animals. Mr. H. O. Forbes, of Liverpool, advocated the centralisation of type-specimens in metropolitan museums, a suggestion that gave rise to a lively discussion. Mr. F. A. Bather, of the British Museum, had a great quantity of useful information, collected from museums in different distant parts of the world, which he endeavoured to impart to his colleagues. Professor T. Johnson, of Dublin, upheld the value of a *Wet*nical Museum, especially in its relation to agriculture.

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Su THE Missouri Geological Survey has been in trouble, and we regret to learn th t Mr. Arthur Winslow has been forced by circumstances to retire from its *D*ectorship. In his successor, however, Dr. C. R. Keyes, who has lately done much to od work on the Iowa Survey, the State secures a skilled observer already well acquainted with the geology of Missouri.

THOSE interested in the United States Geological Survey will find an appreciative notice of Major Powell's administration in the May-June number of the *Journal of Geology*. The writer, Professor T. C. Chamberlin, dwells chiefly on the value of a topographic survey inspired by a feeling for the genesis of physiographic features.

THE "Società Geologica Italiana," which was started in 1881 at Rome, now numbers 223 members. In the last number of its *Bollettino* (anno xii., fas. 4) will be found a series of papers dealing with the geology of Piedmont, more especially with the fossil Foraminifera, Coniferæ, etc., and a geological bibliography of some 1,100 entries.

THE "Society for the Study of the Amur Regions" has now become the nucleus of a new branch of the Russian Geographical Society. It will have its headquarters at Khaborovsk, and be known as "The Amur Branch." A yearly subsidy of 2,000 roubles will be given by the Government.

THE "Recent Territorial Arrangements in Africa" is the title of a short paper by Mr. Ravenstein in the *Geographical Journal* for July. In it are given two maps, one of the Congo State, and another of the Somali-land area, to illustrate the present arrangements come to by the eagles over the carcass of Africa.

IN his anniversary address delivered to the Royal Geographical Society, Mr. Clements R. Markham referred to the new catalogue of the library of the Society, which will be in the hands of fellows some time this year. This catalogue has now been arranged in one alphabetical order, and there will be two appendices, the first containing an alphabetical list of all the collections of voyages and travels, with an analytical table of contents to each volume; the second appendix containing anonymous and periodical literature arranged in geographical order. These catalogues have been prepared by Dr. Mill, Mr. Vincent Hawkins, and Dr. Murie.

A still more useful catalogue is being prepared for the same Society by Dr. Mill and Dr. Murie. That is a subject catalogue which is to be at once exhaustive, systematic, and exclusively geographical. Such a book as this, if properly carried out, will be an invaluable contribution to geography, serving as a guide to workers in all parts of the world. But, of course, it involves great labour. The number of titles to be indexed Dr. Murie calculates at 110,000, and this will print into something like 5,000 pages octavo. It is hoped that this book will be ready in about two years, and one might perhaps indulge the hope that Dr. Murie will keep an especial eye on bibliographies.

DR. R. LLOYD PRAEGER has written in the July number of the *Irish Naturalist* a sketch of the history of the Belfast Naturalists' Field Club. This is but the first of what must be a most interesting series of histories of the Irish Natural History Societies, and we shall await the succeeding papers with interest. The Belfast Naturalists' Field Club was the outcome of some courses of lectures on Natural Science by Mr., now Professor, Ralph Tate, and was started in 1863. Since then it has steadily increased in prosperity and usefulness, and has published many interesting and valuable papers in its well-known but somewhat complicate[d], "Proceedings" and "Appendices." The membership is now just about 500.

PART 1. of the fourth volume of the Actes Soc. Sci. Chili has just reache[d]. Mr. Boulenger has a paper on the fresh-water perches of Chili, and Dr. L. Ver[eau] Flores one on the cranium of an aboriginal Bolivian. The Society numbers s[ome] 250 members, and publishes in its "Actes" many valuable observations relating [to] Chili. The library seems to be growing rapidly under the care of Dr. Brion[es]. Parts 4 and 5 of volume iii., and parts 4 and 5 of volume ii. of the "Actes" wi[ll] appear shortly, and then these volumes will be complete.

IN a paper read before the Royal Irish Academy, Mr. R. J. Ussher shows that the Golden Eagle still breeds in Western Mayo, Donegal, Galway, and Kerry. The White-Tailed Eagle has been observed in Mayo and Kerry. The Pelegrine Falcon breeds fairly commonly throughout Ireland, but the common Buzzard is rare. Tree Sparrows also breed in co. Dublin, and are increasing in numbers. The Raven is everywhere rare, and the Bittern is now never to be seen.

DR. C. E. STIRLING, F.R.S., has contributed to *Nature* (June 21 and 28) an account of his remarkable discovery of the remains of *Diprotodon* and other extinct vertebrata at Lake Callabonna, South Australia.



OCTOPUS (*Octopus vulgaris*).

From a gelatine cast; reduced. U.S. Natio[nal]

CORRESPONDENCE.

Sewage or Filth-Fed "Fish."

Bordering on our coast, the sea being contaminated by ever-increasing sewage-outfalls, effluvial refuse and dirty rivers—too often only open drains artificially fouled from source to mouth—may sooner or later cause the *extinction* of the British trout and British migratory salmon by stopping their sojourn and spawning in such sewage-spoilt streams.

It was estimated in 1882 that local sewage and refuse had already so polluted and poisoned the rivers of England—reckoned roughly at about 60,000 square miles—that then (1882) upwards of one-sixth of these waters were incapable of supporting fish-life.

On the other hand, like some sea birds, many salt- and fresh-water fish frequent and are fond of a moderate quantity of, sewage, whose warmth, products, and contents also favour local aquatic vegetation.

Sewage-fed oysters, besides other aquatic and amphibian animals usually eaten either absolutely raw or else insufficiently cleaned and cooked, cause avoidable parasitic diseases, fatal fevers and poisoning, which details are discussed in my treatise on "Foul Fish and Filth Fevers," that the United States Fish Commission will shortly publish, and my article on "Sewage-Fed Fish" in *Public* ... of June, 1894.

It is imperative that immediately a Royal Commission, a Select Committee, or at least a Parliamentary Return should be granted to inquire into and report upon remedies to diminish the dangers to health, and the damage to property, food, and ... caused by the augmenting sewage pollution of our inland and coastal waters.

J. Lawrence-Hamilton, M.R.C.S.

Sun or Moon?

Is not *Orthagoriscus mola* usually considered a sun-fish (*vide* Günther "Study of Fishes," p. 690) not a "moon-fish," as stated by your reviewer on p. 71?

11 Wellington Road, Brighton. Edward Crane.

July 7th, 1894.

[Sun-fish or Moon-fish.—The paper we referred to was "Note sur un *Poisson-Lune* (*Orthagoriscus mola*, L.), de grandes dimensions, capturé sur les côtes du Portugal." What *we* know as the moon-fish is *Ephippius gigas* of the Antilles, but there is no reason for supposing that *Orthagorisci* are called sun-fish in all parts of the world.—Editor.]

A Three-Toed Passerine.

Sir,—I am much indebted to you for pointing out in your Editorial Note on my paper (Natural Science, vol. v., p. 10) the interesting fact of the persistence of the *flexor longus hallucis* in three-toed birds. The note contains, however, a slight mistake, viz., the statement that there is no three-toed passerine bird. *Cholornis*, a genus believed to be allied to *Suthora*, has but three toes. The suppressed toe, however, is unfortunately not the hallux, but the fourth toe. For a three-toed passerine of the kind you implied, I fear we must wait till an abnormality occurs.

Frank Finn.

Two Corrections.

We are indebted to Miss Agnes Crane and to another correspondent for pointing out that the paper by Dr. Meyer referred to in our last issue (p. 14) is not a new publication, but the first part of vol. iv. of the "Abhandlunge und Berichte des K. Zoologischen und Anthropologisch-Ethnographischen Museums zu Dresden." We are indebted to Miss Agnes Crane also for reminding us that the Senior Editor of the "Country Month by Month" is not the "Son of the Marshes," but Mrs. Owen, who has herself edited the writings of the "Son of the Marshes."

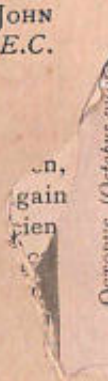
Editor.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan and Co., 29 *Bedford Street, Strand, London, W.C.*

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Octopus (*Octopus vulgaris*). From a gelatine cast; reduced. U.S. Natio...

NATURAL SCIENCE.

SPECIAL ILLUSTRATED SUPPLEMENT TO NO. 30, AUGUST, 1894.

Taxidermy as a Fine Art.

IF any doubt the value of that connection between science and art to which we have referred on p. 90, then a glance at the fascinating Report by Dr. Shufeldt, which we reviewed in our July number (p. 58–60), would convince them of their error, at least so far as ...dermy is concerned. Dr. Shufeldt's plea is throughout for a ... artistic rendering of the stuffed and modelled animals and ...ips in our museums; and when we inquire how this desirable ...ult is to be attained, we find that it is solely by holding the mirror ... to Nature herself. That this has not always been done is proved, ... proof be needed, by several of the illustrations to the Report; while that it can be done by those with a wide knowledge of nature, artistic feeling, and command of technique, is sufficiently evidenced by many other of the beautiful plates in the book. Through the kindness of Dr. Brown Goode, Assistant Secretary of the Smithsonian Institution, we are now enabled to present our readers with a selection from these plates, to many of which we alluded in our review.

The specimen of *Octopus vulgaris* (Pl. I.) is a gelatine cast taken from a plaster mould, and then faithfully coloured according to nature. The mould was made, as is usual in these cases, not from the animal itself, but from a carefully prepared model, which in this instance was based on a figure by Verany. "It is hard," says Dr. Shufeldt, "to realise what a perfect representation one of these finished gelatine casts gives of the living animal; and, the cast being perfectly pliable, it still further enhances the resemblance to the original."

The Skate, *Raia erinacea* (Pl. II.), is a plaster cast taken from the animal, and coloured, the eyes and minor appendages being added after the cast is made. Some such process as this is the only one by which the large cartilaginous fishes can be reproduced with any marked fidelity to nature and fit for a first-class museum. "Rays,"

says Mr. Hornaday, "are the meanest of all subjects that vex the soul of a taxidermist. Such abominable animated pancakes, with razor edges that taper out to infinite nothingness, were never made to be mounted by any process known to mortal man. To mount the skin of a vile ray, and make it really perfect and lifelike, is to invite infinite shrinkage, rips, tears, warps, defeat, and humiliation at the hands of your envious rivals. . . . The best way to mount a ray is to make a nice plaster cast of it, paint it, and then bury the accursed ray in a compost heap."

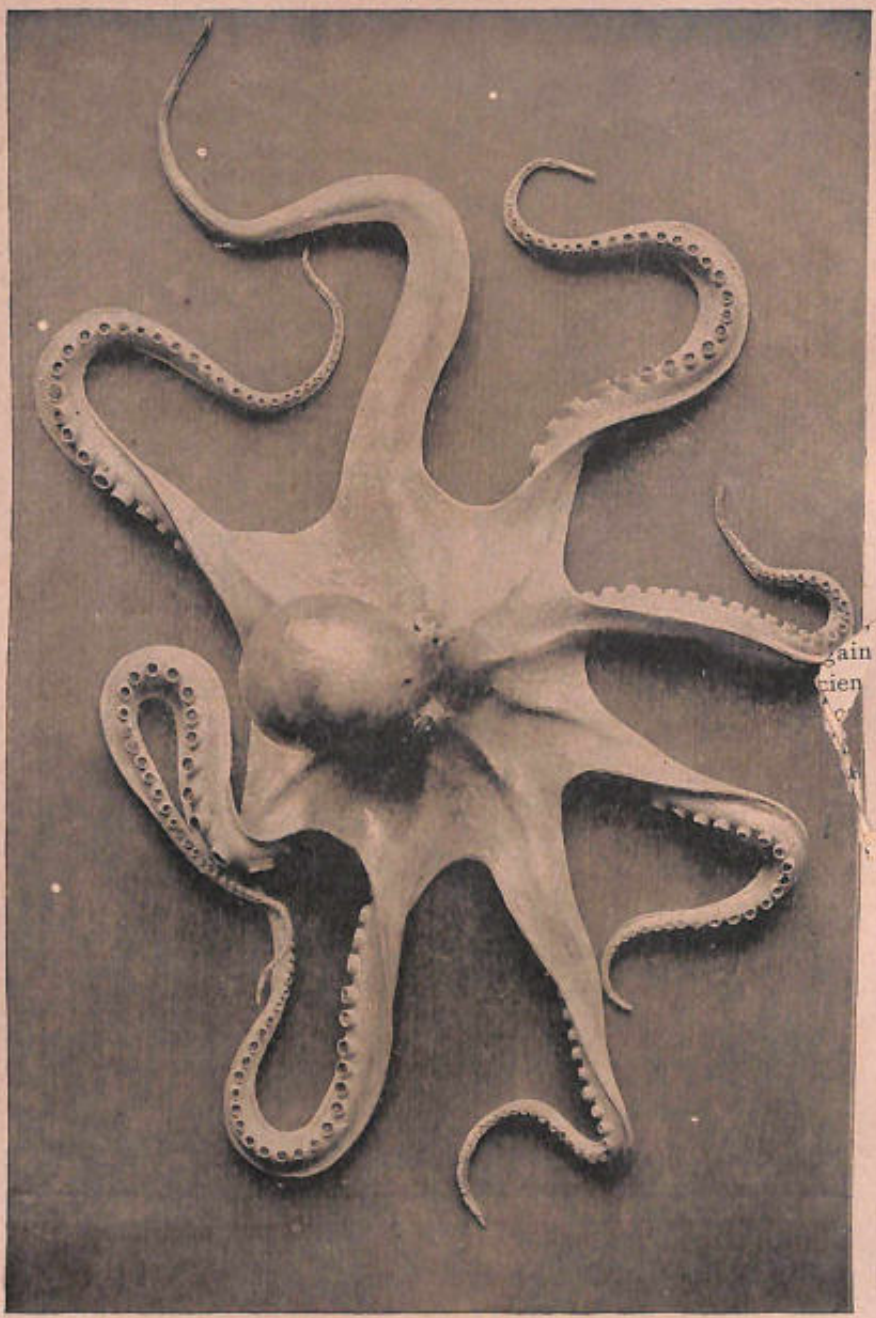
The Rattlesnake, *Crotalus adamanteus* (Pl. III.), is an example of the beautiful plaster casts of reptiles, made by Mr. Joseph Palmer, which are a striking feature at the National Museum at Washington. "With tail elevated, and the reptile thrown into natural coils, partly within the recess of the spreading roots of a large tree, we have," says Dr. Shufeldt, "an accomplishment in plaster, the equal of which for that particular snake I do not believe to be extant."

Lizards, owing to the delicacy of their details, are far more difficult things to cast. "Nevertheless the plaster casts of some of the larger lizards leave us nothing to be desired in that art. A truly magnificent thing is seen in the plaster cast of *Tupinambis*. It would seem to be perfect in every particular." (Pl. IV.)

The figures of the Great Auk both represent the same specim[en] which is owned by the Museum at Washington (Pl. V.) We a[re] quote Dr. Shufeldt. "A figure of this as first mounted by some an[cient] bungler is shown. No living auk in good health ever stood in that p[ose]tion; but, thanks to what art can sometimes accomplish in these da[ys] this outraged bird was not destined for all eternity to stand as a drum major at dress parade. It was determined to have it remounted; a operation, owing to the age of the specimen and the lack of knowledge as to what condition the skin might be in, that required a full measure of judgment. The work of remodelling was accomplished by Mr. Wood." The result is that the extinct fowl "presents a far more respectable appearance, and is certainly posed in a far more natural attitude, though judging from Audubon's plate of it, I believe it still to be not a posture this auk was wont to assume. Still it was most assuredly the very best that could have been done under the circumstances."

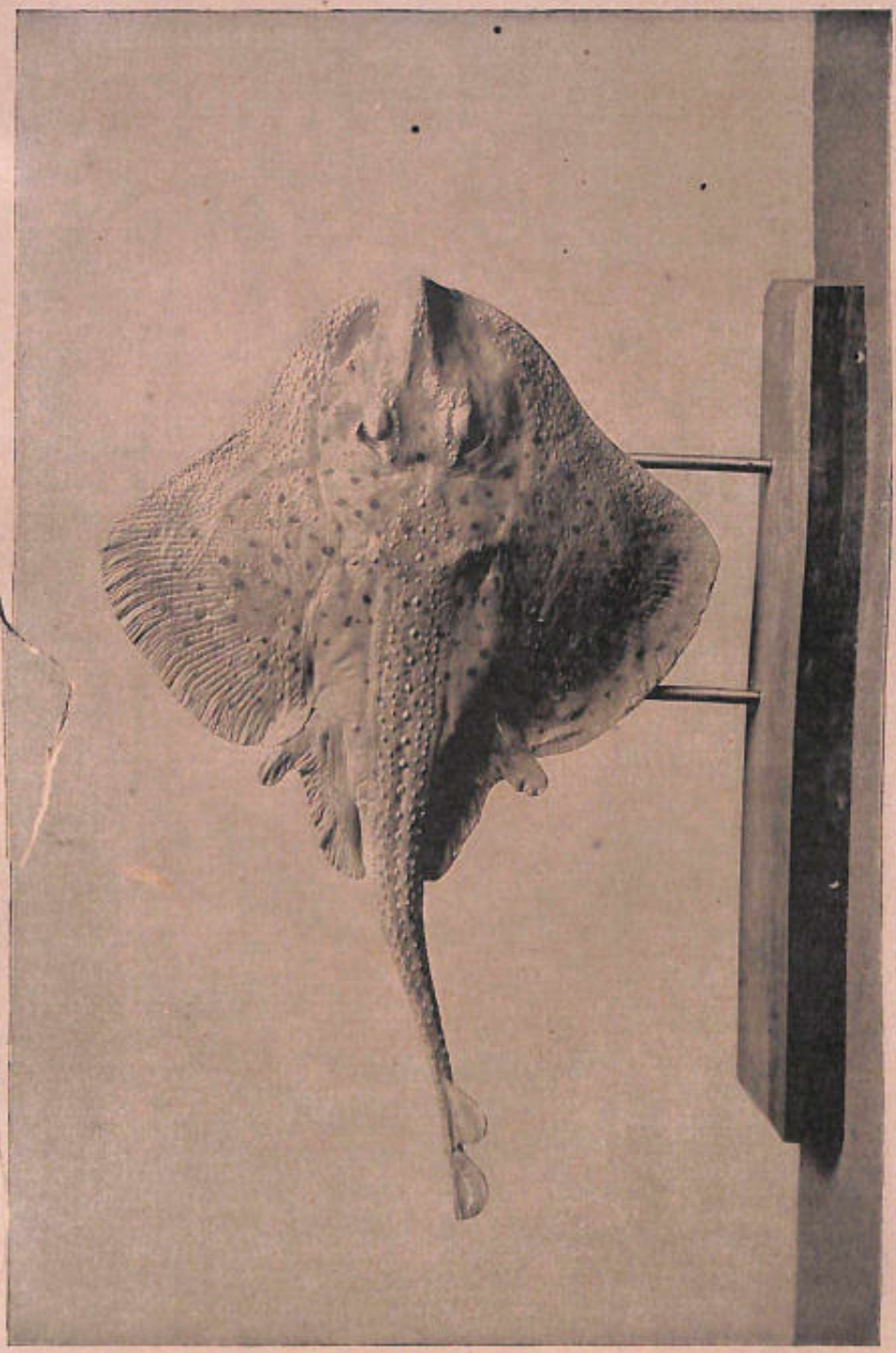
The head of a Tiger (Pl. VI.) illustrates how well the great difficulties encountered in representing the mask of a living animal can be overcome. On this hear again the words of Dr. Shufeldt: "One who has not seen the feat performed in one of our larger museums can have but little conception of the skill required in handling the facial expression and all the structures that enter into the mouth parts. The skinning of a tiger's tongue and preserving it so as to make that organ resemble the original as it appeared in the living subject; the cleaning of the teeth; the blending of the black part of the lips with the delicate pink gums inside; to make the animal grin and not smile,

OCTOPUS (O[ctopus] "From a gelatine cast; reduc[ed]



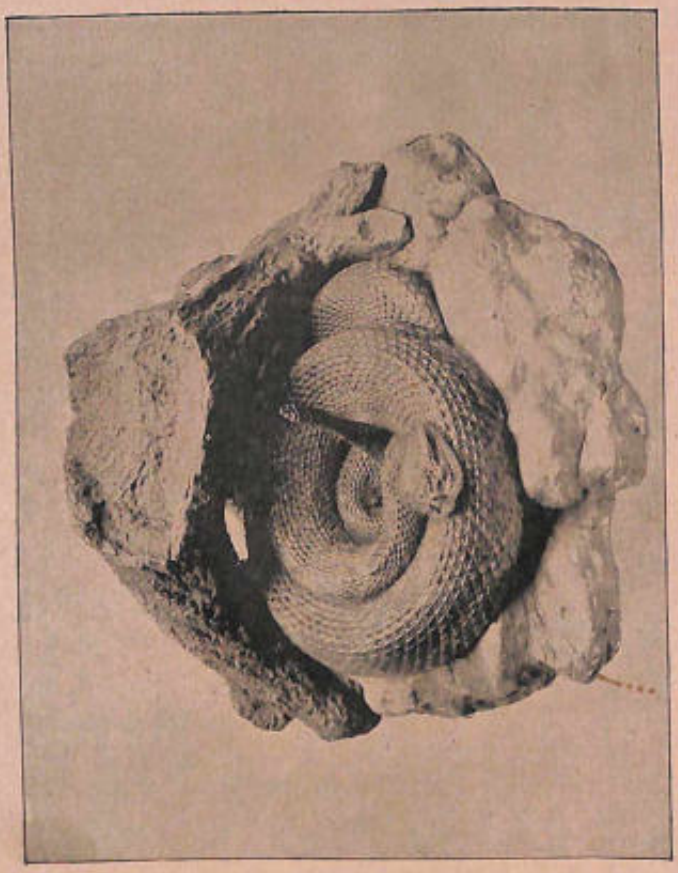
OCTOPUS (*Octopus vulgaris*).

From a gelatine cast; reduced. U.S. Natl Museum



SKATE (*Raia erinacea*).

From a plaster cast; greatly reduced. U.S. National Museum.

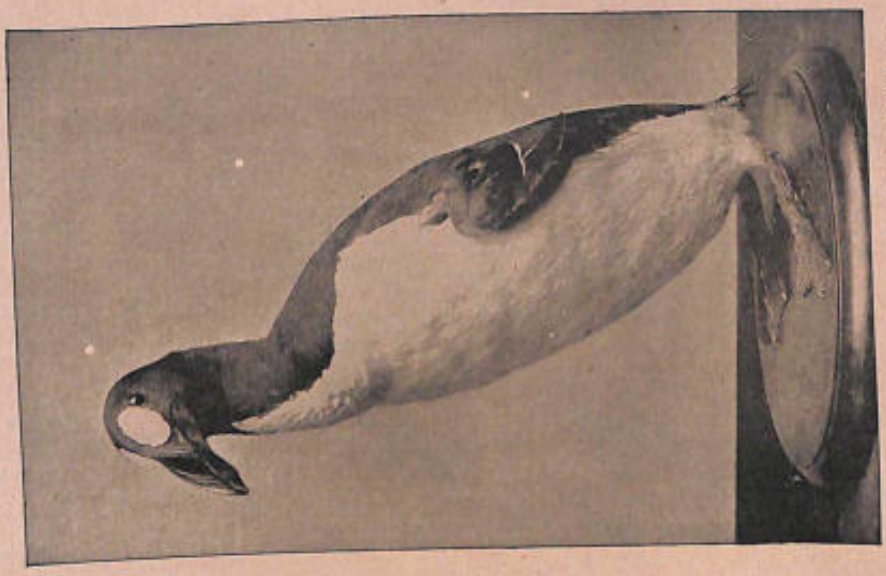


RATTLESNAKE (*Crotalus adamanteus*).
From a plaster cast. ½ reduced. U.S. National Museum.

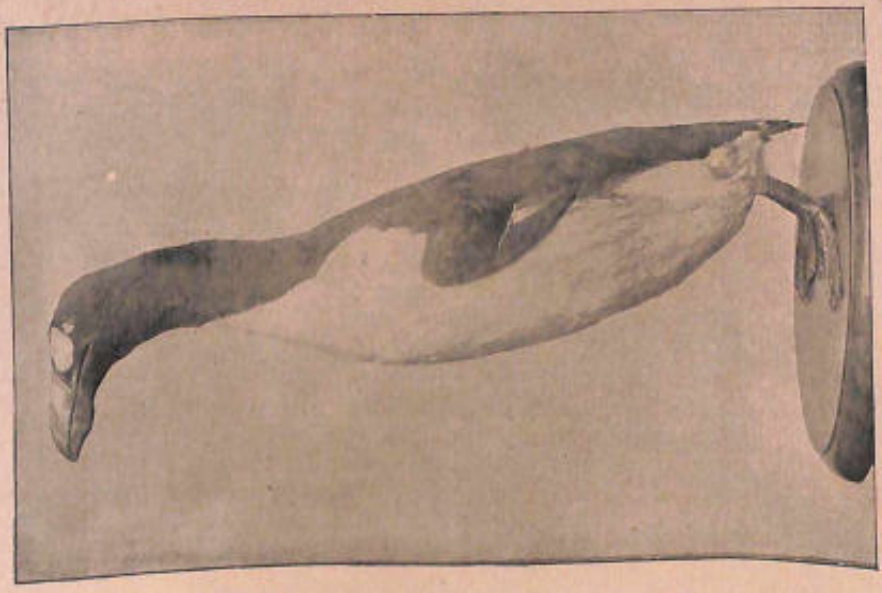


TUPINAMBIS TEGUIXIN.

From a plaster cast; reduced. U.S. National Museum.



As remodelled by Mr. Nelson R. Wood.



As first preserved.

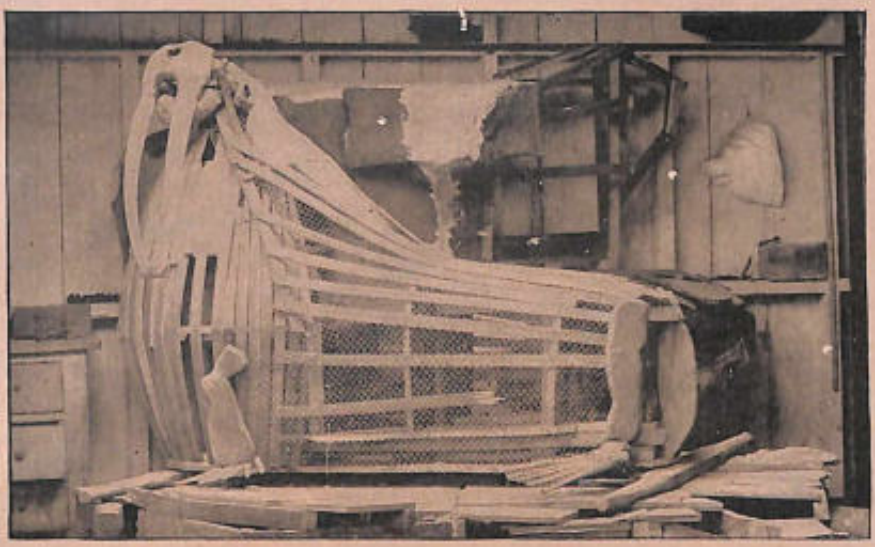
The C[illegible] *[illegible]utus impennis*).
Museum.



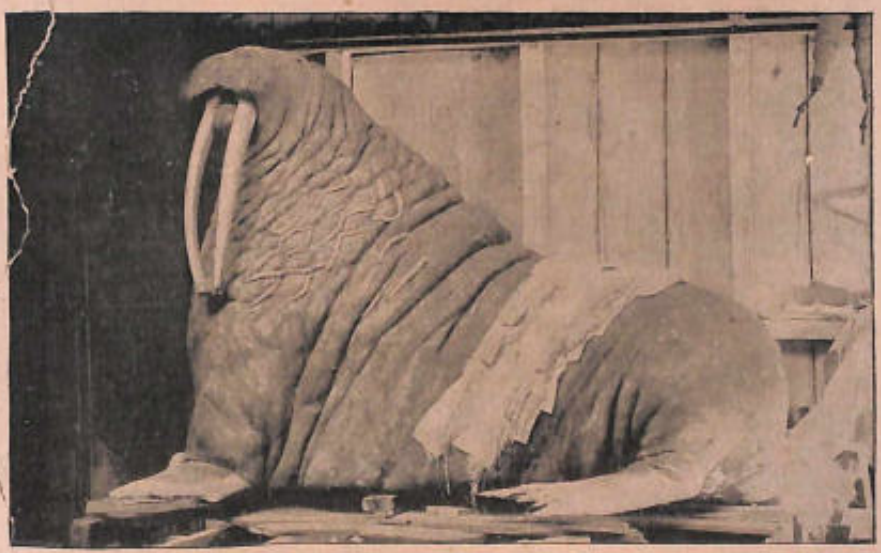
HEAD OF TIGER (*Felis tigris*).
U.S. National Museum.



HEAD OF ZEBRA (*Equus* [illegible]).
U.S. National Museum.



MANIKIN FOR A WALRUS. (Partly completed.)



MALE WALRUS (*Odobænus obesus*). (Nearly completed.)
U.S. National Museum.

and to lend to the eyes the flash of anger, are all accomplishments that demand of the artist his best judgment, knowledge, skill, and, what is more, his infinite patience." Hear also that eminent artist in this line, Mr. James Hornaday. "The large Felidæ are the finest subjects for the taxidermist that the whole animal kingdom can produce. They offer the finest opportunities for the development of muscular anatomy, and the expression of the various higher passions."

A specimen in which difficulties of a similar nature have been admirably coped with is the Burchell's Zebra, of which the head is shown from the side in Plate VII. This is thus described by Dr. Shufeldt.—"The animal has been given an attitude indicative of moderate movement, with the evident idea in its mind of making an attack or standing at bay, in which he will use his teeth to bite—a habit so familiar to us in some cases of vicious horses. The short mane is semi-erect, the ears are thrown back, the eye looks the owner's intent, while the quivering and nearly rigid lips drawn apart show the glistening upper 'nippers' and the crowns of the lower ones; the nostrils are somewhat closed by the elevation of the superior lip; finally the entire rendering of the whole animal is most perfect in all particulars."

The two figures of the Walrus on Plate VIII. are of exceptional interest. This animal is now exceedingly rare and on the very verge of extinction. Familiar though it has been to us from our earliest childhood, yet we have learnt of late that the pictures of it in works on zoology and natural history, "even in so good and generally correct a work as Brehm, are glaringly false." Only in the last few years have truthful drawings, made from actual observation by Mr. H. W. Elliott, been available for the taxidermist to take as his models. According to Dr. Shufeldt, the specimen of this colossal mammal at the National Museum in Washington "was mounted in the light of all the improvements and skill modern taxidermy could bring to bear upon the undertaking, and the success was complete. It constituted, when finished, one of the grandest subjects the Smithsonian sent on to the Government exhibit at the Columbian Exposition." All opinions, however, are not so favourable; for Dr. W. H. Dall, as already quoted in NATURAL SCIENCE (vol. iii., p. 337), said,—"A fine walrus might have been more lifelike if the taxidermist had had a better guide than Elliott's caricatures of this unfortunate animal, which, in addition to extinction, seems to be doomed to posthumous misrepresentation." The figures, whatever may be the accuracy of the specimen, serve to illustrate the mode of stuffing by means of a manikin, which, especially in the case of hairless animals, is often covered with clay, which can be worked into shape after the skin has been put over it.

In selecting the plates for this article, we have paid but small attention to the many beautiful illustrations of birds. In respect to

bird-groups our home museums do not require much teaching, though even they have yet to learn that a bird can be mounted in the most natural manner on an ordinary museum-perch or stand. It is in preparing the other classes of Vertebrata and the Invertebrata that American taxidermists take the lead, and it is their excellence in this direction that we have endeavoured to set forth as an example. We cannot, however, go so far with Dr. Shufeldt as to look forward to a time when museums shall display in monster cases, picturesquely arranged, the faunas of entire regions or the animal and plant life of various geographical areas. Such a method of exhibition leads, almost certainly, to hopeless incongruities, and prevents a proper inspection of the specimens exhibited. The scene-painter must not interfere with the scientist, A museum is a palace of truth before it is a palace of art.

It is particularly interesting to note the enormous development of the art of taxidermy in the United States which characterises a quite recent period. This is due, in Dr. Shufeldt's opinion, to the stimulating influence that the World's Columbian Exhibition had upon every art and industry, an influence, one may add, that extended far beyond the limits of the great republic. It was not, however, merely the desire to rival other institutions and countries that gave so great an impetus to the art, but the fact that a sufficient appropriation of the needful dollars enabled their true strength and best work to be put forth by such men as F. W. A. Lucas, Joseph and William Palmer, Nelson R. Wood, Henry Denslow, A. H. Baldwin, George Marshall, A. Z. Shindler, and J. E. Benedict, who form part of the enthusiastic staff at the National Museum of the United States of America.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 31. Vol. V. SEPTEMBER. 1894.

NOTES AND COMMENTS.

Organic Life on Mars.

THE hold taken upon the popular mind by the astonishing revelations of astronomy is again in evidence. The planet Mars is the nearest neighbour to the earth, and at present the two are at the nearest parts of their circling courses. From the observatory at Nice, Mr. Javelle has seen on the southern edge of Mars a luminous projection. In accordance with the admirable co-operation among the astronomers all over the world, this interesting observation has been telegraphed from station to station. These telegrams naturally have found their way to the public Press, and from their reaction upon the journalistic mind have come a crop of articles and interviews, of speculations and theories, and all these turn upon the twin questions —is the planet Mars inhabited? and are the inhabitants signalling to us?

We share with everyone else the compelling fascination of these questions. Time was when the earth, like a little child, thought itself the centre of all things: now it has grown up enough to know that it is the tiniest unit in the ordered polity of the universe. It is an insignificant appanage of a system which is the merest dot in star-sown space. In the effort to reach some cognisance of our place in the unknown, the planet Mars seems to offer some hope of a standard of comparison, some basis for induction. With a knowledge only of this earth, the imagination may run riot and people the planets of this and other systems with benign or evil figments of fancy. With a knowledge, however slight, of the actual state of things on the surface of Mars, we should lose the morbid fictions engendered by isolation and at the least gain a method by which speculation might be guided.

It is now known that the physical conditions here and on Mars

are similar in many respects. Both the earth and Mars "turn alternate cheeks to the reverberation of the same blazing world," and both encounter the same, or nearly the same, range of temperature. Mars, like the earth, is covered by land and by sea. It has mountain ranges and volcanoes, plains and valleys; it has wind storms and rain and clouds. The polar regions are covered with snow and ice. Summer and winter succeed each other as here, although the actual transition from season to season is much more sudden. Mr. Norman Lockyer, for instance, says that he has seen a tract of snow, that would reach from London to Madrid, melt in a week, and suggests that the so-called canals of Mars are deep channels hollowed out by the sudden periodical melting of the snows. We know, of course, that the same chemical elements, and many of the compounds with which we are familiar here, exist on Mars. The only known physical condition differing much is the force of gravity, which is considerably less on Mars.

Can we conclude anything from these data about the presence of organic life? In the first place, organic life, as we know it, is associated invariably with protoplasm. In that mixture of materials, the chief chemical compounds present are the albuminous substances known as proteids. A temperature about freezing point arrests the activity of these: a temperature above 40 degrees centigrade destroys them. So far there is no difficulty, for certainly there are on Mars, as here, some parts where the temperature remains all the year round within these limits. Next, water must be present, and in this respect also, the conditions on Mars almost certainly are favourable. The water must be tolerably pure: the presence of a certain amount of salts would be no barrier, for, although even the best elementary text-books of biology know only the amœbæ of fresh water, amœbæ are common animals of brackish water, and of the sea-shore. The presence of free mineral acids in solution would be fatal: but from the similar physical and chemical conditions here and on Mars, there is every reason to believe that free acids would exist as seldom in nature there as here. The different conditions of gravity afford more perplexing problems. Oscar Hertwig and others, by experiments upon developing ova in centrifugal machines, and so forth, have shown that neutralising the effects of gravity makes little difference to the forces of growth. But the researches of Bütschli open up graver questions. If there be truth in his analogy between protoplasm and physical foams, only those who have attempted to repeat his experiments can judge how small are the differences in tensions and consistencies that make or mar the results. If the structures and movements of protoplasm depend, as he thinks, chiefly upon differences of surface tension, no one can say how a difference in the force of gravity would interfere with the delicate interaction of forces upon which tensions depend.

But granted the possibility of the mere existence of protoplasm

on Mars, a much wider set of problems at once arises. For we are more interested in the possibility of the existence of that reasoning intelligence which, within our experience, is always associated with such integrations of protoplasmic structures as we find in man and the higher animals. The framework and structure of all animals and plants are adapted so intimately to the existing conditions of gravity, that the course of the evolution of life under other conditions must elude even our imagination. If from the same simple beginnings a chain of life has grown up upon Mars, it must have grown up so different that to us its final results would be a horror and a monstrous prodigy. Even if it has grown so that its highest result has like us become able to be conscious of a side or part of the absolute truth of things, it is very improbable that this consciousness, acting in terms of and through different sense organs, possibly upon a different side of the absolute truth, could ever come into relation with ours. If ever we could "open up communication with Mars" in the sense of projecting such physical signs as would be intelligible to minds acting through sense organs like our own, it is more than probable that they would be unintelligible to minds deaf to what we hear, blind to what we see, not feeling what we touch, and realising their environment in ways inscrutable to us.

Lord Salisbury's Address.

The University of Oxford and the English nation have equal reason to be proud of the remarkable address of the President of the British Association; for, so far as the University is concerned, their supreme officer, the Chancellor, is chosen for very different reasons than his eminence in science; so far as the nation is concerned, Lord Salisbury is honoured as a man of affairs, as one who gained worthily and held worthily the great office of her Chief Minister. It is the fashion to say that the University is the home of effete culture; it is the fashion to say that politics are in the hands of mere rhetoricians, of shallow devotees of expediency. Here is the head of a great University, an acknowledged leader of politicians, who can hold enthralled on scientific subjects an audience as full of scientific experts as ever is gathered together. Discount all one may for the modulations of the practised speaker, for the political habit of exquisite adaptation to an audience, it remains to be said that unless Lord Salisbury had had solid scientific matter in his speech, as well as subtle intellectual form, he would not have received the enthusiastic reception that was his.

It is easy to analyse his arguments and to criticise his conclusions. To these agreeable tasks we shall presently address ourselves. It is not so easy, on after thinking, to account for the general scientific approval of an address which was really an exposure of what the

average cultured Briton regards as the failures of science. Probably the root of the matter is this. With regard to any scientific subjects but their own, scientific men are no other than average cultured Britons. During our conduct of NATURAL SCIENCE this has been thrust upon our notice. It is our endeavour to have subjects so treated that the results of any research shall be intelligible to those who are unfamiliar with the methods of that research. Of course we receive much criticism, but, for the most part, it is not the average cultured Briton who writes to say that a paper on biology, or on geology, is unintelligible. It is the working geologist, apparently, whose standard of the intelligible in biology is the lowest; it is those who are adding to our knowledge of plants and animals that find most difficulty in comprehending what geologists say they are about. Lord Salisbury's address dealt with three or four widely-different branches of science, and we may take it that although his audience included a large number of experts in various branches, yet the verdict of the audience was not the verdict of science, but the verdict of the average cultured Briton. Why, then, is average culture so cheered by the exposition of "the great failures" of science?

AN UNFOUNDED STORY.

IT is reported, in an entirely unveracious chronicle, that after the discovery of America, Columbus returned to Spain and settled down to a life of dignified ease. He became the lion of several seasons, and, as the great discoverer of America, was the favourite of the Court and the darling of the populace. He was invested with every national and civic dignity, and grew a power in the land. When a question of state was being discussed in the council of the king, the king himself would command that the voice of the discoverer be heard. When the populace was seething and troubled, and demagogues shouted against the accredited rulers of the nation, the words of the discoverer of America acted like a charm. But one day, in a moment of expansion, Columbus remarked that there were, no doubt, many undiscovered countries, and that, personally, he had a longing to discover the North Pole. His words were taken up and repeated upon all sides, and dissatisfaction grew with their repetition. Finally, the king himself sent for Columbus, and said:—"Columbus, we honour you greatly and have rewarded you, perhaps lavishly, for your discovery of America; but we must remind you that you have not discovered the North Pole. There is but one America; there are, you tell us, two Poles. We will not bind you harshly to the discovery of either or both. There are two Poles—discover us one of them." And so Columbus, gathering an expedition, set out for the north. When, after years of wandering, he returned, unsuccessfully, he sank into an unhonoured grave, known not as the great discoverer of America, but as the man who failed to find the North Pole.

LORD SALISBURY'S NORTH POLES OF SCIENCE.

Two of these undiscovered countries of science (the nature and origin of the elements and the nature of ether) are outside the special scope of NATURAL SCIENCE. But in passing we may observe that, while modern science is as unable to transmute the metals as were the early alchemists, it is the merest dogmatism to say that the boundary of our knowledge in this direction remains where it was many centuries ago. The cataloguing and recording of the elements, the negative results themselves of Stas, the researches of Crookes upon the evolution of the elements, and the grouping of them by Mendeléeff, are all great strides in knowledge. We may be near or far from knowledge of the nature and origin of the elements: it is certainly true that we are nearer the goal by those years of brilliant result and patient research upon which Lord Salisbury justly laid so much stress.

With regard to the nature and origin of life, we should reply to Lord Salisbury in similar fashion. It is quite true that we do not know the exact nature or the origin of life. But we do not regard it as a problem so remote as to rank among the almost impossible quests of science. If to-morrow Professor Bütschli or some other investigator were to demonstrate that the phenomena of life could be called into existence in the laboratory, we should feel that an advance much more momentous in the minds of the populace than in the minds of biologists had been made. For the result of recent research has been to break down the barrier between the mystery of living matter and force and the mystery of inorganic matter and force. Biologists will not accept Lord Salisbury's definition of life as "the action of an unknown force on ordinary matter," as "a mysterious impulse which is able to strike across the ordinary laws of matter, and twist them for a moment from their path." Whatever be the nature of life, it is not a force twisting and thwarting the ordinary laws of matter. Nor is life in any ordinary sense of the term a force at all. If we speak of the chemical or physical work done by living organisms as the results of the force of life, either we use the term as the vaguest metaphor, or we imply that the force of life is only a phase of ordinary physical and chemical forces. For the work done by an organism can be measured in terms of the chemical and physical energies it employs, and the actual growth of an organism, the multiplication of cells, depends upon ponderable transformation of ordinary physical and chemical forces. The results of much recent work on the nature and functions of protoplasm lead many to think that the supposed barrier, between living matter and matter that is not alive, has already been broken down, and that living material is only a peculiarly complicated mixture of known chemical and physical forces. Be that as it may, there is more of the orator than of the thinker in Lord Salisbury's handling of the problem of life. Life, like electricity, or

chemical affinity, or gravity, is not a thing to be explained in a formula. We know the positive and negative conditions of its existence; every month adds to our cataloguing of its phenomena and of its relations to other phenomena. We know nothing but by its qualities, and it is a fair statement to make that science has learned to know as much of the qualities of life as it knows of the qualities of anything. As each step in the slow accumulation of knowledge is made, it will always be open for someone to say, "Ah, my friends, you have discovered America, but how about the North Pole?"

LORD SALISBURY AND NATURAL SELECTION.

DARWINISM and the Doctrine of Descent are now in the unhappy position of popularity. Only those who have some practical acquaintance with the facts involved, with the anatomy and embryology of living animals, and with the history of their fossil remains in the rocks, hesitate to lay down the law for or against what they consider Darwinism. Lord Salisbury's acute mind has avoided the most vulgar error. Since Darwin's solution laid hold of the intelligence of the world and armed it with a new method for the study of organisms, month by month proofs of the doctrine of descent have accumulated, until now no naturalist of position or attainments asserts the immutability of species. But we go beyond Lord Salisbury, and say confidently that there are not six among the well-known biologists of the world who do not hold it as proved that all animals and plants that are alive, or have lived on the earth, are connected with each other by the chain of a common descent. It is upon this achievement that the undying fame of Darwin is based. The dispute about the fact of evolution is over. After the lapse of so many stormy years, the word evolution has earned its right to Lord Salisbury's witty designation "comfortable." It is, as Lord Salisbury rightly said, concerning the method or mechanism of evolution that there is still controversy, and that there may be controversy to the end of time. At the end of the introduction to the first edition of the "Origin of Species," Darwin wrote:—"I am convinced that Natural Selection has been the main but not the exclusive means of modification." At different times after the publication of the "Origin," as may be seen in the different editions of that and other works, Darwin was inclined to attribute sometimes a greater, sometimes a less, important sphere of influence to this factor. At the present time one school, of which Weismann is the leading exponent, believes that Natural Selection, to the exclusion of other factors, is the motive force in evolution. Probably a majority of living naturalists attach to it less importance than did Darwin. But it was because Natural Selection or, "the preservation of favoured races in the struggle for life," furnished an intelligible mechanism for the process of evolution, that Darwin was able to

convince the scientific world of the fact of evolution. Whether or no the final verdict of science be for or against Natural Selection, it will remain the most stupendous example of the value of scientific theory in directing scientific observation.

But Lord Salisbury has regarnished for us two objections to the validity of Natural Selection. The first of these is the question of the exact amount of time during which evolution has proceeded. Now we do not know the duration of time which is expressed by the Tertiary strata. We do know that, relatively to preceding periods, the Tertiary period is but a short episode in the history of the earth. Yet, in this short period, there have come into existence nearly all the species and genera, and many of the families now living. A very small acquaintance with geology shows that, whatever be the case with the method, there has been time for the actual occurrence, for the excellent reason that the occurrence is actual. As for the amount of time being prohibitive of the process of Natural Selection, Lord Salisbury's objections depend upon a view of the mode of operation of Natural Selection which, so far as we know, is peculiar to Lord Salisbury. But it is not necessary to enter into the view of Natural Selection that the President propounded, because his second objection to Natural Selection is, that we cannot demonstrate the process of Natural Selection in detail, and that we cannot even imagine it. This second and final objection is taken by Lord Salisbury from a recent paper of Weismann. To enter into it would be to open up the whole Darwinian controversy. But we may say that we agree with Lord Salisbury that, if Natural Selection implied that for every step "two individuals of opposite sexes, accidentally blessed with the same advantageous variation, had to meet and transmit by inheritance that variation to their successors," then no lapse of time would be sufficient to transform even one species into another. But that, of course, is a burlesque of the supposed process.

We do not agree with Weismann's statement, quoted with approval by Lord Salisbury, that, if Natural Selection be rejected, there is no resource but to fall back on the mediate, or immediate, agency of a principle of design. But we admit fully that if there be no hypothesis to account for the evolution of species but the action of selection upon indefinite variations in each generation, then there is room, and more than room, for the hypothesis of some inherent and directing force of phylogenetic development, which is simply design writ small. On the other hand, every recent publication bearing upon these matters is full of suggestion and argument and fact against the nature and use of variation being such as Lord Salisbury supposes.

Dwarfing by Experiment.

Those of our readers who have paid attention to the recent researches on the manner and causes of variation, to which we have

of late directed their attention, will be interested in a paper that Mr. H. de Varigny has just published in the *Journal de l'Anatomie et de la Physiologie* (1894, pp. 147–188).

In 1874 Professor Semper published the results of some experiments made with specimens of *Limnæa stagnalis*, in order to ascertain the effects of the environment on the growth of the animal, and more especially the causes of dwarfing. By isolating young individuals obtained from the same mass of ova, and placing them in vessels each containing a different amount of water, he found that reduction in the bulk of water, below a certain quantity favourable to full development, was attended by a corresponding reduction in the size of the animals. Since the young snails were fully supplied with food, and placed under similar conditions of temperature and light, and since the state of aëration of the water would be without effect on these air-breathers, the cause of the dwarfing was not very apparent; and Dr. Semper was finally driven to conclude that the reason must be sought in the presence in the water of some substance, not as yet ascertained, which was necessary to the growth of the animal, and the amount of which in the smaller volumes of water was insufficient.

To test these observations more fully, and more especially the Professor's conclusion, Mr. de Varigny undertook a series of experiments. Some of the results arrived at he has embodied in the paper before us. He selected as subjects *Limnæa stagnalis* and a species which he terms *L. auricularis*, but which from his figures is undoubtedly an inflated form of *L. peregeri*. Individuals hatched from the same mass of ova were isolated in vessels containing different amounts of water and offering equal or different areas of aëration in the several experiments. The vessels were placed under equal and favourable conditions of temperature and light, and the animals furnished with abundant food. The results went to show, first, as Semper's trials did, that, within limits, the size of the shell increased with the increase of volume of the water the animal was placed in; secondly, that the dimensions of the *Limnæa* increased in proportion to the increase of surface exposed to the air, and that increase was almost more pronounced than in the case of increase of volume of the water alone, and even persisted when the superfices was increased while the volume was diminished; thirdly, other conditions being equal, the size of the shells varied in inverse proportion to the number of individuals inhaling a given volume of water. Semper's conclusion, however, as to dwarfing being due to some unknown substance in the water, was neatly disposed of by experiments in which individual *Limnæa* were penned off from their fellows, and compelled to occupy a small space in the same vessel, while a thorough circulation of the water was effected. The results were just the same as if they had been confined in separate small receptacles.

Seeking, therefore, to explain the cause of this dwarfing in some other way, Mr. de Varigny suggests that it is due to lack of room in

which to move about, a proposition he supports by ingenious arguments, while admitting that further experiments (now actually in progress) are necessary. While looking forward to the publication of these further details, we would venture to commend this paper to the special attention of certain of Mr. de Varigny's countrymen and others in this country, as offering, by means of its figures, a magnificent opportunity for self-advertisement in the creation and naming of limitless varieties and so-called new species.

"A Moving Grove," *Shakespeare*.

In our June number (vol. iv., p. 406) we had something to say about the way in which animals were protected from discovery by their resemblance in colour or form to surrounding objects. Now a man, if he wishes to escape observation in this manner, does not change his shape or the colour of his skin, but clothes himself in some appropriate substance, or, like the soldiers of Macduff when they advanced on Dunsinane, hides himself beneath a covering of natural objects. Just in the same way, though possibly without the same conscious intent, certain of the very humble dwellers in our modern seas take to themselves a cloak fashioned from the common objects of the sea-floor. Thus, a certain coiled shell has received its name, *Xenophora*, "stranger-bearer," from this very habit; for the molluscs of this genus attach to their shells, which are usually very thin, such things as stones, shells and pieces of coral, which are found at the bottom of the sea in which they dwell. Three such specimens have recently been placed in the central hall of the Natural History branch of the British Museum, one of them, *Xenophora pallidula*, from Japan, and the others, *X. conchyliophora*, from Bermuda. One of them is covered with a number of other shells, and another with stones and sand; and it is interesting to see how large these foreign bodies are in proportion to the fragile shell itself, and how completely they hide it. Close by them there is exhibited a crab, *Maia squinado*, which came from Plymouth, and is entirely covered by fragments of sea-weed, sea-mats and sea-firs. These organisms are not merely growing on the shell of the crab, but are attached to it by the hooked hairs with which the exposed surface of its body and limbs is clothed.

These cases are interesting enough, but a further and more wonderful development of the same method of protection is found in certain sea-urchins that inhabit the harbour of Kingston in Jamaica. Their names are *Toxopneustes variegatus* and *Hipponoë esculenta*, and unlike many other sea-urchins or sea-eggs, as they are called in Jamaica, these species do not stay at home in holes or crevices, but wander about the smooth, hard bottom, in very shallow water, where many sea-weeds are growing. But like other sea-urchins, these animals are provided with little suckers or tube-feet, and while they use some of these to drag themselves along, they use the suckers on their backs

to carry about bits of sea-weed and shell and small pebbles. By this means they very successfully conceal themselves from the naturalist and no doubt from other enemies; but the edible *Hipponoë* is, unfortunately for itself, less an adept at the practice than is the species of *Toxopneustes*.

NEMATODES IN THE BLOOD OF MAN.

THE English race, among the many interesting things which they bring back from the tropics, occasionally unwillingly bring back for the consideration of English medical men parasites of the tropics. In the new issue of the *Transactions* of the Norfolk and Norwich Field Naturalists' Society Mr. H. D. Geldard describes an interesting case of *Filaria sanguinis hominis* in the blood of a patient who resided many years in India. The specimens abounded in the blood of this patient, and were that larval phase of *Filaria bancrofti* which is called *Filaria nocturna*, as it appears in the blood of man only from sunset to sunrise. These young embryos are taken from the blood by the female mosquito, which also is a nocturnal animal. A few days after its meal the mosquito retires to water to deposit her eggs, after which she dies, and her dead body drops into the pond or stream. Within her body the embryos which had got into the alimentary canal from the blood of man have been rapidly maturing. They issue from her dead body by means of a boring apparatus developed at their anterior end. They swim freely through the water, and apparently ultimately perish unless they are swallowed in the water by man. If this happens they bore their way from the stomach to the lymphatic vessels, where they become sexually mature. Each female pours out a multitude of small embryos, each within a delicate egg-shell. These find their way into the blood-vessels and lie concealed, except at night, when the mosquito is active, and so gives the opportunity for this cycle of development to begin again. The consequences of their presence in man may be trifling, but by choking the lymphatics they may cause some most unpleasant and dangerous diseases.

HOP-DISEASE IN KENT.

WE learn from Professor J. Percival, of the South-Eastern Agricultural College, Wye, Kent, that he is at present engaged investigating the disease which causes hops to become what is known as "nettle-headed." The disease is met with in many districts in the county, and has rapidly increased during the last few years—in some cases leading to a complete destruction of considerable areas of hop gardens. The early delicate varieties are most attacked, and especially those which have been growing for a considerable time. It is rarely until five years after planting that the disease is seen, and usually not until a much later period.

In a typically affected plant the leaves are smaller than is generally the case, and are transformed in such a manner as to become strikingly like those of the stinging-nettle. The edges of the leaves turn upwards; the fibro-vascular bundles in them are increased slightly in thickness, and stand out well from the under surface of the leaf.

Closer examination shows destruction of the soft tissue in immediate contact with the fibro-vascular bundles; the parts become thin and yellow, and much resemble the thin spots produced by the action of sulphur dioxide gas on the leaf.

The internodes of the plant are short and the main stem and branches taper more rapidly than in healthy plants. In practically all cases the bine or stem, after climbing a short distance up the pole, loses it power of twining and topples over; when not tied up the whole stem becomes slack, slides down its support, and the plant lies in a heap on the ground.

So far the experiments, which are being carried out on the Bettring estate of Messrs. E. A. White & Co., Paddock Wood, point to the circulation within the plant of a poison which brings about curling of the leaf, stoppage of growth of the fibro-vascular tissue, and loss of turgor of the cells in the leaf near the ribs, with resulting yellow patches there.

The leaves on the main stem nearest the ground are affected first, and then follow those at the nodes above in regular succession. The branches in the axils of these show the same regular sequence of diseased leaves, the lower branches being affected first. The cause is undoubtedly connected with the root, and Professor Percival finds in the cortex of many diseased specimens, close up to the bast, considerable numbers of the nematode *Tylenchus devastatrix*, Kühn, usually known as an eel-worm.

Before any completely satisfactory cause can be established or remedy suggested, much work is necessary. At a later date we hope to be able to give an extensive communication from Professor Percival dealing with this interesting and important investigation.

The Indian Geological Survey.

The usefulness of the Geological Survey of India appears to be fully recognised by the practical men of the Dependency. Thus, we read in the *Records*, vol. xxvii., p. 65, that, in his inspection of the Bengal collieries, Mr. Grundy, the Inspector of Mines, has "experienced very earnest co-operation on the part of the agents and managers of the mines." On the other hand the Survey is not blind to the advantages it may itself gain by friendly co-operation with the practical men. The maps of many large coal-fields that were made some twenty and thirty years ago, on the largest scale available at that time, are now found to be insufficient. It is suggested therefore

that the Bengal Colliery managers should form a committee to prepare a revised and enlarged map of the Ranigunj coal-field. "The managers would appear to have quite sufficient data before them in the way of borings and shaft sections, which, combined with their local knowledge, should enable them to contribute materially to the production of a lasting standard work of this kind. An initiatory step might be made by supplying managers with copies of the latest revenue and other maps of their districts, on which each gentleman could enter all the data at his disposal according to some settled scheme of geological and mining delineation. The Geological Survey Staff is all too limited for such an undertaking, but by judicious co-operation with a committee of the kind indicated, a great deal might be done to effect the desired end." A concrete instance of the assistance that may be so rendered is already to hand in an excellent map of the Giridh coal-field by Dr. Walter Saise, manager of the East Indian Railway Collieries, which is to be presented to the Survey for publication. It is distinctly cheering to find that the Indian Geological Survey does not snub the work of private observers in the way that some other Government Departments are accused of doing.

TERTIARY RIVERS.

THE little island of Eigg in the Inner Hebrides has long had a fascination for the geologist. Readers of Hugh Miller's charming book "The Cruise of the Betsey" will remember his description of the Scuir of Eigg, that large wall of curious pitchstone-porphyry that surmounts the southern end of the island, running across it from E. to W. This wall appears to be the remains of a once far larger mass of lava, which flowed down from some adjacent volcano and filled a river valley. The river deposits of this valley, with remains of a pine, are preserved beneath the Scuir. At the meeting of the British Association, Sir Archibald Geikie drew attention to a little rock called Hysgeir, which lies in the sea some eighteen miles west of Eigg. This rock consists of just the same peculiar porphyry as the Scuir, but lies at a much lower level. It therefore seems to prove a further extension of the river-valley, and a descent of it about 35 feet in a mile. Some other islets north of Hysgeir exhibit records of a still older river. In what must have been the valley of this river there are found a succession of coarse river gravels alternating with bedded basalts. The lowest of these gravels contains remarkably large blocks, some as much as six feet in length. Some of the gravels or conglomerates pass into true volcanic agglomerates; while in other places there are layers of fine tuff, volcanic sandstone, or shales with remains of land-plants.

The sequence of events which these various deposits indicates was described by Sir Archibald as follows. "During the outpouring

of the lavas of the great basaltic plateaux of the Inner Hebrides, a river flowed across the volcanic plain from the Western Highlands, whence it carried large quantities of shingle. By successive violent floods, these materials, together with the detritus of the lava-fields, were strewn irregularly far and wide beyond the immediate channel of the river. In the pools thus left behind, fine volcanic silt gathered and entombed leaves and stems of the surrounding terrestrial vegetation. But volcanic activity still continued, and though ashes, slags and pumice were swept down, new eruptions took place by which masses of rock, sometimes nine feet in diameter, were thrown out to a distance of a mile or more, and fresh streams of lava were poured out, completely burying the previous accumulations. Renewed river-floods of gradually-lessening severity spread fine detritus over the cooled sheets of basalt, and again these later fluviatile deposits were entombed between fresh outbursts of lava. Perhaps no more striking evidence can be elsewhere obtained of the condition of the land surface over which, from many scattered vents, the materials of the volcanic plateaux of the Inner Hebrides were slowly piled up." It is interesting to find that in these far-distant Tertiary times the slope of the drainage areas of the Western Highlands was the same as in our own day.

Landscape Marble or Cotham Stone.

The curious deposit known as Cotham Stone has exercised the ingenuity of many to find a cause for its formation. As early as 1754, Edward Owen published a work entitled "Observations on the Earths, Rocks, Stones and Minerals for some miles about Bristol, and on the Nature of the Hot Well and the Virtues of the Waters," and in this book he suggests that the peculiar arborescent structures of the Cotham Stone were produced by the escape of imprisoned air, which permitted the blacker portions of the mud to follow the bubbles, and so get dispersed in tree-like patches. Others have thought that gaseous emanations from the *Avicula contorta*-shales had produced the peculiar markings, and others again suggested that mineral infiltrations would account for them.

Mr. H. B. Woodward in 1893 (Mem. Geol. Surv. The Jurassic Rocks, vol. iii., p. 31) gave it as his opinion that the "arborescent markings were produced during the consolidation of the stone, and more particularly by the shrinking of its upper portions. In this way, and while the mud was still in a more or less pasty condition, one or more of the dark films in the banded mass were disarranged and dispersed in arborescent form in the slowly-setting rock." He suggested also that pauses in deposition and exposure to the sun's rays might have accounted for some of the structure, and concluded that markings were due mainly to mechanical forces, though there were evidences of chemical change.

Mr. Beeby Thompson, in the last number of the *Quarterly Journal of the Geological Society* (August, 1894), has examined all previous writings on the subject, investigated the matter, and concludes that the peculiar characteristics are due to interbedded layers of vegetable matter, which continued to decompose and evolve carbonic acid gas and marsh gas after deposition; and that where a layer of extra thickness occurred, the decomposition continued whilst a thickness of several inches of new sediment was laid down, with the result that arborescent markings were produced along the lines taken by the escaping bubbles. Mr. Thompson, in this interesting paper, gives his reasons for these conclusions, and records his experiments to reproduce artificially the characteristics of landscape marble.

THE SYSTEMATIC POSITION OF TRILOBITES.

FOR many years the position of the Trilobites in the scale of organised beings has been doubtful. One school has insisted that they should be referred to the Crustacea, and another to the Arachnida. Mr. H. M. Bernard has just published, in the *Quarterly Journal of the Geological Society* (Aug., 1894), an important zoological paper in which he discusses at length the question, and refers these ancient life-forms definitely to the Crustacea, the original position assigned to them by Linnæus and by Burmeister. We are unable to give the details of Bernard's paper, but quote his own words as given in his "summary":—

"It is now possible . . . . . to fix with great probability the zoological position of the Trilobites. The bending round ventrally of the first segment, the great labrum with antennæ attached at its sides, the 'wandering' of the eyes, the pores (pointing to the probable presence of water-sacs), the head with a varying and progressively increasing number of segments, the dorsal organ, the rudimentary character of the posterior segments, and the gradual diminution in size, with the essentially lobate or phyllopodan type of the limbs, all serve to connect the Trilobites with *Apus*.

"This relationship cannot, however, be considered as direct. *Apus*, on account of its richer segmentation, the absence of pleuræ on the trunk-segments, and its more membranous parapodia-like limbs, must be assumed to lie in the direct line upwards from the original annelidan ancestor towards the modern crustacea. The Trilobites then must have branched off laterally from this line either once or more than once, in times anterior to the primitive *Apus*, as forms specialised for creeping under the protection of a hard imbricated carapace. This carapace was obtained by the repetition, on the trunk-segments, of the head-shield which, as we have already seen, almost certainly existed as a structure *sui generis* in earlier forms, and, somewhat modified, has been retained as such in the early crustacea proper ('Aspidophora').

"It seems to me, therefore, that the Trilobites, studied in the light of new discoveries, especially of those which we owe to American investigators, yield the most interesting and important evidence as to

the origin of the crustacea. Stripped of their pleuræ and of the expansion of the head-shield, we have, in the early Trilobites (*e.g.*, *Olenellus*), long segmented animals tapering at the posterior end. The first segment is bent round ventrally, so that the large labrum points backwards. The appendages of the first segment appear to have functioned as sensory organs and to have pointed downwards, being inserted at the sides of the labrum. The following segments were provided with membranous lobate appendages carrying, on their dorsal edges, gills and sensory cirri, and distally specialised into locomotory organs. The alimentary canal ran through the whole length of the body, bending round anteriorly to open through the mouth.

"The Trilobites may thus be described as *fixed specialised stages in the evolution of the crustacea from an annelidan ancestor, which bent its mouth round ventrally so as to use its parapodia as jaws.*"

To us it has seemed in the highest degree improbable that crustacea could have come from a specialised annelidan, but it will be interesting to see what the "arachnoid" theory-holders have to say in reply to this paper, which appears to be carefully and thoughtfully worked out. It is fully illustrated, and marks a distinct revival of palæozoological interest in the Geological Society which we are pleased to see. We note, however, that of those four who entered into the discussion of the paper, three were zoologists, geologists as a rule caring for little else than stratigraphy or petrology.

Species-Making and Species-Taking.

In one branch of biology there still rages a battle about species. Those who are concerned with the collection, naming, and description of fossil and living species are troubled constantly by the stupid and unprincipled action of species-mongers of the baser sort.

Opposed though we are to too great extension of professionalism or officialdom in science, we have sometimes wished that there could be something in the nature of a diploma without which no one should be permitted to practice, *i.e.*, to publish, or that there were some means of disabling an incompetent or dishonest writer, such as the Bar exercises when it strikes a man off the rolls. Even now there is a case that, if the facts be correctly stated, certainly calls for some such drastic treatment. A writer named Victor Lopez Seoane has recently been distributing a couple of pamphlets, entitled "Aves nuevas de Galicia" and "Revision del Catalogo de las Aves de Andalucia," both of which bear the imprint "La Coruña, 1870." There is, however, clear internal evidence that the date of one of these is incorrect, as it refers to the British Museum Catalogue of Birds, the first volume of which did not appear till 1874. Moreover, it is stated by Mr. C. D. Sherborn, who discusses the question in the August number of the *Annals and Magazine of Natural History*, that when these pamphlets reached him "the ink in which they were printed was apparently fresh and easily smeared." Whether or no it is designed for the purpose, this pre-dating gains priority for some

of Seoane's own names over names proposed by Howard Saunders and Reichenow. The sooner such writings are ruled out of court, the better it will be for systematic zoology.

Another case of what seems illegitimate "Species-making and Species-taking," is illustrated by a little polemic now proceeding between two well-known palæontologists, Professor Ralph Tate, of Adelaide, and Dr. Bittner, of the Royal and Imperial Geological Institute of Vienna. Every now and again some "lumping palæontologist" figures two specimens to represent the same species; then, to his annoyance, one of the "splitting" students of that science promptly makes a new species out of the second figure. We have all heard of cases in which some over-zealous member of the *nouvelle école* of conchology has made a second species out of a different view of the same specimen. Dr. Bittner has not been so unscientific as this. Some time ago Professor Tate sent to Vienna a collection of specimens of the Australian Lower Tertiary Echinoidea. As this fauna has not yet been fully worked out, Professor Tate asserts that he was very careful to send only well-known species, and to select what he regarded as most typical forms. Nevertheless, so different are Dr. Bittner's and Professor Tate's views of specific differences, that the former subsequently issued a paper ("Ueber Echiniden des Tertiärs von Australien." *Sitzber. Ak. Wiss. Wien.*, Bd. ci., Abth. i., pp. 331–371, pls. i.–iv.) in which he founded seven new species and one new variety on this material. That he also established two new genera for two species which had been previously described was not surprising. To this Professor Tate has replied in some "Critical Remarks on A. Bittner's 'Echiniden von Australien'" (*Trans. Roy. Soc. S. Australia*, vol. xv., pp. 190–194), in which he refuses to accept any one of Bittner's new species, and maintains that these are all founded on specimens as typical of the old species as any that could be selected. He ascribes Dr. Bittner's error to his ignorance of the specimens as they occur in the rocks, a cause which is undoubtedly efficient in many other instances of splitting palæontology.

I.

Rev. George Henslow on Natural Selection.

IN NATURAL SCIENCE of July Mr. Henslow makes some statements with regard to variation and Natural Selection which call for critical remark. He says that, though cultivated plants vary *indefinitely*, and therefore require selection to produce definite modifications, this is not the case in Nature—"Variation in Nature is always in strict adaptation to the direct action of the environment; in other words, *natural variation is always definite.*" This statement seems to me so extraordinary, and so opposed to well-known facts, that I can only impute it to the use of the terms "vary" and "variation" in two very distinct senses; first, as meaning those individual variations which occur abundantly both in nature and under cultivation; and, secondly, as meaning those particular variations which alone survive under nature and produce a "variety" or a "species." In this latter sense, of course, "natural variation" is definite; but so, in the same sense, are the variations of cultivated plants. From unstable and indefinite "variations" man and nature alike produce definite "varieties." As one out of the innumerable examples of indefinite variation which might be named are the fifteen different *modes* of variation observed by Alph. dê Candolle on a single oak tree, while in a great number of common species an equal amount of variability may be observed both in wild and cultivated individuals; and all these variations are *indefinite*, in the sense that they do not usually occur in one direction only, from the typical form. A few examples of such variations have been given in my "Darwinism," pp. 76–80. I cannot, therefore, understand either the meaning or the value of the statement—"*natural variation is always definite.*"

It is not quite clear whether Mr. Henslow admits the agency of Natural Selection at all. He says: "I would ask what *facts* are producible to *prove* that Natural Selection acts at all on the maintenance, if not the origin, of any floral and, indeed, other structures?" It is, of course, admitted that *direct* proof of the action of Natural Selection is at present wanting; but the indirect proofs have been so cogent as to overcome the most violent prejudice and opposition, and to convert a large majority of naturalists to a belief in its agency. It is, therefore, rather late in the day to deny its existence without

adducing some adequate and proved substitute. What Mr. Henslow does put in its place is the reaction of vegetable tissues to the environment, resulting in adaptation; and in the special case of flowers he imputes all the variety of form and endless modifications of structure to "the responsive action of the protoplasm, in consequence of the irritations set up by the weights, pressures, thrusts, tensions, etc., of the insect visitors."[1] Now the very first essential to this theory is to prove that modifications produced by such irritations are hereditary. Here, if anywhere, we want facts. Yet in the very interesting volume to which Mr. Henslow refers us, crowded as it is with facts and observations, I can find only two or three slight references to this most vital point. At page 147 he quotes Darwin as saying that the excellence of our milking cows and goats may be attributed partly to selection and "*partly to the inherited effect of the increased action, through man's art, of the secreting glands*," and adds, "This fact" is strictly analogous to what takes place in the vegetable kingdom! Here we have a mere opinion of Darwin's, nowhere supported by direct observation or experiment, and now seriously challenged by a large body of naturalists, set forth as "a fact." Again, at page 157, the case of the various "ant-plants" of the eastern tropics is referred to, and it is stated that Dr. Beccari explains the curious hollow stem in which the ants dwell as partly due to the irritation of the ants inducing hypertrophy of the vegetable tissue, which "then becomes hereditary"; and Mr. Henslow concludes that there is abundant evidence to prove that many organs of a plant, if subjected to irritation, can become materially altered and develop new processes, and, "secondly, that these altered states, if the irritation be persisted in, may become hereditary." Here again are only opinions without a particle of proof; and I can find nothing more to the point in the whole volume. The case of galls is very briefly referred to at p. 144, and their non-heredity is passed by with the remark that the predisposition to produce them *may* be greater now than formerly, and that the galls themselves *may* be larger than they were at first. But surely if the effects of insect irritation are anywhere hereditary it would be here. An oak tree which lives several hundred years is subject to this irritation in greater or less degree almost every year, and the irritation itself is not momentary and intermittent, as in the case of insects visiting flowers, but is kept up by the presence of the egg and growing larva during a considerable portion of the period of active vegetable growth, and this has been going on for thousands, probably millions, of years. Yet neither do oaks nor any other plants produce galls spontaneously, as they certainly should do if the results of irritations are in any general sense hereditary. This seems to me to be a really crucial experiment continually repeated by nature.

I may here remark that Mr. Henslow's theory utterly breaks down owing to the want of any conceivable connection between

[1] "The Origin of Floral Structures," p. 340.

insect irritation and most of the innumerable adaptations of the parts of flowers to attract insects and secure cross-fertilisation. Such are the sticky glands, the elastic filaments, the springs and traps, and the accurately timed motions of the pollinia in orchids; the innumerable complexities in papilionaceous flowers; the large coloured tracts in Bougainvillea, Poinsettia, and many others; the flowers with tightly-closed lips, as Linaria, Antirrhinum, Melampyrum, etc.; the enlarged rays of Compositæ, Umbelliferæ, and Caprifoliaceæ; the general massing of small flowers into heads, umbels, corymbs, or dense racemes, so as to become conspicuous, and many other characters. To these may be added the negative evidence of the numerous genera and orders of regular flowers, such as Campanula, Rosaceæ, Gentianaceæ, and many others, which, though thoroughly adapted for insect fertilisation, and whose lower petals have therefore been always subject to irritations, have never developed irregular flowers. In all these cases variation with Natural Selection will account for the phenomena, while insect irritations, even if we admit heredity, will not do so. From whatever point of view we approach the question, the attempt to explain floral structure and colour without the aid of Natural Selection is a hopeless failure.

In the *Journal of the Linnean Society* ("Botany," no. 208, July 10) there has just appeared an elaborate paper by Mr. Henslow on "The Origin of Plant-Structures by Self-Adaptation to the Environment, exemplified by Desert or Xerophilous Plants," in which the author still further develops his view as to the influence of the direct action of the environment unaided by selection. The only portion of this paper on which I propose to remark is that dealing with the origin of spines and prickles, on which I have already had occasion to write in my book on "Darwinism," when combating Professor Geddes' views on the same subject. Mr. Henslow imputes the spines and prickles of so many plants inhabiting dry countries to the direct influence of the conditions under which they live. This, he thinks, is proved by some of these plants losing their spines when grown under other conditions; he adduces numerous examples of the abundance of spiny plants in such countries as Nubia, Abyssinia, and the Kalahari Desert; and he again and again reiterates the statement that these characters are "simply the inevitable results of the action of environment."

Now if these statements comprised all the facts, that is, if in all dry countries spiny plants abounded, while in all moist or fertile districts they were absent or very rare, the explanation given of their origin would have some plausibility. But there is no such general coincidence of aridity of soil or atmosphere with abundance of spiny plants, as very little enquiry will show. Mr. Henslow points out several other plant-characteristics which indicate, and, as he thinks, are directly caused by, aridity. Such are very small, coriaceous, or rolled up leaves, or their complete absence; a hairy or woolly covering

to the whole plant; succulent foliage; special protection of the buds; enormous development of roots; abundance of bulbs and tubers; together with thickness of bark and various protective coatings to stems and leaves. Now many of these peculiarities are present in the flora of the Brazilian Campos—as well described in the memoir of Eug. Warming on Lagoa Santa—which is referred to by Mr. Henslow as corresponding in many respects with that of other arid regions. Yet the author of this memoir expressly states that "spiny plants are very rare" (p. 463). Again, the plants of the Galapagos present similar indications of aridity—shrubs with minute and almost invisible leaves, for example—yet, except the cacti, which may be of American origin, none of the endemic species are spiny. So, also, the rich Sandwich Island flora contains hardly a single endemic spiny plant; and I am informed by the Rev. R. P. Murray, who is well acquainted with the botany of the Canaries, that spiny plants are exceedingly rare in those islands, though much of the surface, owing to the porous volcanic rock and the long periods of drought, presents the conditions which elsewhere are said to produce spines.

Now without denying that—other conditions being equal—aridity may favour and moisture may check the growth of spines, there is another and altogether different set of conditions which seem more directly connected with their abundance or rarity. This is, the presence or absence of herbivorous mammals, against whose ravages spines are a protection. The most destructive of these animals are camels, goats, and antelopes, and it is where these are indigenous—in Arabia, North-east and South Africa, and Central Asia, that thorny shrubs and trees are especially abundant. Again, few countries have more spiny plants than Chili, where the camel-like vicuguas and alpacas, as well as large rodents, are very destructive. But the country is not especially arid, and the remarkable Puyas, whose leaves are armed with excessively sharp recurved spines, inhabit the subalpine regions where rain and mist prevail. In our own moist islands we have a full proportion of prickly plants, and the same may be said of North America, where the *Gleditschia* or Honey Locust has the young branches, and in old trees the trunk, armed with groups of very strong and sharp spines. So also in Japan, notwithstanding its moist insular climate, we have an *Olea* and an *Osmanthus* with holly-like prickly leaves; while the prickly *Berberis Darwinii* is found in the damp atmosphere of the Straits of Magellan.

Equally opposed to the theory of aridity as the efficient cause of spines is their abundance on palms growing in the hottest and moistest regions of the globe. In many Amazonian species the stem is thickly set with long and very sharp spines pointing downwards, and thus forming a complete protection against monkeys and other arboreal fruit-eating mammals. Many species of *Bactris* and *Astrocaryum* are thus armed, as is also the beautiful *Guilielma speciosa*, the Peach palm, whose fruit is large and edible. It is a suggestive

circumstance that, with the exception of palms, few large trees are spiny, and when they are so, as in the case of the *Gleditschia*, the spines are most abundant on the trunk and on the younger branches. In the same way, our holly, when it grows to a large size, usually has the leaves towards the top spineless: the wild pear also is spiny below but unarmed above. The climbing palms, on the other hand, are armed to the very top, but in this case the spines assist climbing.

The anomaly of the flora of the Brazilian Campos having most of the true xerophilous characteristics, yet being almost wholly without spiny forms, is quite in harmony with the fact of the great poverty of this region in mammals destructive of woody vegetation. There are really none but a few deer and cavies, which are mostly inhabitants of the more wooded valleys, and which are kept from undue multiplication by the considerable number of species of Felidæ and Canidæ in the same area.

We are, therefore, led to conclude that the apparent direct dependence of an unusually spinescent vegetation on arid conditions of soil or climate is to a great extent deceptive. Such conditions are inimical to the growth of dense forest, and it is a well-known fact that the larger mammalia abound most in partially wooded or open country. Many of these animals are exceedingly destructive to shrubby or aborescent vegetation, especially in districts which are subject to occasional droughts; and it is in such areas that so many of these plants have acquired the protective armature of spines or prickles, while others not so protected have sooner or later succumbed, thus leading to a preponderance of the former. But the numerous instances in which considerable areas and extensive floras are found to have hardly any spinous plants, as compared with other areas in which the soil and climate are generally similar and where such plants abound—the only important difference being the absence or presence of destructive herbivorous or frugivorous mammals—show us clearly that it is the latter rather than the former condition which is the real starting point and efficient cause for the development of spines, while the mode of their production has been through spontaneous variation and Natural Selection.[2]

A few remarks may now be added on the general question of adaptation in the vegetable kingdom. Reference has already been made to the numerous cases in which the special adaptations of flowers to insect-fertilisation can by no stretch of imagination be imputed to the direct action of insects, and the same thing is equally clear in many other directions. The whole group of insectivorous plants, for instance, exhibit strange and complex adaptations which have no

[2] Professor A. Kerner gives an admirable account of the various forms of spiny and prickly plants, which are exceedingly numerous in the Mediterranean district, and he adds: "In northern regions not exposed to summer drought, where grazing animals find in summer enough green fodder, this form of plant is almost entirely absent." ("The Natural History of Plants," English Translation, vol. i., p. 445.)

direct relation to the mere fact of insects crawling over them or settling upon them. So also are those varied adaptations by which, as Kerner has shown, injurious insects are prevented from reaching the flowers.

Even more unintelligible on this theory are modifications of fruits and seeds, by which some attract birds or mammals to eat them, while others are guarded against being eaten; some seeds have beautiful wings or plumes for wind dispersal, others have hooks or sticky hairs which cling to wool or feathers, while others again are scattered abroad by the sudden elastic bursting of the capsules. Take the comparatively simple case of nuts. Did they acquire their hard covering and brown protective tints and detachment from the tree as soon as ripe by the direct agency of birds, or monkeys, or squirrels? Of course, the question is absurd, since those eaten by these creatures could not transmit their special qualities; but those that, by the possession of any of these qualities, escaped being eaten, would transmit those qualities to the next generation.[3]

Any conceivable direct action of the environment can therefore have produced only a very small portion of the modifications and adaptations that actually exist. In by far the larger number of cases no such explanation is possible, and no other adequate explanation has been suggested except variation and Natural Selection. It is, of course, admitted that the action of the environment does produce definite changes in all organisms, more especially in plants, but there is no evidence that such changes are transmitted to the offspring of the individuals in which they have been produced.

On the other hand, there is direct evidence that many such changes are not transmitted, an example of which is the *Arabis anachoretica* with remarkable tissue-papery leaves, due to its growth in hollows of the rock, where neither sun nor rain reach it. Seeds of this plant when cultivated at Kew produced the common *Arabis alpina*. The same thing occurs with many plants, as every cultivator knows; but other forms with no greater peculiarities externally preserve their characters under cultivation, though exposed to the most varied conditions. As we thus know that *some* variations directly due to the environment are *not* transmitted, and also know that an immense number of spontaneous or congenital variations *are* transmitted, since by taking advantage of this fact almost all the improvement in our domestic animals and cultivated plants has been effected; and yet further, that no case has been found in which such spontaneous variations are wholly intransmissible,—the logical conclusion is that the two kinds of variation are distinct in their nature. This view of the subject is adopted by those botanists who are now endeavouring to determine the true nature of the

[3] Other cases of the want of relation between adaptations and their supposed cause are given in my article, "Are Individually Acquired Characters Inherited?" in the *Fortnightly Review* of May, 1893, pp. 664–9.

numerous alleged species, sub-species, and varieties of our native plants. They test the fixity of the characters which distinguish each form by cultivation. If these characters remain unchanged, and are transmitted by seed, the form is a permanent one and deserves to be recorded as a species or sub-species; but if, as frequently occurs with forms which appear quite as distinct as those which are stable, the plant reverts on cultivation to some other form, it is evidently a modification due to some local conditions of the environment, and should be treated differently. Mr. Beeby has proposed to call the former "intrinsic," the latter "extrinsic" varieties, terms corresponding to Weismann's "germ variation" and "somatic variation," and these can in many cases only be distinguished from each other by the test of cultivation under different conditions. On this point Mr. Beeby remarks:—"The most transient states of plants due to the direct action of their environment are often far more distinct in appearance from their normal forms than are some varieties from their types; but the first-named return at once to their normal state on being removed from their special surroundings, while the latter remain permanently distinct from their types even when grown under circumstances most disadvantageous to the continuation of the particular variation. That these two kinds of variation exist in plants is certain; and the separation of them seems to be the very basis on which all investigations of the Phanerogamia must be made, if it is hoped that this branch of botany is to throw any further light on Evolution."[4]

In conclusion, I submit that the whole body of facts in relation to the direct action of the environment indicates that modifications thus produced in the individual are not transmitted to the offspring; and that until it is demonstrated by experiment that they are so transmitted, theories of plant modification founded on that assumption are altogether worthless.

ALFRED R. WALLACE.

[4] "On the Flora of Shetland." *Annals of Scottish Natural History*, January, 1892, p. 32.

II.

Hertwig's "Preformation or New Formation."[1]

Part II.—Criticism of Weismann's Theory of the Germ-plasm and Doctrine of Determinants.

ALTHOUGH Weismann has carried out his theory into the greatest detail, it is not with details so much as with the fundamental principles of it that Hertwig disagrees. It is with these principles, as they concern what is known about cells, that the chief difficulties seem to him to occur. No doubt, as Naegeli has said, there are many units of different orders above and below the cell, but the observation of the last thirty years has made it more and more certain that the cell is the most striking morphological and physiological unit of the body. Every theory of heredity must be tested by comparing it with facts observed about the cell. The cell, consisting of protoplasm and a nucleus, is an elementary organism, which, by itself or in combination with others, forms the basis of all organisms. It has an inconceivably complex structure, which, in its minuter parts, eludes our observation, and it is composed of many physical substances. Of these, albuminates, fats, carbohydrates, water, salts free or in solution, serve as material for the growth of the cell. Others form the living cell-body, in the narrow sense: by growth and division they can multiply and form the elementary unit of which the cell is a higher combination. In this category of intra-cellular units would be placed, if they existed, such theoretical structures as the gemmules of Darwin, the physiological units of Herbert Spencer, the bioplasts of Altmann, the pangenes of De Vries, the plasomes of Wiesner, the idioblasts of O. Hertwig, and the biophores of Weismann. The cell of each species has its specific organisation of a simpler or more complex structure, and contains a proportional number and variety of organised lower units. An unfailing organ of every cell is the nucleus, which is composed of elementary living units, the idioblasts, which differ chemically, morphologically, and physiologically from the units of the protoplasm, but may, perhaps, be able to turn into them. As a matter of observed fact, the

[1] Zeit- und Streitfragen der Biologie. By Professor Dr. Oscar Hertwig. Pamphlet I. Präeformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen. Pp. 144, with 4 illustrations in the text. Jena: Gustav Fischer, 1894. Price 3 marks.

nucleus is the bearer of the hereditary plasm, and may be inferred to be more stable than protoplasm and less subject to outer influences. It is the inherited plasm of the nucleus that stamps the specific character of the organism. The capacity of the cell to feed, grow, and divide is the mechanism by which the continuity of life is secured.

The cell plays a leading part in Weismann's theory. His germ-plasm is made up of primary units, the biophores, which are built up into determinants, a determinant existing for each independently varying cell or group of cells in the adult organism.

The arrangement of the biophores in the determinants, and of these in the ids or higher units, follow an elaborate historical architecture. This complicated germ-plasm lies in the nucleus, and nuclear division is the mechanism by which it passes from cell to cell. An individual life-history begins with a fertilised egg-cell containing paternal and maternal germ-plasm, which have come respectively in the nucleus of the ovum and in the head of the spermatozoon. In the actual course of ontogeny two things happen. By a series of nuclear divisions some of the germ-plasm is saved in an unaltered form, and ultimately gives rise to the sexual cells of the new individual. The structure of the rest of the germ-plasm in the course of another series of nuclear divisions gradually breaks down in such an orderly fashion, that the various determinants are marshalled to their proper places until ultimately the tissue cells of the various parts of the embryo and the adult, instead of containing a complicated plasm, contain only the determinants of their own order. Thus nerve-cells contain only the determinants of nerve-cells, muscle-cells of muscle-cells, and so forth. In certain special cases, which Weismann believes to be secondary adaptations, in addition to the specific determinants, cells may contain some other latent determinants, to provide for the regeneration of lost parts and so forth. But, with these exceptions, there is in every organism an impassable separation between the unaltered germ-plasm, which has been preserved to pass into the sex-cells, and the specialised simplified nuclear plasm of the various independently varying parts of the organism. Moreover, inasmuch as the unaltered germ-plasm of the sexual cells is continuous from generation to generation, while the part of it which breaks up into the nuclear matter of the various tissue constituents perishes with these at the death of the organism, Weismann distinguishes the one as the immortal germ-plasm, the other as the mortal somatic plasm.

Comparing this theory with actual knowledge of cells, Hertwig points out that it implies two quite different kinds of nuclear division. That kind of nuclear division by which the unaltered germ-plasm is handed on from cell to cell until it reaches the new sex-cells may be called "heirs-equal" division (erbgleiche Theilung), for in it the nuclear matter is distributed equally and unaltered between the two daughter-cells. That kind of nuclear division which occurs

while the various determinants are being marshalled to their places may be called "heirs-unequal" division (erbungleiche Theilung), for in each stage of it the daughter-cells receive different sets of determinants. Thus, if, as Weismann supposes, the first cleavage plane determines the longitudinal axis of the future body, the daughter-cell on the right would receive all the determinants for the right side of the body, the daughter-cell on the left all the determinants for the left side. This process of heirs-unequal division would be the characteristic method of nuclear division in all ontogenies, heirs-equal division occurring only in the few cases necessary to separate off the sexual cells. Later on in development, when the determinants have been completely sorted out, heirs-equal division would re-establish itself, for a determinate or independently variable part of the body generally would consist of a large number of cells resulting from the simple numerical multiplication of the cell or group of cells which, by heirs-unequal division, had received the complete set of determinants for the tissue in question.

Now Hertwig strikes at the root of all this process, and therefore at the root of the whole Weismannic theory of heredity, by saying that heirs-unequal division does not occur. Against it he brings up five groups of facts.

GROUPS OF FACTS AGAINST HEIRS-UNEQUAL DIVISION.

I.—*Unicellular Organisms.*

Among single-celled organisms, we know only of heirs-equal division. It is the means by which the species are perpetuated. Were the division a case of heirs-unequal, then there would result, not daughter-organisms of the same species, but daughter-organisms of new species, and all our evidence is against this happening. But although the nucleus divides equally, it is not necessary that the organisms of the new generation be at once like their parent. In many protozoa an ontogeny is gone through. The results of division, for instance, in the fixed Acinetan *Podophrya gemmipara* are free-swimming ciliated forms, which, after a vagrant period, settle down into the fixed adult form. Similarly, among Gregarines quite complicated larval phases are gone through. None the less, in these and similar cases, the exact adult form finally appears. From such occurrences the inference is plain. Because cells are unlike we must not conclude that their nuclear plasm is unlike. To use Weismann's phraseology, some of their determinants may remain latent for a time, and while the complete adult form will appear ultimately, in the meantime the cell may present appearances very different.

II.—*Lower Metazoa and Metaphyta.*

In the case of such organisms as the filamentous Algæ the filament is, so far as can be judged, produced by heirs-equal division. Yet any of the resulting cells, similar to all appearance, may turn into sexual cells. If, with Weismann, it is assumed that the distinction between

somatic and sexual cells, once it has arisen, is inviolable, then we are led to strange conclusions. For instance, in two forms so nearly allied as *Volvox globator* and *Pandorina morum*, we should have to assume that *Pandorina* had no soma, for all its cells can reproduce: while in *Volvox*, where certain cells are specialised as sexual cells, there is a soma.

III.—*Reproduction and Regeneration.*

A multitude of cases show that very many cells and tissues contain the possibility of reproduction. Thus, a plant like *Funaria hygrometrica* may be chopped in tiny pieces, and each piece, placed in damp earth, will reproduce the whole plant. Similar occurrences are familiar in cases like the willow, the begonia, or in many coelenterates, worms, and tunicates. It is making a large drain on the imagination to suppose that in so many widely-scattered cases a condition diametrically opposed to what Weismann regards as the normal condition of complete separation between soma and germ-plasma has arisen under special conditions.

IV.—*Heteromorphosis.*

Loeb uses this term to denote the power of organisms, under the stimulus of outer conditions, to produce organs on parts of the organism where they do not occur normally, or the power to replace lost parts by parts unsimilar to them. Regeneration is the reproduction of like parts. Heteromorphosis is the reproduction of unlike parts.

If one cuts off part of the stem of almost any plant, on placing the stem in suitable soil, roots will grow out, although roots are not natural to that part of the stem. The prothallus of ferns produces the male and female organs on the lower side turned away from the light. If a prothallus be darkened on the upper surface, and illumined by reflected light on the lower surface, then the antheridia and archegonia will be produced on the upper surface. Galls are produced under the stimulus of the insect almost anywhere on the surface of a plant. Yet in most cases these galls, in a sense grown at random on the surface of a plant, when placed in damp earth will give rise to a young plant. In the hydroid *Tubularia mesembryanthemum*, when the polyp-heads are cut off, new heads arise. But if both head and root be cut off, and the upper end be inserted in the mud, then from the original upper end not head-polyps but root filaments will arise, while from the original lower end not root filaments but head-polyps will grow. In *Ciona intestinalis* round a slit cut into the body-wall a tubular process grew out, forming a new mouth, while round the base of this, a series of eye-spots, corresponding to the eye-spots round the real mouth, appeared. In all these cases, it is plain that there were present in parts affected the determinants, to use Weismann's term, not only of the normal parts, but also of parts which, under normal conditions, would never have appeared there; and these new parts growing in

the unwonted places bore the normal species-stamp as characteristically as similar parts grown in their normal places. It can hardly be supposed that the architecture of the germ-plasm contains special determinants to be ready for occurrences so casual, especially as these are called into existence by circumstances quite foreign to the normal environment of the organisms. On the other hand, the facts are consonant with Hertwig's belief that, as all division is heirs-equal division, all the species-characters that depend upon cells are latent in every cell.

The experiments of Driesch, Wilson, and Hertwig upon the early stages of developing ova show that heteromorphosis begins with the very earliest divisions of the egg. Thus Driesch, working upon echinoderm embryos, was able to flatten out the stage where there was a sphere of sixteen cells into a flat plate where all the cells were in the same plane. In such a plate, the nuclei of the cells occupied relative positions very different from the normal conditions. Yet from these Driesch obtained normal plutei larvæ. It was, in fact, as if the cells could be pushed about like billiard balls without destroying the future shape and characters of the embryo. Did each cell contain only the determinants that would correspond to the structures that would arise from it under its normal conditions, then change of its normal position would have arrested development. Each cell must, on the other hand, have contained the determinants for all the animal, and have allowed those to come into operation that were required by the new positions into which the cells were forced. Driesch, by separating the first two and the first four segmentation-spheres of an *Echinus* ovum, obtained two or four normal plutei, respectively one-half and a quarter of the normal size. Here again each sphere must have contained all the determinants for the whole organism. Heirs-equal division must have occurred. So, also, in the case of *Amphioxus*, Wilson obtained a normal, but proportionately diminished, embryo with complete nervous system from a separated sphere of a two- or four- or eight-celled stage.

Hertwig himself, some years ago, published the results of experiments he made upon the development of frogs' eggs under abnormal conditions. He showed that there could be no question of imperative divisions separating the germ-plasm into right and left halves, and so forth, but that the method of division was determined by pressures and relative gravities. Alteration of these made the ova divide into novel but symmetrical forms. Chabry obtained normal embryos in cases where some of the segmentation-spheres had been artificially destroyed.

These cases all show that in its possibilities each segmentation-sphere is identical: that as a result of heirs-equal division, each cell contains all the material necessary to cause the development of a complete embryo. Weismann would have to suppose that in all these cases, in addition to its half of the nuclear matter resulting

from heirs-unequal division, it had also a stock of unaltered germ-plasm ready to be called into activity by unwonted stimuli. But even this hypothesis would not account for cells distorted by compression responding with the production of unwonted symmetries.

V.—*Vegetative Affinities.*

It will now be clear that Hertwig considers that all cells, in addition to their apparent characters, possess, in addition, all the characters of the species.

For instance, histologically considered, the similar tissues in a group of animals such as the mammalia, are, in most cases, not to be distinguished. No doubt an expert could, in some cases, distinguish through the microscope the red blood corpuscles of certain mammalian groups from each other; but he could not do so in the case of species, while with most tissues—connective tissues, nervous tissues, muscular tissues—it would be only in rare and extreme cases that he could distinguish one mammal from another.

But, in addition to their visible structure, which bears an obvious relation to their function, Hertwig attributes to each cell hidden constitutional or specific characters. This is the case plainly enough with the sexual cells. From indistinguishable ova and spermatozoa, there arise animals with different specific, generic or even family characters. But the facts of translation and transfusion for animals and grafting for plants, show that there is a parallel between tissue-cells and sex-cells so close as to suggest that tissue-cells are as much specific as sex-cells.

In plants, the operation of grafting is very easy to bring about; yet it is found that the approximated shoots will not always unite; if the cases where grafting succeeds and fails be considered, it is found that they are parallel to success or failure in sexual crossing. Near allies will unite, but failure follows the attempt to bring together shoots from different species. There are, it is true, some notable exceptions—cases where plants, considered closely allied, will not unite, and cases where plants, regarded as specifically separate, will unite. Grafting follows, not the apparent outward tokens of resemblance, but the vegetative affinities of the cells. It is their latent constitutional characters, and not their patent tissue characters, that are at work. It is upon the experiments of Voechting that most of these conclusions depend. He found in the case of the pear and the apple, which belong to the same genus, that grafts would not succeed, and in their case sexual crossing will not occur; but the pear and the quince do graft, although these belong to different genera. Hertwig thinks it probable that sexual affinity and vegetative affinity depend upon the same ground characters of the cell.

Voechting calls the grafting of twig and stem *harmonic* when a full individuality is reached, *disharmonic* when this is not the case. When the disharmony is complete the two either poison each other

or develop separately. When, however, an intermediate condition exists, growth together may go on for a time. Then, after a time, the twig grows rootlets for itself, which penetrate the stem of the other, and, trying to achieve its own individuality, becomes a parasite instead of a part of the other plant. Sometimes plants, between which disharmony exists, may be brought together by a third. Thus plant A, which will not grow upon B, may have a third plant C, such that when C is grafted upon the stem of A, then B can be grafted upon C.

Resembling the conditions of vegetative affinity are the conditions of sexual union. Thus, infertility is graded from being complete to partial. Sometimes only the first cleavage occurs; sometimes a gastrula and so forth, but death ensues from no outward cause.

Animal parallels, such as transplantation, are rarer. Trembley succeeded in cutting two specimens of *Hydra fusca* across and getting, in one case, the cut halves of the different animals to unite. But he could not graft upon *Hydra* part of another hydroid.

Ollier succeeded, after removing a part of the periosteum from one spot in an animal, in making it grow and become bony in another part of the same animal. But it was an evanescent success, for the new growth being a foreign object in its novel place, speedily became absorbed. In the case of transplanting from the dog to other animals like cat, rabbit, etc., either the new growth got absorbed, or it festered, or became enclosed in a cyst.

Paul Bert transplanted part of the tail of a young rat to another part of the body. There it grew, all the tissues but the nervous developing well. Transplantation of the tail to other animals was more difficult. Usually it festered, and nearly killed the animal. Sometimes it was resorbed; only in cases of very nearly-allied animals did it grow.

A. Schmitt recently found that pieces of skin transplanted to animals of another species invariably festered out, or were quietly resorbed.

In the case of transfusion of blood, failure always was the result of experiments between animals of different species. In large doses it was fatal; in small, harmful; and always, in a very few minutes, the transfused blood corpuscles began to degenerate, their hæmoglobin appearing in the plasma. Landois and others conclude that the results of transfusion correspond with the anatomical affinities of the animals experimented upon.

Hertwig concludes that the cells, in addition to their tissue characters, have species characters, and that as one can speak of the sexual affinities of the sex-cells, so one can of the vegetative affinities of the tissue-cells.

Before proceeding to give an account of Hertwig's further criticism of Weismann's theory of heredity, it is worth while to sum up the part of the argument already treated. Upon Weismann's

theory, the part of the germ-plasm required to direct an individual ontogeny gets broken up and distributed to the various cells of the embryo by a series of heirs-unequal divisions. By this means the cells of the soma come to contain only the determinants which can produce cells of their own order.' The exceptions are not numerous. There is, first, the great exception by which unaltered germ-plasm is passed along the germ-tracks to the place of origin of the sexual cells. Next there are a few special cases of adaptation to the needs of regeneration and bud formation in which a certain amount of reserve nuclear matter is retained. Against this Hertwig asserts that all cell-division is by heirs-equal division, and, therefore, every cell, in addition to its patent characters, retains the power of producing on emergency all the species-characters. He goes on such facts as these. In single-celled organisms all cell-division obviously is heirs-equal division. In the lower animals and plants, almost any cell, when occasion demands, may become a sexual cell. Regeneration of lost parts, and still more heteromorphosis, show that each cell is not limited in its capacity to its predestined sphere of work, but can reproduce totally distinct parts of the organism, and can reproduce them in such a way that the specific characters are maintained. The results of grafting, transplantation of tissue, and transfusion of blood, show that cells beside their patent tissue-characters contain latent species-characters, and that, generally speaking, the resemblance between sexual affinities and vegetative affinities shows that tissue cells are as equally specific as sexual cells.

Considerations Against the Doctrine of Determinants.

With his doctrine of heirs-unequal division, Weismann has united his theory of determinants in the germ-plasm of the fertilised egg-cell. Each independently varying part of the adult is represented by biophores united into a determinant. The various determinants are arranged in the plasm or are endowed with such powers that they come liberated at the proper time to enter into the cells to form the determinate. Hertwig's criticism of this, and his arguments against it, are strongly epigenetic. In the modern, as in the older, doctrines of preformation, he sees the same confusion between sequence and causation. If the ancestor of the whole chain of animal-life were an amœba, it would be an empty and meaningless phrase to say that the amœba contained in any real sense the material germ of all the subsequent animals that arise from it. Similarly, although all the cells of the body have their origin from the egg-cell, it is no necessary consequence that the material germs of all subsequent tissues and cells are contained in the egg-cell. The egg and the adult do not form a closed chain of forces. Many other conditions must be present.

Every organic development depends, for instance, upon the absorption and metamorphosis of nutritive materials. The cells grow

at the expense of material taken from the outside, and what in one stage is raw material becomes in the next the starting point of the next cell-generation. Thus food-yolk enters the cells, and, though the difference is only quantitative, produces a qualitative change in the embryo. If one thinks of the number of stages between the egg and the adult, and that each stage has had a large absorption and change of foreign material, it gets plain that it is an error to put into the egg all the incipia of adult structures, as many of these come into existence only at later stages of development.

A second error in the theory of determinants is to attribute to cells—as, for instance, to the ovum and spermatozoon—properties which are not cell properties, but those of combinations of cells. Of the characters of animals and plants, many are due to the co-operation of the whole organism, others to single organs or to groups of cells. If these are to have physical carriers in the germ, are the carriers biophores or determinants? Hertwig can admit for a cell only characters peculiar to cells. A sexual cell can contain physical incipia for chondrin, ossein, pigment, chlorophyll, nerve fibrillæ, but not for the formation of a definite spinal ganglion, a hair, or the biceps muscle. The incipia of these must be groups of cells.

The egg is an organism which, by feeding, growth, and division, can split up into exactly similar organisms, and it is first through the inter-relations of these new organisms that in each stage of development the combined organism makes its stages of growth. Criticising the pangenes of De Vries, Weismann says that there cannot be zebra-stripe pangenes, and so forth. There could be black and white pangenes, for these are cell properties; but the zebra stripe and many similar characters depend not on cells but on the arrangement of cells. Hertwig accuses him of falling into precisely the same error in his determinants. There cannot be in a cell the determinants of organs or structures which depend not on cell characters but upon the relations between cells.

Thus the human state depends upon the co-operation of many classes of men. Supposing, for the argument, all men to have come from a single pair, this pair, according to Weismann, would contain the determinants of the whole state. But the co-operation of individuals into groups, and of groups into the state, is something new which could not exist while there was only a single pair. Of course the subsequent results are based upon the nature of men, but in no mechanical fashion are they contained in the single pair. So far as causal relations exist between the egg and the organism, the parallel holds exactly. There can be no material particles present in the egg as the incipia of characters due to the co-operation of cells. As Naegeli says, to understand heredity we do not require a separate individual symbol for each difference caused by space, time, and property, but we require a substance which, by the linking of its limited elements, can represent every possible combination of differ-

ences, and through permutation can pass into other combinations. As Hertwig puts it, the hereditary material contained in the egg and spermatozoon can be made up only of those material particles which are the bearers of cell properties. Every composite organism can inherit its characters only in the form of cell characters. The numerous indefinitely variable properties of animals and plants which are expressed in the different form, structure, and function of their different organs and tissues, and in the different relations between them, are functions of cell complexes. They depend on the interaction of many cells, and cannot have as their hereditary bearers material particles in a cell. They are new structures which come into existence only through the combination of cells with different individual properties.

Take as an example of the impossibility of determinants, the early stages of the frog's development.

In cleavage the nucleus plays the chief part, but to start this there is needed not a special determinant, but the co-operation of all the functions of the cell through assimilation of nutriment from the yolk. The chromatin fibrils, which we can represent as independently growing and dividing elements, have to be doubled in number. The centrosome also must be independently doubled. All these processes are the result of chemical and other changes depending on the powers of the cell as an elementary organism, and extended to the chromosomes as units of a lower order. The division of the nucleus into two, four, eight, and so forth, gives the stimulus to the yolk-mass to divide, and this exercises a strong influence upon the arrangement of the cells and the direction of the planes of cleavage. But these are not due to special determinants, for many of them are due to the special properties of the yolk. Thus Hertwig himself has shown that the direction of the planes of cleavage depends upon the relations of the specific gravities of the various parts of the cell, and that the unequal size of the cells and the unequal rate at which they divide depend upon the separation of the protoplasm into parts richer and poorer in yolk granules. No doubt the first three cleavage planes often coincide with the future axes of the animal, and this is laid hold of by Roux and Weismann as proof of their view that these planes separate cells containing material for the right and left halves, and so forth. But these also Hertwig thinks he can show to result not from specific determinants, but really to depend upon the shape of the egg and the yolk distribution. Thus the shape of the egg itself in many cases determines the necessary shape of the adult. In the case of eggs, those with polar arrangement of yolk give rise to polar segmentations, those with equal distribution of yolk give rise to holoblastic division. In meroblastic eggs the richer or slighter assembling of yolk, and therefore of specific gravity, is the determinant of the special kind of embryo. In very many cases where the cleavages give the future axes, these are determined not by the

nucleus, but by the distribution of the yolk and by the shape of the embryo.

The formation of the blastula depends on several factors.

1. The cleavage into four, eight, and sixteen causes a loosening of the cells and the appearance of intercellular spaces.

2. As the cells multiply rapidly they naturally come to be pressed into an epithelial structure.

3. As the outer cells become pressed into an epithelium, the segregation of fluid and arching of the outside cause a central cavity to appear and grow larger.

Where is the room for special determinants? The division is an elementary property of all cells; the appearance of intercellular spaces is a result of cellular and external forces as the nucleus forms an attraction centre round which the yolk tends to arrange itself equally.

Gastrula formation is a result of the co-operation of all the cells, of the inequality in curvature, and of a series of external circumstances. To attribute this to determinants is to turn things round. It is not because certain cells contain certain elements that they become epithelial, digestive and so forth; but it is because, by mechanical and other reasons they come to lie in special spaces, that the corresponding side of their character gets developed.

Hertwig's leading objections to Weismannism are then, first, that he thinks the evidence is in favour of cell-division being heirs-equal division, and that, in consequence, the germ-plasm is not separate from the soma, but all the cells in an organism contain the cell-characters necessary to produce a complete organism. Next, a large number of the characters of organisms depend upon combinations of cells, and so cannot have determinants in a single cell. His view is epigenetic in a double way. Each cell contains all the cell-characters of the organism: the special characters which become active are educed by its environment. Characters which depend upon the co-operation of cells can come into existence only by the combination of cell-characters after these have been found. In the next number of NATURAL SCIENCE I hope to give an account of the concluding portion of Hertwig's pamphlet.

P. CHALMERS MITCHELL.

III.

The Parasites of Malarial Fevers.[1]

NO one to whose lot it may fall to have to travel alone in tropical countries can afford to neglect the study of the various diseases there prevalent. This is necessary not only for self-protection and self-cure, but for sake of the influence that may be thus acquired over the natives by a rapid cure or sudden soothing of pain. Of all the diseases which it is necessary for such a one to consider, there are none more important than those of that puzzling series grouped together as malaria. For long it was thought that many ailments included under this name had no connection, for their causes were quite unknown. During the last fifteen years, however, great advances in the knowledge of the subject have been made. England has taken but a very small share in this, as the disease is not one often met with in ordinary British practice. It is true that in the lower parts of the eastern counties there is a good deal of a malarial ague. A "West Coast" captain recently remarked that the malaria on parts of the Essex coast is as bad as it is anywhere in Africa; but this need not be taken seriously, and only serves as a further illustration of the fact that a West Coast captain will say anything. In the tropics, on the other hand, the disease is one of the most important, but the conditions for its study are not as favourable as might be wished. In Italy, however, both the disease and skilled investigators occur together. There malaria is so prevalent and assumes such a pernicious type, that it has been a matter of study since the earliest days of medical science. Thus, even in the time of Hippocrates, so closely had it been watched that the fevers were then divided into two classes.

The literature of recent work upon the subject is very scattered, but a general sketch of the whole subject has been given in two monographs, of which a translation has been issued by the New Sydenham Society as the 150th volume in its series. These treat the subject from somewhat different points of view. The first is rather

[1] Two Monographs on Malaria and the Parasites of Malarial Fevers. New Sydenham Society. 8vo. Pp. xxvi. and 428, six plates, three charts. London, 1894. I.—"On Summer-Autumn Malarial Fevers." By E. Marchiafava and A. Bignami. Translated from the Italian by J. Harry Thompson. II.—"The Malarial Parasites." A description based upon observations made by the author and by other observers. By Julius Mannaberg. Translated by R. W. Felkin.

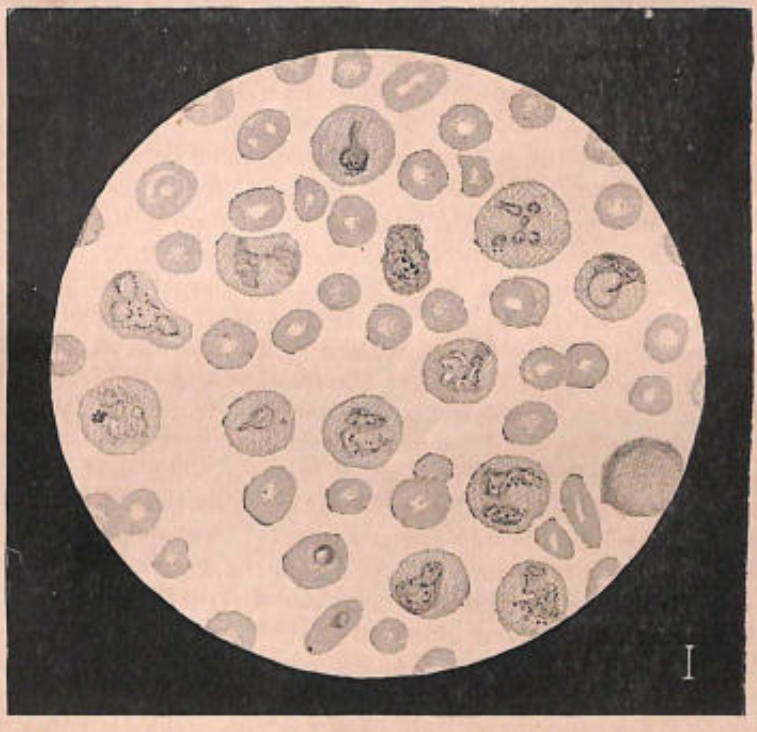
medical in its treatment; it is a study of the worse group of the two groups of fevers of the Roman Campagna: it is entitled "On Summer-Autumn Malarial Fevers," and is by Professor E. Marchiafava and Dr. A Bignami. The second, mainly biological, is on "The Malarial Parasites," and is the work of Dr. Mannaberg, of Vienna. The former is admirably translated by Dr. Thompson, of Washington, who has added an interesting preface of his own: not so much can be said for the translation of the second part of the volume, where misprints, errors in translation, and unintelligible sentences detract from the value of the work.

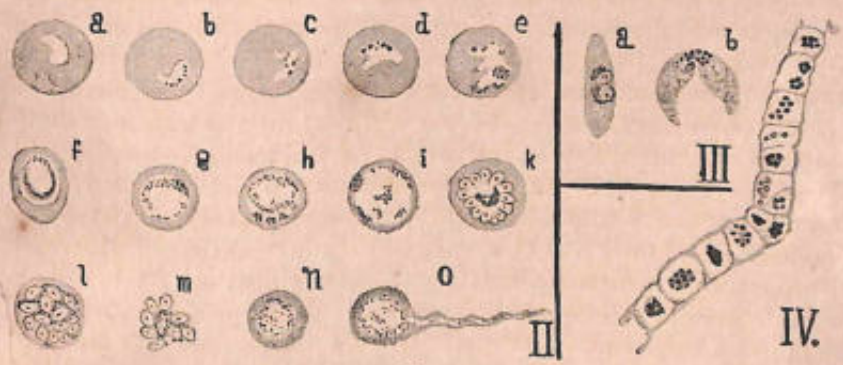
The study of the subject has an important bearing on some biological questions. It may be of interest, therefore, to attempt briefly, in non-medical terminology, to summarise some of the results for the benefit of general students of biology.

The dawn of modern work on the subject began with Heinrich Meckel's (1) discovery in 1847 of pigment in the blood of a dead patient who had suffered from malaria. The significance of this, however, was not seen till, in 1879, Marchiafava and Celli began their brilliant course of investigations into malaria, by proving that this black pigment or "melanin" was formed within the red blood corpuscle from the hæmoglobin as a product of disease. The next year the great modern impetus to the study was given by the announcement by A. Laveran (2) that he had seen a flagellate organism in the blood of a patient suffering from malaria, which he maintained was the cause of the disease. From that time the study of the subject has made great strides. Laveran appears to have rather jumped at conclusions, and these were therefore not accepted at the time by many of the most careful investigators; thus Marchiafava and Celli dismissed these parasites as merely the products of the decomposition of the blood corpuscles. Nevertheless, the truth of Laveran's main conclusion has been verified, and it is now undoubted that the various types of malarial fevers are due to organisms in the blood.

As in our English ague, the first and most striking feature in these malarial diseases is their recurrence at certain regular intervals. In the old days this could not be properly explained, but it is now known to be due to the fact that the malarial parasites pass through a regular life-history. If the blood of a malarial patient be examined with suitable precautions, with the best type of modern instruments, it will be found to present somewhat the appearance shown in Fig. I., which is magnified a thousand diameters. There it will be seen that many of the blood corpuscles contain foreign parasitic bodies: these are either small and rounded, and occupy only about a tenth of the corpuscle, as shown in one at the lower margin of the figure. These are immature parasites, and their development may be illustrated by the series shown in Fig. II. Here the round discs represent the blood corpuscles: in *a* we see the parasite in its most primitive condition, living inside the corpuscle. It feeds upon the material of its host, and secretes the

red colouring matter or hæmoglobin which it takes in, as small scattered grains of a black pigment or melanin: further stages show the growth of the parasite and increase in the amount of pigment, until in Fig. *i* the whole corpuscle is occupied by the parasite. Nutrition is thus stopped and spore formation commences. In *l* this process is com-





PARASITES OF MALARIAL FEVER.
(For explanation, see end of Dr. Gregory's Paper.)

plete and the next figure shows the free spores. The last two figures of the series show a less normal development that sometimes takes place: here the whole parasite is set free by the rupture of the blood corpuscle. The particular species illustrated in Fig. II. runs its life-cycle in three days, and therefore produces the so-called quartan

fever in which, after one paroxysm, the next occurs on the fourth day. If the blood of a patient be examined on the first day in a case where the fever has a single origin, then only such forms as those represented in Figs. *a*—*c* will be seen; on the second day will be found the forms with the larger hæmatozoa—as the parasites may be called—such as those shown in *h* and *i*. During this period the patient does not suffer fever: if the disease be of the intermittent type the temperature will be normal; if remittent, it will be a little above this point. Then, if the blood be watched, the spore formation (Fig. *k*) is seen to commence, and the temperature of the patient rises, probably by the secretion of some poisonous product into the blood. The paroxysm of the fever, during which the temperature may rise to 106° without serious danger, is always approximately synchronous with the setting free of a fresh generation of spores.

Numerous deviations from this occur: thus some fevers are quotidian and the hæmatozoa complete their life-cycle in one day, and others are tertian with a 48 hours' cycle. Moreover, in any type, many of the hæmatozoa do not complete their normal development. The most important series of irregularities, however, is due to the presence in the blood at the same time of more than one generation, or more than one species of parasite. This is seen in Fig. I. Thus in the lower part of the figure is a corpuscle with a hæmatozoon in the earliest stage, while scattered over the field are several fully developed and two which are undergoing spore formation. Owing to the presence of this latter series the patient is either in or very near to a paroxysm of fever. Twenty-four hours later the younger generation will have reached the spore-forming stage, and thus another paroxysm of fever will be brought about. Therefore, though the life-cycle of this parasite takes two days for its completion, the patient will have a daily attack of fever, as the two generations alternately reach the spore-forming stage.

Considerable difficulty has thus been introduced into the study of these malarial diseases, owing to the complications caused by the simultaneous presence of two or more generations of the same species or even of altogether different species.

This raises the question as to whether the various hæmatozoa are definite species or only forms of one or few species modified by different conditions. This has given rise to great difference of opinion. Laveran and his school maintain that there is but one species and that all the varying forms are due only to polymorphism; thus Laveran sums it up in his great monograph (3), "Du paludisme et de son hématozoaire," issued in 1891, that "Le parasite est unique, mais son évolution est variable." The Italian school, however, and Dr. Mannaberg agrees with them, maintain the plurality of the species; Grassi and Feletti defined two genera, *Laverania* and *Hæmamœba*, and numerous species, and these are now generally

accepted. Mannaberg gives an interesting summary of the discussion and of the observations and experiments whereby it has been proved that each fever is the result of its own special parasite, and that by no known means can one species be changed into another.

Now that the life-history of these parasites has been worked out their true zoological position becomes a matter of interest. Laveran at first assigned them to the vegetable kingdom under the name of *Oscillaria malariæ*; but this view was promptly abandoned by its author, and he renamed the species *Hæmatozoon malariæ*. Numerous other names have been suggested, such as *Hæmatophyllum*, *Hæmatomonas*, *Cystosporon*, *Hæmogregarina*, *Hæmoproteus*, *Polymitus*, etc., but the name most generally accepted is *Plasmodium*, by far the most inappropriate. Medical men usually refer to the parasites as "malarial plasmodia." It need hardly be pointed out to biologists how utterly inappropriate this name is; a plasmodium is an irregular mass of protoplasm *containing many nuclei*. The figures, reproduced here, show that these bodies have but a single nucleus. As Laveran's name, *Hæmatozoon*, was not defined with sufficient precision to allow of its adoption in zoology as a generic term, Mannaberg wisely proposes to use it as a general name for the parasites.

In regard to the class to which they must be referred, numerous opinions have been held: of these several merit notice. Metschnikoff assigned them to the Sporozoa, and placed them in the same group as the genus *Coccidium*. Antolisei, however, referred them to the Gymnomyxa, while Grassi and Feletti considered them to belong to the subclass *Amœbeæ* of the class Rhizopoda. Kruse, again, referred them to the Sporozoa, and to the class of Gregarines. Danilewsky (4) finally, in agreement with the most generally received opinion, placed them in the Sporozoa, but established for them a new class, the Hæmosporidia. Mannaberg, after a careful discussion of the subject and comparison of the characters of the parasites with those of the allied forms of Protozoa, agrees with Danilewsky. He divides the Sporozoa into five subclasses, the Coccidia, Gregarina, Myxosporidia, Sarcosporidia, and the new subclass of Hæmatosporidia.

The results of these researches are not only of interest biologically but of great practical value, for with a knowledge of the cause, it is possible to apply remedies with greater certainty than of old. One of the most interesting sections in each of the two monographs is that dealing with the action of quinine. This has long been known as the most efficaceous of all medicines, but its power has been assigned to very different methods of action, as, for example, to a stimulation of the nervous system. Binz (5), however, in 1867, laid the foundation of a sounder view by showing that, if Infusoria be subjected to the action of quinine, they are killed, and, further, that this is due to asphyxiation: the alkaloid destroys the power of the Infusoria to take up oxygen. The quinine acts on the Hæmatozoa

in the same way; it therefore acts only when the parasites are passing through the stages of nutrition and development. When all the hæmoglobin of the corpuscle which acts as the host has been converted into melanin, the parasite ceases to grow, but divides into spores for reproductive purposes, and quinine becomes powerless. It is therefore necessary to take quinine in the intervals between attacks of fever, and useless to do so when the pyrexia is at its worst. This was long ago determined empirically by tropical practitioners, who also understand that quinine really does as much harm as good if taken at the wrong time. The reason for this also is now explained, for Binz has also shown that quinine has an unfavourable action on the leucocytes, or white corpuscles, which are thus checked instead of being aided in their struggle with the disease. Thus an unintelligent use of this reagent may be attended with very unfavourable results. This is likely to happen as many of these malarial parasites are not checked by quinine: this is the case with those which cause malarial fever in birds, and with some forms of human parasites. For instance, those shown in Fig. III., and known as the "crescentic bodies of Laveran," are quite uninfluenced by this reagent. If microscopic examination be neglected, and quinine administered in strong doses under the idea that the fever is produced by the species illustrated in Fig. I., serious injury may result.

The researches embodied in these two monographs are of the highest interest, both to the biologist, from the detailed investigation of the life-history of these low Sporozoa, and to the doctor, from their guidance in the proper treatment of a very troublesome and obscure group of diseases.

EXPLANATION OF FIGURES.

I.—Blood infested by malarial parasites (after Mannaberg), magnified 1,000 diameters. It shows (1) normal red blood corpuscles (the round discs with white centres); (2) a normal white corpuscle or leucocyte (the large dark circular mass touching the lower right hand border of the figure); (3) red corpuscles infested by the parasite in its youngest stage (two shown one above the other, in the middle line, just above the lower margin of the figure); (4) red corpuscles almost fully developed (the large discs containing shaded bodies); (5) parasites in which spore formation has commenced (one occurs in the upper part of the figure, a little to the right of the middle line; it is a dark body, roughly oval in form.)

The hæmatozoa belong to a tertian form. Thus, as spore formation is just beginning (as shown by the large size of most of the older generation of parasites, and that in two spore formation is fairly advanced), the patient is approaching an immediate paroxysm of fever. As a second generation is also present this will cause another attack of fever next day, so the patient is suffering from a "double tertian fever." Quinine if taken now would kill the parasites of the second generation, have no influence on the older generation, and check the efforts of the white corpuscles and of the phagocytic cells in the spleen to eradicate the disease.

II.—*a–o*, the life-cycle of the hæmatozoa of a quartan fever. (After Marchiafava and Bignami). In *a–i* the parasites are shown in red blood corpuscles. In *a* the parasite is in its simplest unpigmented condition; *b–i* show the gradual

growth of the parasite and increase in the amount of pigment; *k* and *l*, spore formation; *m*, free spores; *n* and *o*, free pigmented degenerate forms.

III.—The "Crescentic bodies of Laveran." (After Mannaberg). Magnified 1,000 diameters. In *b* transverse segmentation is shown.

IV.—Capillary vessel from the brain of a patient dead from malaria comatosa; the capillary is choked by spore-forming bodies.

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J. W. GREGORY.

IV.

A British Palm.

THE mention of Palms carries us, in imagination, to a warmer climate than our own, as we figure the tall, stately, unbranched stem crowned by a few large spreading leaves. But all are not lofty; in some cases the trunk is very short, or there is only a stout underground stem terminating at the surface of the soil in a leafy crown; while a *Thrinax* recently discovered at Anguilla, in the West Indies, generally reaches only about a foot in height—a specimen 2 ft. 8 in. being described as quite tall—and a dwarf species of *Phœnix*, the genus to which the date-palm belongs, discovered shortly before in the East Indies, was likewise only about 25–30 in., forming, with its slender stem, a very attractive little plant.

Others are not even self-supporting, but climb or scramble over surrounding vegetation. Such are many of the tribe Calameæ, which includes the Rattans or Cane-palms (*Calamus*), with a very long slender stem, stated by Blume, in his "Rumphia" (vol. ii., p. 158), on the authority of Rumphius himself, to reach a length of 1,200 to 1,800 feet. This statement has not, however, been verified, though 300 feet is said to be a common length in Ceylon and the Malay archipelago.

The leaves of the scramblers often terminate in long appendages or flagellæ, armed with stout recurved hooks, which may be extremely formidable and dangerous, though of great service to the plant as holdfasts. In one of these, by the way, *Korthalsia scaphigera*, from the Malay peninsula, the stipules of the leaf are united and swollen, forming an oval, hollow, smooth-walled chamber, of which ants take possession as a home.

According to the "Genera Plantarum" of Bentham and Hooker, there are about eleven thousand species, many imperfectly known; while many more, without doubt, remain to be discovered in Africa, Central America, Madagascar, and the islands of the Pacific Ocean. The family is essentially a tropical one, the greater number being American, a few Asiatic and Australian, and very few African. In America, a few small genera are peculiar to the Southern United States, such as *Sereneæa*, dedicated to the late Sereno Watson by Sir J. Hooker, and found in Florida and Carolina; *Washingtonia*, inhabitating South California and Arizona; and

Erythea, peculiar to the former. In South America, *Jubæa*, a Chilian genus, reaches as far south as the 37th parallel of latitude. The only species, *Jubæa spectabilis*, is popularly known as the Chili Coco-nut, and its fruits, looking like miniature coco-nuts an inch and a quarter long, are sometimes sold in this country. In the Western Hemisphere, the southern limit is 44° south latitude in New Zealand, the extreme form being a small endemic palm, classed originally as an *Areca* (one species of which, *A. catechu*, yields the well-known Betel-nut) but now separated, together with a closely-allied plant from Norfolk Island, as a distinct genus, *Rhopalostylis*. North of the Equator the monotypic (*i.e.*, containing only one species) genus *Nannorhops* lives on the mountains of India, beyond the Indus, and extends through Afghanistan and Beluchistan to S.E. Persia. The only native of Europe is a species of *Chamærops*, a small genus of dwarf palms confined to the Mediterranean region. *Chamærops humilis* is found in the south of Spain and Portugal and on the Mediterranean seaboard, reaching its northern limit at Nice at a latitude of 43° 44'. It is generally quite a dwarf, only three or four feet in height, and sends up numerous suckers in dense tufts. It may, however, form a trunk twenty or thirty feet high. Nyman, in his "Conspectus Floræ Europæ," speaks of it as the lowly and degenerate representative of a noble family afforded by the European flora, but reminds us that in the extreme south a loftier palm has found a home, viz., *Phœnix dactylifera*, the date palm. Beyond 38° or 39° latitude the date becomes merely an ornamental tree, as its fruit no longer matures, remaining more or less acid to the taste. It is cultivated in great quantity over the whole of North Africa, and more sparingly in Western Asia and Southern Europe. Like wheat, the sugar-cane, and many of our ancient useful plants, it is now never found in a wild state, and its original home is unknown.

Of course, pampered individuals in favoured spots will thrive in still more northerly latitudes. The Chusan palm (*Trachycarpus excelsa*) is sufficiently hardy to stand the winter in the north of France and even in the Isle of Wight. It has, however, a protection from the cold in the form of a coarse hair resulting from the decomposed bases of the leaf-stalks. The ordinary ratepayer may become acquainted with the family in the London parks during the summer months, where, by the way, I recently heard one described as "the kind of tree of which you see pictures with missionaries sitting under." It was labelled, incorrectly, *Seaforthia Cunninghami* in small letters and low down. "An Australian Palm" in larger print would have conveyed more information to the author of the remark.

The one hundred and thirty-two recognised genera of palms are somewhat restricted in their distribution. The *Raphia*, indeed, is a native of both Old and New Worlds, having several species in tropical Africa and Madagascar, and one in America, from the mouth

of the Amazon as far north as Nicaragua. Several of the tropical Asiatic genera, like *Areca*, run down through New Guinea into Australia. Others are extremely local. Lord Howe's Island has, for instance, two peculiar genera. *Juania* is found only on the small island of Juan Fernandez, while the small group of the Seychelles, in the Indian Ocean, boasts no less than five endemic genera. One of these, *Lodoicea*, bears the fruits known as the double coco-nut, or *coco de mer*. The great woody nuts were found floating in the sea long before the discovery of the tree which produced them or the islands on which it grew, and many absurd stories of their origin were in vogue among the earlier navigators.

Of somewhat restricted distribution, as well as of doubtful relationship, is the *Nipa* palm. Only one species is known, *Nipa fruticans*, which frequents the brackish estuaries and littoral marshes of tropical Asia from Ceylon to the mouth of the Ganges, through the Malayan Archipelago to the Philippine Islands, New Guinea, and tropical Australia. It is of dwarf habit, having a long, thick horizontal root-stock which sends roots into the ground and bears, at the apex, a number of long deeply-cut pinnate leaves. The male and female flowers are borne separately on a spadix or long fleshy stalk springing from among the leaves. The male are thickly crowded on catkin-like branches, the female borne in a rounded head terminating the spadix. The head of ripe fruits is as large or larger than that of a man. The individual fruits vary somewhat in size and shape, according to their position in the head, but are all more or less inversely egg-shaped, the upper-third projecting and rounded with generally an apical umbo, the lower portion pressed on all sides by neighbouring fruits and more or less angular, showing four to six obtuse angles. The coat is of a deep brown colour and polished, while the substance is thick and fleshy but densely fibrous. It contains a single seed with a small basal embryo and a quantity of cartilaginous albumen, which, however, is edible before the seed is quite ripe. The seed is said to germinate within the fruit, which remains on the spadix for several years, by which time the embryo has so far advanced in growth as to be unaffected by salt water. I think, however, that this statement requires confirmation. The fruit finally drops into the brackish water or mud and the seedling may grow where it falls, or float some distance. Great numbers of the fruits are often found floating in the river estuaries. Sir Joseph Hooker, in his delightful "Himalayan Journals," refers to the frequency with which they were tossed up by the paddles of his steamer in the Sunderbunds.

In the "Philosophical Transactions" of the Royal Society for 1757, Dr. James Parsons gave an account of some fossil fruits and other bodies found in the London Clay, in the island of Sheppey, by Edward Jacob, a surgeon of Faversham, who also subsequently published an account of them as an appendix to his "Plantæ Favershamienses"

in 1777. Of two of the fruits, figs. 1 and 3 on plate xv. of the Royal Society paper, Parsons says they seem to be figs petrified when hard and green. In 1785, James Douglas, in a work entitled "A Dissertation on the Antiquity of the Earth," refers to the Sheppey fruits, one of which he makes out to be "a species of almond," but somewhat qualifies his determination when he further characterises it as "remarkable in the opinion of the most intelligent persons, as bearing no analogy in size to any recent specimens of this nut discoverable in any quarter of the globe." This particular specimen was the property of Sir Joseph Banks, and is probably identical with one of the two figured nearly twenty years later by James Parkinson, a London surgeon, the author of "Organic Remains." By this time the fossils, through Sir Joseph Banks and Mr. Douglas, had become the property of the British Museum. In the "Organic Remains," the green figs and the species of almond become allied to the coco-nut, Parkinson suggesting that they may, perhaps, be referred to the genus *Cocos*.

A year before the publication of Douglas's "Dissertation," Burtin, in his "Oryctographie de Bruxelles," had already described as coco-nuts some large fossil fruits found in beds of Middle Eocene age at Woluwe near Brussels. Others have since been brought from the same locality, and may be seen along with the Sheppey fossils in the geological department at the Natural History Museum; they are as large as coco-nuts, but broader and more egg-shaped.

None of these fruits received specific names till 1828, when Adolphe Brongniart, in his "Prodrome," accepting Parkinson's generic determination, dedicated the Belgian ones to Burtin, as *Cocos Burtini*, and those from Sheppey to Parkinson, as *C. Parkinsonis*.

In 1840, James Bowerbank, who had accumulated an enormous number of fossil plants from Sheppey, brought out the first and only part of his "History of the Fossil Fruits and Seeds of the London Clay." The first portion of this book is devoted to a discussion of the affinities of the particular specimens above-mentioned and others more or less resembling them. From a careful study of a large number, Bowerbank was led to reject the idea of a close alliance with the coco-nut, and also the name *Pandanocarpum*, applied to some of them by Brongniart, to indicate what he considered to be their great analogy with the fruits of *Pandanus*, the Screw-pine. As Bowerbank points out, the fruits are never aggregated into portions containing several seeds as occurs in *Pandanus*, while on the other hand they closely resemble those of *Nipa fruticans*. In both cases we find the thin outer skin, or epicarp, the fibrous central portion, or mesocarp, and the somewhat indurated internal endocarp, protecting the single seed. Each is in fact a drupe constructed on the same principle as the plum or almond. The fossils never show any indication of the presence of three carpels, either by three pores as in the modern coco-nut or otherwise. To indicate their affinity with the modern *Nipa*, Bowerbank founded the genus *Nipadites* and went

on to describe no less than thirteen species. So close is the resemblance to *Nipa*, that though widely separated from its present area of distribution, there is little doubt but that the Sheppey fruits belong to that genus of Palms. In fact, I was greatly tempted in a recent monograph, which the Linnean Society did me the honour to publish in their Journal, to follow Ettingshausen, who makes them all species of *Nipa*. The suffix "ites" has, however, been much used in palæontological work, and as there must always be some uncertainty in the specific diagnosis of fossils, based, as it often is, on material which would scarcely be considered sufficient in the case of recent plants, I have retained the form in which it was left by Bowerbank.

As often happens when a man makes a special study of small groups of plants, Bowerbank made too many species out of his fossil palm fruits. Some of these are founded on differences which may be merely individual variations or depend on the degree of maturity of the fruit or its position in the head. It is important to remember that, densely packed as they are in a sphere, there is not only considerable variety in the number of sides to the fruit and the development of the angles, but as many never ripen, all stages of abortion may be found, especially near the base of the head, compressed and distorted by the growth of the ripening drupes. There is every reason to believe that the fossil fruits were borne in a similar way. A comparison of some of Bowerbank's species with a number of immature or aborted fruits, taken from a head kindly sent by Mr. Ridley from Singapore, showed that more than one might be thus accounted for. The thirteen can be in this way reduced to six or at the most seven. One or more of these have also been found near Bournemouth and in the Isle of Wight. Three years ago Mr. Clement Reid, of the Geological Survey, made an interesting discovery on the Sussex coast in the Bracklesham Beds, at Bracklesham and West Wittering. In portions of the beach bared by the effect of a recent gale, he saw embedded a number of large fruits which we found on closer investigation to be *Nipadites* of a much larger size than any hitherto found in this country, and quite equalling those already referred to from the Belgian beds. Mr. Reid has since come upon similar specimens at Hengistbury Head, near Christchurch, Hants. The mode of preservation of these differs from that of the London Clay and Belgian fruits, for while the latter have been rendered hard and solid through the infiltration of mineral matter, our recent finds consist merely of a carbonised shell filled with sand, which rapidly crumbles on removal from the damp sea beach, while the carbon film cracks and peels. Hence it is impossible to keep them for any length of time. One, in a fair state of preservation, is now in the Jermyn Street Museum, and another in the British Museum at South Kensington. Fruits almost precisely similar to these have been found in Italy, near Breonio, in Verona.

Though described under a different name, *Palæokeura*, I have no doubt as to their identity with *Nipadites*.

The conditions under which the fossils are found show that, like their modern representative on the shores of tropical Asia, the palms from which they fell grew in the rich brackish mud of the shore or estuary. Associated with the Sheppey fruits are remains of crabs, fresh-water turtles, and a great variety of fruits and seeds, with stems and branches; such a collection, in fact, as characterises the delta of a large river. The great diversity of forms, and the water-worn condition of many of the specimens, indicate a collection brought together from a large area. The Belgian and Italian fruits must have floated some time, as they are often much bored by the teredo, and the associated fossils have a somewhat more marine character than those from the London Clay. On the other hand, the appearance of the fruits which we saw on the Sussex coast gave the idea that they might have grown close by, so uniform was their size and condition. The absence of teredo-borings and water-wear favours this view, but this being the case, we should almost expect to find traces of the stem or leaves of the palm; these, however, have not yet been found in any locality. Water-worn drift-wood, showing coniferous and dicotyledonous structure, occurred along with the West Wittering specimens.

From the general facies of the flora and fauna in the different beds in which *Nipadites* are found, there is ample evidence, as Bowerbank, Ettingshausen, and Schimper have demonstrated, of the existence of a climate much warmer than the present, and approaching a tropical one, or at least subtropical in character.

A. B. Rendle.

V.

The Bird's Foot.

I.—By Frederic A. Lucas.

I TRUST that the author of the note on the bird's foot, Natural Science for July, page 10, will not consider me "a captious critic" for saying that the statement that "in the great majority of birds. . . . the tendon of the flexor of the great toe is connected by a tendinous slip with the common deep tendon of the other toes" may be a little misleading.

Regarding the groups of birds in a general way, the statement is true, but so far as mere numbers go, the Passeres and Humming-birds, which are schizopelmous (*i.e.*, with the tendon to the great toe unconnected with the common deep tendon), probably contain a majority of species.

Now in regard to the presence of the flexor longus hallucis in three-toed birds, it seems to me that we can bring additional evidence to sustain the proposition that it remains not as a mere rudiment, but because it is functional. In certain lizards—possibly in the majority, or even all—the long flexors of the foot unite beneath the ankle and are worked by a single muscle; and did we know of any bird in which there was no separate muscle for the flexor longus hallucis, this bird would be considered as presenting the primitive arrangement of the tendons.

In the common Chimney Swift of North America (*Chætura pelasgia*) we have a comparatively simple condition of affairs. The deep tendons are united for a considerable distance, and the tendons to III, 2, and IV, 3,[1] arise from a common slip. There is another tendon to II, 1, the total number of separate long tendons thus being four. I, 1, is worked by a separate muscle and tendon arising from the upper inner portion of the tarso-metatarsus, this being present also in other Swifts, in Humming-birds, and in such Passeres as I have dissected.

In another and more typical Swift, *Micropus melanoleucus*, there are separate tendons to II, 2, III, 2, and IV, 2, the deep flexors being, as before, firmly united. In *Trochilus* we have a somewhat intermediate

[1] Roman numerals represent digits; Arabic, phalanges.

condition, the deep flexors being independent of one another, while III, 2, and IV, 3, are, as in *Chætura*,[1] forks of a common tendon, separate tendons running to I, 1, and II, 1. Finally, in a number of passerine birds we have, besides the separate deep flexors, the following tendons, II, 1, II, 2, III, 1, III, 2, and IV, 1, seven long tendons in all. Concomitant with this there are other steps towards increase in what we may call tendinal individuality, for while in the Swifts the tendons playin a groove[2] at the upper end of the tarso-metatarsus, in some, though not in all, Passeres, there are six distinct foramina for the accommodation of the seven tendons.

Now the bearing of these facts on the case under consideration seems to me this: There is an increase in the number and freedom of tendons as we proceed from the lower to the higher birds, and if three-toed birds retain the flexor longus hallucis it is because they stand low in the avian scale and had lost their fourth toe before its tendon became differentiated from the others.

Right here is a point I should like to see explained. Why has *Chauna*—and *Palamedea* also—a thumb and no flexor? Has it lost the flexor through disuse, or has it, which it has no right to do, grown a thumb after having once discarded that digit?

Turning back to Mr. Finn's interesting paper in the June number, there are one or two points which invite a few additional words. The great objection to Darwin's remark that "it might be of advantage to a penguin-like bird first to flap along the surface of the water like the Loggerhead Duck (*Micropterus*) and ultimately rise in the air" is that, so far as I am aware, birds which take to water have their flight impaired, and that the transition is from air to water, and not from water to air. The Loggerhead Duck is said to fly when young and do its flopping later in life. The auks have had their wings much reduced in order to use them in a medium for which they were not originally intended, while the great auk lost his power of flight, and eventually his life, by not taking sufficient aërial exercise.

Lastly, is the foot with all toes turned forward a case of reversion for adaptive purposes, or is it a case of survival? While the supposition that, if birds are descended from reptiles, these reptiles were arboreal in their habits seems to the writer as very plausible, it is also fair to assume that there was a time when all four toes pointed forward,[3] and why may not the cosmopolitan Swifts have retained their toes as they were originally? Opposed to this, however, is the highly modified condition of the phalanges, and the fact that in *Micropus* the tendons are a little more specialised than in *Chætura*, which has the

[2] *Macropteryx* has a single tendinal foramen, but the members of the genus *Macropteryx*, though Swifts, belong, to the best of my knowledge and belief, to a distinct family.

[3] In support of this assumption is the fact that in the embryo the four toes point forward.

foot of an ordinary perching bird; so for the present we may let the question rest.

II.—By Frank Finn.

There are some points in Mr. Lucas's interesting paper on this subject on which I should like to offer a few remarks.

In the first place, Mr. Lucas considers that the deterioration of flight in birds which take to water is the greatest objection to the derivation of flying birds from aquatic ancestors. This would indeed be a powerful argument were such deterioration universal; but it is not. A surface-swimming bird may on the one hand take to diving, and then is pretty certain to deteriorate in flying power. The Anatidæ form a good example of this, the semi-terrestrial Sheldrakes having larger and broader wings than the more aquatic typical ducks, while these again are better winged than the diving-ducks (Fuligulinæ); and the spine-tailed ducks (Erismaturinæ), which seem to be as aquatic as Grebes, have the smallest wings of all, except the Loggerhead, which is an outlying degenerate form with no very great diving power.

But, on the other hand, a surface-swimming bird may take to flying more than ever. It can hardly be doubted that the Gulls are better flyers than even their Plover-like ancestor, though the Alcidæ have, as Mr. Lucas points out, degenerated.

Similarly, the Steganopodes exhibit, in the Pelicans, surface-swimming forms with powerful wings, and the wing-power is increased in the Gannets, Tropic birds, and especially the Frigate birds; while the diving Cormorants have their wings reduced, and the extinct great Shag (*Phalacrocorax perspicillatus*) was apparently becoming flightless when its fate overtook it.

The Phalaropes present us with an interesting instance of water-birds of recent evolution; they are as yet only surface-swimmers, with wings not noticeably different from those of ordinary littoral sandpipers, and may in the future develop either way.

Thus, the transition is rather, in the first place, from land to water, and the bird may either become more aërial or more aquatic in consequence of the change of habit.

Similarly, birds which are markedly terrestrial are either good flyers, as Plovers and Sand-grouse, or exhibit a tendency to degeneration in flight just as marked as that of some aquatic birds. Instances of this are, of course, the Ratites, and such Carinates as *Ocydromus* and *Stringops*. The Gallinæ and Crypturi are also usually poor flyers, though none are yet quite flightless. *Stringops*, be it observed, must have lost its arboreal habits before it became flightless, and we find that the genera of Parrots which are probably most nearly allied to it, *Pesoporus* and *Geopsittacus*, are already terrestrial. Arboreal habits tend to preserve an average type of wing, flightlessness being obviously disadvantageous in such case, and also marked length of

wing feathers.[4] And this average wing is particularly characteristic of the Passerines and of most of their allies, which, as has long been known, present a general structure most marked by the absence of special peculiarities. In certain respects, indeed, their specialisation fairly entitles them to their usually conceded place at the head of the avian tree; but their broad outlines are those of a simple and primitive form. In this respect, as I pointed out in my previous paper, they show some analogy to the Primates among Mammals.

This brings me to my second point. Mr. Lucas suggests that the first birds might have had all their toes pointing forward, and that the Swifts may have retained this arrangement. But the fact, which he points out, and which I implied in my paper, that this arrangement is not universal even with them, tells against this view, as he admits. As to the embryo, I must say that in some nearly mature embryos of the Common Swift of Europe (*Cypselus apus*) which I examined, the hallux did not seem to me distinctly directed forwards.

Besides this, it is noteworthy in this connection that a type of foot with opposable toes is very likely to be developed in consequence of arboreal habits. The Primates almost universally, the Opossums, and *Lophiomys* in the Rodents, have opposable halluces among the Mammals; and though the grasping foot of the Chamæleons is not exactly like that of a perching bird, it shows that a very arboreal reptile can develop a prehensile foot with opposed digits; and I can see no reason why this should not have taken place with the primitive bird-reptile before it acquired the power of flight.

III.—By P. Chalmers Mitchell.

Owing to the opportunities afforded me by the kindness of my friend, Mr. F. E. Beddard, the Zoological Society's Prosector, I have recently been able to devote a considerable amount of attention to the anatomy of the bird's foot, and I am glad to respond to the invitation of the Editor to say something on the interesting discussion raised by Mr. Finn's paper in the June number, and by the editorial note in the July number of Natural Science.

There are two questions which have an important bearing upon each other, but which must be argued out separately. Mr. Finn argued for his view that three-toed birds, so far as the toes are concerned, are degenerate descendants of four-toed birds, and that the avian stock probably arose from an arboreal four- or five-toed reptile. In support of his argument, he brought together the cases in which apparently three-toed birds have been shown to possess a rudimentary fourth toe. In the Natural Science note it was pointed out that

[4] A typical form would be the Jay. Is it not possible that the flight by single violent flaps alternating with sailing often affected by this bird and others similarly winged may represent the earliest form of flight, originating in the effort of a parachuting animal to avoid falling?

even in those cases where the first toe is absent there is present a distinct flexor muscle corresponding in its position and relations to the flexor longus hallucis, the long flexor of the great toe. The tendon of this muscle has, of course, no great toe in which to be inserted, and unites with the common tendon of the deep flexor which splits up to work the other three toes. The writer pointed out that the existence of this muscle appeared to be strongly confirmatory of Mr. Finn's view. Unless three-toed birds originally were in possession of a hallux, why should they have a flexor hallucis? He proceeded to point out that this, however, cannot be taken as the survival of a meaningless vestige. For, in the great majority of avian groups (I accept Mr. Lucas's emendation, and quite agree with him that the words of the note were expressed so that a careless reader might suppose that individuals or species were meant), the flexor tendon of the hallux, before running to its insertion upon the hallux, gives off a stout slip to the common deep tendon of the other toes. Apparently it is this functional part of the muscle that is preserved. Were there a case of a Passerine where the hallux is absent, then we could see whether a functionless rudiment would be retained; for in Passerines there is no slip from the longus hallucis to the common tendon of the other toes.

Mr. Lucas brings forward interesting considerations directed to the view that in birds there is a progressive tendency towards the individualisation of these tendons. He suggests that, as is the case in many existing Lizards, so possibly in primitive birds a single muscle had many tendinous insertions, and that later on the muscle gradually broke up till more and more of the tendinous insertions came to have separate muscular bellies. He suggests, therefore, that in the case of three-toed birds they may have lost their toe before the muscle became individualised, and therefore he thinks that this condition of the tendons means that the birds are low in the avian scale, and therefore also, he concludes that the note does not supply an argument in favour of Mr. Finn's view. The three-toed birds, according to him, do not show evidence of the modification of their foot from a specialised four-toed foot, such as would be possessed by an arboreal form.

The matter is so interesting that I shall elaborate the argument more fully, as I am convinced that it is sound. In a typical four-toed bird there are two deep plantar muscles. The belly of the flexor longus communis lies most closely applied to the posterior surface of the shaft of the tibio-tarsus. At its upper end it spreads out fan-like, and takes origin sometimes continuously, sometimes by a series of digitations, from the upper end of the tibio-tarsus, with occasionally a slip from the femur. Its single tendon runs through a bony canal in the posterior part of the ankle-joint, and then runs down the posterior face of the shaft of the tarsus-metatarsus, near the lower end of which it splits up into a tendon for the second, third, and fourth toes. The flexor longus hallucis lies externally to the belly of the flexor

communis. It is a rounder and narrower muscle, entirely distinct from and unconnected with the flexor communis above the ankle-joint, and arising by a single head or by two heads from the external condyle of the femur, and from the intercondylar region. Its single tendon of insertion passes through the ankle-joint, generally through a separate bony canal which is placed more laterally than the bony canal of the flexor communis. Below the ankle-joint the tendons of the two muscles lie side by side, but in their course down the shaft of the tarsus-metatarsus the tendon of the flexor hallucis crosses over the tendon of the other, and in so doing (as has already been mentioned) except in the Passeres and Humming-birds, it sends a strong vinculum to the communis tendon, and then runs on to its insertion in the hallux. Now whatever may have been their original connection, these two muscles are as distinct and individual as possible. In the case of an ordinary three-toed bird like the Oyster-Catcher (*Hæmatopus ostralegus*), the two muscles are as separate in their origin and course as in a normal four-toed bird; the only difference is, that with the loss of the hallux, the slip of tendon which runs from below the vinculum to the tendon has disappeared. The facts almost irresistibly bear out the conclusion that such a three-toed bird has been derived from a four-toed bird not of a primitive character, but with a foot like existing normal four-toed birds. But evidence—one is almost tempted to call it experimental evidence—is furnished in favour of this view by the anatomy of the foot of *Pterocles*. Whether the Sand-grouse be considered a pigeon or a gallinaceous bird, most will agree that it is in the present condition of losing its great toe. In *Pterocles alchata* there is no trace whatever of the slip from the vinculum to the minute toe. But above that the two tendons, and above the ankle-joint the two muscles, are perfectly distinct; the flexor longus hallucis arises by two heads from the femur, while the flexor communis spreads out over the upper end of the tibio-tarsal shaft. Here is a case of recent degeneration of a hallux and the production of the same type of foot as in ordinary three-toed birds. It seems to me absolutely conclusive in favour of Mr. Finn's view.

But Mr. Lucas's remark about *Chauna* and *Palamedea* calls up another exceedingly strong piece of evidence. *Chauna* has no flexor tendon to its hallux, but in other respects the relations of the flexor longus hallucis down to the vinculum are precisely similar to those of four-toed birds. Mr. Beddard and I recently had an opportunity of dissecting *Palamedea*, and the results of our observations, communicated to the Zoological Society, will shortly be published in its *Proceedings*. We found that there was a separate tendinous slip to the hallux. So here again is a case showing that the mere suppression of the tendinous slip from the vinculum to the hallux changes a four-toed foot into the type of structure found in three-toed birds.

VI.

Notes from the British Association, 1894.

THE British Association is always a medley, and this characteristic is very apparent in the addresses delivered before it and the papers read at it. There are always some people who have something to say and some who have to say something. There are those who have manner without matter, and those who have matter without manner. Its great mission is to interest the public in science. The general public go there to be as scientific as they can. The scientific public go there to frivol as much as they may. It serves many useful purposes, but among these the least is to bring new scientific results before the scientific public. But although one need not expect any large bulk of novel and interesting scientific matter, there are always some interesting messages from the scientific world to the public, and not a few matters of general scientific interest.

We propose to give here, not an account of the meeting that shall pretend to any completeness, but notes on such matters as seemed of special interest to our representatives at the sections. As we have dealt elsewhere with Lord Salisbury's address, we shall pass straight to the sectional meetings.

Neglected Mineralogy.

The President of the Geological Section represents a neglected branch of science, and Mr. Lazarus Fletcher made excellent use of his opportunity in a direct plea for mineralogy by pointing out the lack of opportunity for the study of it in this country, and in an equally cogent indirect plea by the brilliancy of his own exposition.

Having given a Homerically imposing alphabetical list of living German scientists, from Arzruni to Zirkel, whose names are familiar as distinguished mineralogists, Mr. Fletcher went on to contrast the wealth of educational and research equipment of Germany with the apathetic treatment which mineralogy is receiving in the British Isles. In every German university there is a professor of pure mineralogy, while other continental nations are not far behind Germany in their practical appreciation of the subject. But here, throughout Great Britain and Ireland, the imposing number of the professorships in the science amounts to *two*; and in only one of the two universities is the subject allowed to rank with other sciences in the examinations for degrees.

University extension in a mineralogical direction was the theme of Mr. Fletcher's interesting address, and surely such an appeal was never more needed than at the present time when the study of minerals is being more than ever pressed by other countries, and must be undertaken by almost every geological student. Such has been the indifference felt towards mineralogy, that the last report on the progress of the science submitted to the British Association was that given sixty-two years ago by Dr. Whewell, shortly after he had resigned the professorship of mineralogy in the university of Cambridge. It was therefore incumbent upon the president of Section C, as a mineralogist, to give some account of the progress of the science since that date, and of its present condition: this duty Mr. Fletcher performed in the earlier part of his address in a masterly and captivating manner.

The audience was rapidly conducted over the recent history of crystallographic methods and theories up to the parallelepipedal and other arrangements of points which have been devised to account for the structure of crystals, special emphasis being laid upon the notation and methods introduced by the late Professor Miller, of Cambridge. This was followed by a brief survey of the development of optical and physical research as applied to the study of minerals; by a sketch of the remarkable progress of mineralogical chemistry during the last sixty years; and finally by an enumeration of the more important instruments which have been invented and improved during this period and which have enabled the mineralogist to extend his investigations in every direction, and have helped to raise mineralogy to the position of an exact science. No one has by his labours contributed more to this result than the late Professor Mallard, of Paris, whose lamented death last month was alluded to by the President as depriving mineralogy of her greatest philosopher.

If much of the address dealt with things that were absolutely unknown to the majority of his audience, Mr. Fletcher's argument was only the more strengthened, for where, under the present conditions, could they be expected to have received any instruction in the refinements of modern mineralogy, or to have learnt that there is a real science of minerals?

We hope that this address will open the eyes of many people. The words quoted from the Report of Dr. Whewell are strictly applicable to the present day; they related to the lack of "interest with which mineralogy, as a branch of natural philosophy, has been looked upon in England. Indeed, this feeling appears to have gone so far that all the general questions of the science excite with us scarcely any notice whatever. But a more forward and hopeful spirit appears to have prevailed for some time in other countries, especially Sweden, Germany, and more recently France." And yet "the step to which mineralogy owes the best portion of its scientific character was made by an Englishman, the Doctrine of Definite Proportions,

and if Englishmen seriously propose to themselves the task, we are justified by the history of Science in asserting that none are more likely than they to solve the great problem of mineralogy which now offers itself—the connection of chemical composition and crystalline form." Such were the words of Dr. Whewell in 1832.

Indeed, we have only to recall the additional names of Wollaston, Brewster, and Miller to remind ourselves that much of the most original work of the century has been done by English mineralogists. An appreciative reference, in Mr. Fletcher's address, to the highly philosophical teaching of Professor Maskelyne on crystalline symmetry, shows that the true spirit is by no means dead among us even in the most abstruse branch of the science; and Mr. Barlow has recently made important contributions to the same subject. Professor Renard, of Gand, in seconding a vote of thanks, declared that no one had done more to elevate the respect already felt abroad for British mineralogy than the distinguished President himself, and referred especially to his admirable "Introduction to the Study of Minerals," which was published in the form of a guide to the vast collection, of which he has the charge, at South Kensington.

ANTHROPOLOGY.

Sir William Flower performed a most useful task when, in his presidential address, he reviewed the past history and the future prospects of anthropology. As a separate branch of science anthropology is so new, and on many of its sides is so largely aided and furthered by the amateur, that it was well worth while pointing out that the scientific training and equipment for the scientific study of anthropology is as great as those required in any of the older sciences. "It invokes the aid of the sciences of zoology, comparative anatomy, and physiology, in its attempts to estimate the distinctions and the resemblances between man and his nearest allies, and in fixing his place in the scale of living beings. In endeavouring to investigate the origin and antiquity of man, geology must lend its assistance to determine the comparative ages of the strata in which the evidences of his existence are found, and researches into his early history soon trench upon totally different branches of knowledge. In tracing the progress of the race from its most primitive condition, the characteristics of its physical structure and relations with the lower animals are soon left behind, and it is upon evidence of a kind peculiar to the human species, and by which man is so pre-eminently distinguished from all other living beings, that our conclusions mainly rest. The study of the works of our earliest known forefathers—'prehistoric archæology,' as it is commonly called—is now almost a science by itself."

Towards the conclusion of his address the President gave an account of the conclusions arrived at by the committee appointed by Mr. Asquith to inquire and report upon the "anthropometric"

methods of registering criminals. He expressed complete agreement with their recommendation of a modification of the French system of measurements, with the addition of Galton's "finger-print" method.

The Antiquity of Man.

It is interesting to watch the gradual change of opinion as to the antiquity of the human race, and to compare what is said at Oxford in 1894 with discussions of the same question at former meetings of the British Association. At one time the debate was largely one for theologians to take part in, and personalities were freely used; now a joint meeting of the anthropologists and the geologists to discuss the Plateau Flint Implements of North Kent leads to a thoroughly scientific and calm review of the whole question. It was unfortunate that age and illness should have prevented the Nestor of our geologists, Professor Prestwich, from taking part in so interesting a debate, and it was unfortunate also that Mr. W. J. L. Abbott, who has done so much good work in the Plateau Gravels of recent years, and whose interesting paper on this very subject will be remembered by our readers, should also have been too unwell to attend; otherwise nearly all the leading authorities took part in the debate.

Professor T. R. Jones opened the discussion on behalf of Professor Prestwich, maintaining his well-known views as to the vast antiquity of the rude implements found on the higher plateaus. Mr. Whitaker doubted whether many of the supposed implements exhibited at the meeting were really of artificial origin, but acknowledged that some were certainly genuine. Most of these had been picked up on the surface, not dug out of undisturbed gravels. He questioned whether the discovery of implements in the "clay with flints" was any evidence of age, for the clay with flints was still in process of formation. Mr. Montgomerie Bell thought that he could trace a continuously improving series of implements in the different deposits of Kent, but Sir John Evans doubted whether Kent was really the cradle of the human race.

Dr. Hicks spoke of the relation of the implement-bearing gravels of Middlesex to the glacial and cave-deposits of North Wales, and insisted that man was pre-Glacial. Professor Boyd Dawkins pointed out that rude workmanship was no satisfactory evidence of antiquity, as rough or finished implements would be used according to the purpose for which they were required. General Pitt-Rivers spoke on the gradual evolution of implements of different types. He did not consider the mere occurrence of a bulb of percussion as sufficient evidence of artificial origin, and doubted the value of the evidence as to the implements having been found in place in the plateau deposits. Mr. Clement Reid questioned the extreme antiquity of the implements, pointing out that careful search had led to no discoveries in undoubted Pliocene and pre-Glacial deposits. Colonel

Godwin-Austen suggested that much of the apparently artificial chipping of the flints had originated through the rubbing together of stones in masses of talus and mud. On the whole, then, the general sense of the meeting was opposed to granting any extreme antiquity to the early dwellers on the former plateau of Kent.

FORESTRY.

Professor Bayley Balfour, in his capacity as President, addressed the Biological Section on the subject of Forestry. In many respects this address was the most important communication that was delivered to the public in any of the Sections of the Association.

Two reasons have interfered to prevent the English people paying necessary attention to the afforestation of our land. The first is the abundance of coal in this country. For many years all practical men have regarded the supply as almost inexhaustible, and it is only now that the warnings of experts in mining and geology have been accentuated by the actual exhaustion of many of our coal-fields that there is a possibility of getting the public to believe that England may soon have to revert to the partial use of charcoal. The next reason is that the continuous extension of our commerce and our territory has opened up for use series of forest-clad areas, while the woods and forests of the continent have by themselves been nearly able to supply our demands. But the increasing scarcity and price of imported timber is now awakening our eyes to the enormous waste of our timber-growing capacity. This waste is in two directions: the forests we have, are, from want of trained experts, in many cases very badly managed; immense areas, well adapted for growing timber, and ill adapted for any other crops, are left barren.

Besides the direct economic advantage of increasing the productiveness of the land, there are many other advantages that would accrue were the course urged by Professor Bayley Balfour and others to be adopted. There would be a great increase in the employment of labour; a very large part of at least the first steps in afforestation would demand unskilled manual labour, directed by a comparatively small staff of experts. We believe that the London *Daily Chronicle* has already advocated this as a field for that State employment of the unemployed which is believed by many to be at once prudent and inevitable.

The extension of woodland and forest would increase not only the natural amenity of our country, but it would appeal directly to all naturalists and lovers of animals and plants, in that it would provide an enormous increase of area suitable for many of the most attractive wild animals and plants that can live in this country. Professor Bayley Balfour made a practical suggestion to aid the attainment of his object. "There are tracts which without damage to the natural beauty, and without depriving in any sensible degree the people of their privileges of recreation they prize so much, might

be and should be dealt with as forest cultivated on scientific principles. These might serve as instruction areas, showing all that is best for the information of foresters. The creation of some such experimental teaching stations in State forests is one of the essentials for forestry in Britain. I would go further, and say that the area of State ownership should be increased to the extent of the establishment of forest stations, of an acreage sufficient to allow of a satisfactory rotation, in other parts of the country as centres of instruction." "In a practical science like forestry an increase of sound technical knowledge can only be possible when facilities for practical instruction are provided. I would, therefore, ask the President of the Board of Agriculture to consider what I have just said with regard to State forest experimental areas. These cannot, of course, be created by a stroke of the pen, but the initiative for their formation would naturally come from the Board of Agriculture." "It appears to me that while we must obtain from the Government the institution of sylvicultural areas for practical instruction, our best chance of success in acquiring the necessary endowment for the rest of the teaching lies in the line of combination between the Board of Agriculture and the County Councils, with, it may be, aid from private benefactors. But if we were to draw financial support from County Councils or from private sources, we must as a first step towards this make known, more thoroughly than it is, the nature of the national interests involved. We must disabuse landowners, land agents, and practical foresters of the notion that forestry consists in the random sticking in of trees, which anyone, no matter how unskilled, may accomplish. We must bring home to the people's minds that in science is to be found the only sure guide to proper timber growing, and that scientifically managed forests are alike a profit to the producer, a benefit to the community of the region in which they are reared, and a source of national wealth. Once we have got so far as to create this opinion, the funds for as extended a scheme of forestry education as may be necessary will, I venture to think, be forthcoming."

Turning from the duties of the nation to the duties of botanists, Professor Balfour urged upon them the necessity of paying more attention to the subject, for it is from the ranks of botanists that the directors of future schemes of afforestation and the teachers of practical foresters must come.

The Nucleus in Plants.

The next subject of botanical interest we shall mention is a very technical one, and a subject in which none but the scientific public could possibly take an interest. At one of the meetings of the Biological Section Professor Strasburger gave an admirably clear account of his researches on the periodic variation in the number of chromosomes in the nuclei of plant cells. After discussing the

probable history of sexual and asexual reproduction in plants, Professor Strasburger went on to speak of plants which exhibit alternation of generations. In these forms it is invariably found that in the nuclei of the germ-cells of the asexually produced individuals, a definite number of chromosomes is always present when the nucleus is in an active stage. This number appears, at any rate in the case of the lower cryptogams, to be twelve. Hence it follows that a fertilised ovum contains twenty-four. This latter number characterises every cell in the body of the sexually produced individual up to a certain point, *i.e.*, the appearance of the egg or spore mother-cells. At this point a reducing division sets in, and the number of chromosomes in every spore formed is lessened to half its original number *i.e.*, twelve.

The exact nature of this reducing division is not yet evident.

Carrying these observations further, he finds that in the higher phanerogams, where the sexual generation may be suppressed, the same sudden reduction from sixteen to eight chromosomes occurs previously to the formation of the germ-cells. From these facts he concludes that the reducing division in these plants marks phylogenetically a more primitive condition in which alternation of generations obtained. Professor Bower is translating Professor Strasburger's paper, and the full text of the translation will appear in a forthcoming number of the *Annals of Botany*.

PHYSIOLOGY.

Professor E. A. Schaefer's presidential address derived additional interest from the circumstance that this is the first year since 1847 that there has been a separate section for physiology. While pointing out that the enormous increase in England of those devoting themselves to physiology as a special science had led to the reappearance of this as a separate section of the Association, Dr. Schaefer suggested that it might not be advantageous to have one at every meeting of the Association. "Physiology is, above all things, a practical science. It requires laboratories and means of demonstration. Physiologists are rarely satisfied with the opportunity of hearing and reading papers; but require that, as much as possible, the actual methods of research employed should be capable of demonstration. By this I am not to be supposed to advocate the demonstration of experiments upon animals, for there are very many subjects in physiology which can be both worked at and illustrated in a manner involving in no sense whatever the word vivisection."

We differ from Professor Schaefer's conclusion. To very few of the sections would his words about "practical science" and so forth not be applicable, and for the purposes of demonstration and explanation of the methods of research, there are special gatherings of specialists. In the case of physiology, there is that very energetic Physiological Society, to which Professor Schaefer referred, and

which meets in the physiological laboratories of its members for the precise purposes to which Professor Schaefer alluded. There are subjects galore on which papers can be read and discussions raised without the necessity of laboratory demonstrations. In many respects, however, we cannot help regretting the breaking up of the section of biology into separate sub-sections and sections. It tends more and more to stamp the Association with that isolated specialism which is inevitable in modern science, but which carries with it great disadvantages. If the zoologists meet by themselves in a zoological laboratory, the physiologists in a physiological, and the botanists in a botanical, the odds are that the subjects they discuss will be of a nature to be unintelligible or uninteresting to those of another specialism. We imagine that a botanist or a zoologist would gather little from those demonstrations of the methods of research in a physiological laboratory which Dr. Schaefer advocated. What we want at the meetings of the British Association is an increase of organised discussions of subjects which lie on the borderland of the different branches, and of the different branches of these branches.

To take an example, one of the subjects of Dr. Schaefer's address was recent discoveries about the cell, and specially about the nucleus and the centrosome. Dealing with the researches upon the centrosome of the physiologist Heidenhain, the President described it as one of the obscure subjects of which, though supremely important, physiology knew next to nothing. Now, in the Biological Section, Professor Strasburger read a paper on the nucleus and centrosome in plants, while Dr. Van Beneden discoursed on the same topic from the point of view of a zoologist. Is not this a lamentable instance of failure to take advantage of the opportunity of joint discussion? Very many similar subjects might be mentioned. For instance, at the present time there are different and novel views of the nature of heredity and of the structure of protoplasm propounded by botanists, physiologists, and zoologists. And elsewhere in this article we allude to similar lack of concert on subjects of Geographical Distribution and Darwinian Theory. We are certain that well organised discussions of these held by biologists from all the sub-sections of biology would prove not only of surpassing interest but of great value. Although the physiologists do not intend to meet as an independent section next year, yet the botanists promise to carry on the evil.

Natural Selection.

The most interesting discussion in the Biological Section was confined almost entirely to zoologists. Professor D'Arcy Thompson boldly threw down his gauntlet by stating his doubts and difficulties in accepting "Darwinism." He could not see how many forms and colours of animal life were to be explained on the simple hypothesis of Natural Selection, even assisted by Sexual Selection; and he instanced the brilliant colours of humming-birds and the logarithmic

spiral of a coiled shell, whether foraminiferal or molluscan, as due rather to physical laws governing growth than to any advantage which they might give their possessors in the struggle for existence. The ingenious speculation that the pear shape of a guillimot's egg is derived from the protection that it affords to the egg from falling off the rock ledge, did not find favour with Mr. Thompson, who pointed out that the same shape was found in eggs, such as the plover's, laid on the broad flat ground. This shape he ascribed to the mechanical pressure exerted by a narrow oviduct on a relatively large egg.

Professor C. V. Riley, the eminent entomologist of Washington, was prevented by the strict time-limits from properly presenting his suggestive conclusions derived from a study of social insects. This we hope partially to remedy by publishing his valuable paper *in extenso*. Broadly put, his opinion was that Natural Selection was an insufficient or impracticable cause in the evolution of the varied groups of individuals met with in an insect colony, and that much was due to the direct action of environment, notably of food. These are views to which much attention has already been directed in our own columns.

Professor J. Berry Haycraft, who felt constrained to describe himself as a physiologist, but not a biologist, raised a most interesting objection to the views held by Weismann as to the part played in evolution by sex. He pointed out that the conjunction of two parents, instead of promoting variation, restrained it and kept it near the mean. This, he said, was proved by the statistics collected by Francis Galton; without sex, we should have no genera or no species, only individuals. This interesting communication will appear in a future number of NATURAL SCIENCE.

Dr. F. A. Dixey contributed to the discussion a detailed account of the evolution of mimetic characters in certain butterflies, without, however, very clearly showing how the gradual stages that led up to the completely mimicking forms could have been due to any struggle for existence or any protective advantage.

Professor H. F. Osborn, of Columbia College, New York, entitled his paper, "Certain Principles of Progressively Adaptive Variation observed in Fossil Series." A large part of it, however, was devoted to emphasising the necessity for analysing variations according to their character and the period of ontogeny at which they occurred. Thus, he divided all variations into Palæogenic and Neogenic, the former being instances of reversion, and therefore not entering, like the latter, into progressive evolution. Neogenic variations may again be divided into four classes:—(1) Gonogenic, due to variation in the original ovum or spermatozoon before their union; (2) Gamogenic, due to the union of diverse sexual elements; (3) Embryogenic, due to the influence of environment on the developing embryo; (4) Somatogenic, due to the action of habits or changed environment on post-

embryonic stages. Mr. Osborn pointed out that a study of the development of variations through a long lapse of time showed that similar structures might be reached by quite different roads, as instanced by the molar and pre-molar teeth of *Anchitherium*. He seemed inclined to attribute this more largely to direct mechanical stimulus acting towards the same given end, than to the action of Natural Selection.

After these various onslaughts, the discussion promised to be lively. But Professor Ray Lankester and Professor Poulton, who were the chief supporters of Natural Selection, dealt with the obvious misapprehensions of Darwinism in the papers of those who spoke against Natural Selection rather than with the details of their objections. While we have the strongest sympathy with those who point out the astonishing misconceptions of Darwin's own views, which, unfortunately, are so prevalent with those who bring forward difficulties in the theory of Natural Selection, we think that the attitude adopted by the supporters of Darwinism is unfortunate. For the general public are even more ignorant of Darwin's own writings than those who urge scientific objections to his theories. The impression left on the minds of the camp-followers of science is, that the defenders of Natural Selection are more concerned to maintain an orthodox interpretation of the Darwinian scripture than to do battle about the actual facts.

Mr. Pycraft followed this discussion with a valuable account of the wing of *Archæopteryx*, which gave rise to an animated discussion, in which Dr. Sclater, Professor Ray Lankester, Dr. Hurst, Mr. Finn, and others took part. As, however, this paper will shortly appear in our pages, further description is now unnecessary.

The Distribution of Animals.

This subject, in one or other of its varied phases, was dealt with in many of the Sections, and it seems a pity that some arrangement could not have been made by the organising committees to enable those interested to meet and discuss the question at the same place and time. Professor W. A. Herdman endeavoured to accommodate himself to the situation by reading his valuable Report on the Marine Zoology of the Irish Sea before the Geological as well as the Zoological Section. The result, however, was that in neither place could it receive the attention which all felt that it deserved. The Committee, of which Professor Herdman is both Chairman and Reporter, has paid particular attention to the deposits now forming on the floor of the Irish Sea. It has mapped out the distribution of the various sands, muds, shell-conglomerates, clays, and nullipore deposits, and has endeavoured to see how they are connected with the older geological formations of the sea coast and the submarine deposits lying off the shore. It has further come to the very important conclusion that the nature of the deposit is the most important of the

various factors that determine the distribution of animals over the sea-bottom within one zoological area. It is proved to be more important than mere depth; while the same locality may vary so much from time to time in the temperature, the salinity and the transparency of the water, that these factors, within certain limits, have far less influence upon the fauna than has the nature of the deposit. In the discussion that took place on this paper, Mr. F. A. Bather pointed out how important the study of these modern conditions was to the geologist who attempted to correlate strata by means of their fossil contents; and he suggested the possibility that at some future epoch, when the floor of the Irish Sea had become dry land, these absolutely contemporary deposits might come to be regarded as a series of successive formations, each characterised by its peculiar "zonal" fossils. A similar state of affairs in the Silurian rocks of Gotland has actually given rise to much heated controversy.

Among the more interesting discussions in the Geological Section was that which arose out of Dr. Hicks' paper on The Homes and Migrations of the Earliest known Forms of Animal Life as indicated by Recent Researches. Few of the speakers, however, seemed to confine themselves to the real questions at issue, a fact which may have been due to a want of clear understanding of Dr. Hicks' position. We hope that our readers will be able soon to form their own opinions on the subject, as this interesting communication will appear in a future number of NATURAL SCIENCE.

In the Zoological Sub-section Dr. Otto Maas, whose paper on "Some Problems of the Distribution of Marine Animals" (NATURAL SCIENCE, vol. ii., p. 92) our readers will remember, gave an interesting account of "temperature as a factor in the distribution of marine animals." It was pointed out by Messrs. H. N. Dickson and W. Garstang, during the discussion, that much variation of fauna, apparently due to variation of temperature, might really be due to an influx of water from other sources and to the consequent introduction of foreign forms of life, or the change of the chemical and physical characters of the water. Most speakers, however, seemed to think that so important a paper should be published before it could be discussed. We have much pleasure in announcing that the complete work will appear in an early number of NATURAL SCIENCE.

These seasonal movements of the water in the North Sea and in the Atlantic, which have so much bearing on this subject, were discussed more fully by Mr. Dickson before the Geographical Section. Here also there was a discussion on the distribution of African fish, and, finally, it was here that Dr. John Murray thought fit to read his important paper on the Geographical and Bathymetrical Distribution of Marine Organisms. In the course of this he attempted to demolish the Antarctic Continent of Mr. H. O. Forbes, a question on which geologists and zoologists, no less than geographers, might well have been asked for their opinion.

Maxim's Flying Machine.

Although artificial flying is a problem to be settled by engineers rather than by zoologists, it is a subject which always must have a special fascination for zoologists. It was therefore natural that a number of zoologists should have deserted to the mechanical section to hear Mr. Maxim.

The subject of his paper is of fascinating interest, and the accounts of his experiments, which have extended over some years, were also exceedingly interesting. As might be expected from a man of Mr. Maxim's inventive genius, something has come out. The first is a steam engine of hitherto unrivalled lightness compared to the enormous power it can develop, and a system of aeroplanes which, as the author claims, has enabled him to be the first to fly—not for long, it is true, but that is, after all, only a question of time. The question of landing safely has not yet been solved, and there seems to be some difficulty, as the machine ceases to maintain itself at anything under thirty-five miles an hour.

While Mr. Maxim has not yet got a perfect flying machine, he certainly has advanced considerably towards the solution of the problem; though it may be questioned whether he has, in using the system of aeroplanes, hit on the best plan, that is if one of the conditions is that the flying machine has to be used for warlike purposes, and not for rapid transit. Perhaps something of the kind suggested by Lord Kelvin is more likely to effect this object.

Lord Kelvin and Lord Rayleigh had both ridden on the machine, and expressed themselves highly pleased with the extremely skilful way in which Mr. Maxim had overcome the various difficulties the condition of lightness in everything had imposed upon him. Mr. Langley, a notice of whose recent publication, on "The Internal Work of the Wind," appears on our next page, sounded a note of warning about the liability of the machine to turn, which, coming from such an authority, must not be disregarded.

Many other points of interest were raised at the meeting, which we have not space to discuss. We hope to recur to some of them on a future occasion.

SOME NEW BOOKS.

THE INTERNAL WORK OF THE WIND. By S. P. Langley. Published by the Smithsonian Institution, Washington, 1893.

To understand how birds fly, and to imitate them ourselves, were primitive ambitions of the race; and ambitions, in spite of Mr. Maxim's aeroplanes, they remain. By severe exertion Lilienthal was able, for a few moments at a time, to develop an ascensional force equal to something less than his own weight. The condor and other large birds poise themselves for hours in mid-air, rising, falling, and progressing with no apparent effort, and without the slightest vibratory movement of their expanded wings. The omission of some important factor in the theory can alone account for so great a discrepancy in the result. That this factor is in some way *the wind* has been long suspected; but the idea, sometimes held, that a uniform current (however strong) can for long assist the soarer, is at variance with the elementary principles of dynamics. In order that the bird may rise like a paper kite there must needs be something to take the place of the string. In a uniform current there is no force available for the purpose except the bird's inertia during the first few seconds after leaving the ground; but that birds do use their inertia, at all events in this limited way, is proved by the observation that many of them are unable to soar without facing to windward at starting. If, however, the current is *not* uniform, the limitation disappears, for in passing from a quicker to a slower current, or *vice versâ*, inertia again comes into play. The question, therefore, remains, do such inequalities exist, and if so, are they adequate to account for the phenomena? Professor Langley's paper answers both parts of the question affirmatively, and it is not too much to say that his experiments constitute by much the most important advance yet made in the theory of aerial flight. "In the ordinary use of the anemometer," he writes, "the registry is seldom taken as often as once a minute." His first experiment was to place an ordinary Robinson anemometer on a mast fifty-three feet high and connect it electrically with the chronograph of the Allegheny Observatory, so as to register every 25 revolutions of the cups. The results are drawn out in a diagram with abscissæ = Time, and ordinates = Wind velocity. In a period of about five minutes, taken at random from the diagram, the wind velocity is seen to have fallen from 13 miles an hour to 9, risen to 17, fallen to 14½, risen to 15, fallen to 14, risen to 19, fallen to 13, risen to 18½, fallen to 14, and risen to 17. This degree of variability in "the wind" is remarkable enough, but Professor Langley's research did not stop here. He proceeded to lighten the cups and to make an automatic record of *every revolution*. The resulting curve often exhibiting nine or ten cusps within a single minute, and deviations of 30 or 40 per cent. on both sides of the line of mean velocity, reveals

a state of things hitherto quite unsuspected either by meteorologists or theorists on flight. Extreme variability seems to be equally characteristic of strong and of gentle winds, nor is there any reason to suppose that a still lighter instrument recording still more frequently would not bring to light further inequality. It is quite likely that "the incessant alterations which, it appears, make 'wind,' are due to impulses and changes which are preserved in it, and which die away with considerable slowness." What we should like to know more of is the change (if any) in *direction* which accompanies the change in velocity. Is the "structure" of the wind mainly undulatory or mainly vertical? that is to say, are the variations observed at a fixed point due to the passage of "ripples" or minute "cyclones"? Upon the answer to this question, it seems as if the future of what Professor Langley calls "Aërodromics" will largely depend.

R. C. GILSON.

PLANT CLASSIFICATION.

UEBERSICHT DES NATÜRLICHEN SYSTEMS DER PFLANZEN. Zum Gebrauch in Vorlesungen für Anfänger. By E. Pfitzer. 8vo. Pp. iv., 36. Heidelberg, 1894. Price 1s.

PROFESSOR PFITZER'S "Übersicht des Natürlichen Systems der Pflanzen," designed primarily for students attending his course of lectures for beginners, will, we doubt not, be found useful at centres of learning other than Heidelberg. It is a small octavo volume of thirty-six pages, with one side blank for notes, and contains a sketch of plant classification. In a few lines are given the chief characteristics of the great groups and divisions, and, *à propos* of these, we cannot fail to notice the changes which have occurred in this branch of the science during the last few years. Those of nomenclature, though necessitating the loss of well-known terms, indicate an advance, in that real and not imaginary differences are now expressed. Instead of the Phanerogams and Cryptogams of the text-books of not many years ago, we now find plants ranged under the three great groups, Siphonogamæ (equals Phanerogams), where the egg, or oosphere, is contained in an ovule and fertilised by the agency of a pollen-tube; Archegoniatæ, where the egg is contained in an *archegonium* and fertilised by an independent cell—the *antherozoid*, and including (1) the ferns and their allies, the lycopods and equisetums, (2) the mosses and liverworts; and, thirdly, the Thallophyta, plants which form neither archegonium, ovule, nor pollen-grain, and including the two great divisions of Algæ and Fungi.

Of the two divisions of the Siphonogamæ, Angiospermæ and Gymnospermæ, the latter, in which, though fertilisation takes place by means of a pollen-tube, the egg is contained in a rudimentary archegonium, finds a most natural position at the end next the true Archegoniatæ. We may also note the tentative subdivision of the Angiosperms into Acrogamæ where the pollen-tube reaches the egg by means of the micropyle, and endosperm is formed after fertilisation, and the Chalazogamæ, where the mode of entrance is by way of the chalaza, while endosperm is formed before fertilisation. The former include everything except the Australian genus *Casuarina*, which constitutes the order Casuarinaceæ, and at present the subdivision Chalazogams. In the subdivision of the dicotyledons with free petals (Choripetalæ) there has been much alteration from Bentham and Hooker's separation into Thalamifloræ, Disciflorae, and Calycifloræ. Only the last of these remains as one of six, while the whole is enlarged

by the intercalation of the families formerly separated as the anomalous division Apetalæ. Some of these, like the Chenopodiaceæ and Amarantaceæ, fall naturally into their places, but the affinity of others, like Salicaceæ (the willows) is doubtful or quite unknown, while others again comprise distinct series. Under each natural order (the very rare or small ones are omitted) the most important characteristics are given, with the floral formula and the broad geographical distribution. In the case of larger orders, division into tribes is noticed, in the smaller only the endemic genera and the species of economic importance.

Professor Packard on Acquired Characters.

On the Inheritance of Acquired Characters in Animals with a Complete Metamorphosis. By Alpheus S. Packard, M.D. Reprint from the *Proceedings of the American Academy of Arts and Sciences*, 1894, pp. 331–370. Boston, Mass.

The historical *précis* with which Professor Packard opens his paper very naturally consists largely of an exposition and criticism of Weismannism. In this he makes one serious error. By quotations from the "germ-plasm," he supports his statement that "Weismann's reasons for not accepting the doctrine of transmission of acquired characters would appear to be purely hypothetical and *à priori*." It is certainly true that Weismann has adduced purely theoretical and *à priori* reasons. But again and again in published papers he has examined supposed cases of the transmission of acquired characters and of mutilations, and has shown the considerations of actual fact which support his hypothetical and *à priori* objections. As a matter of history, each term in the continuous adduction of fresh cases, or supposed cases, of such a transmission, has been brought forward after Professor Weismann and his supporters have shown reasons for not accepting preceding terms. The present position of the controversy is this. As yet no case of the transmission of acquired characters has been brought forward based on evidence accepted as cogent by Professor Weismann and his supporters. Professor Packard may say that this is so much the worse for the intelligence of Professor Weismann and his supporters. He may not say that their reasons are purely hypothetical and *à priori*.

Professor Packard reviews a number of the arguments brought forward by the advocates of this transmission and adds several of his own. We are, however, compelled to state that the arguments are not presented so as to convince any but the already convinced. Take one of his instances. He cites Paul Bert's experiments upon daphnids, in which he attempted to acclimatise these fresh-water crustacea to saline water by gradually adding salt. At the end of forty-five days, when the water of the aquarium contained 1·5 per cent. of salt, "all the Daphniæ had died; but the eggs contained in their brood-sacs had survived, and the new generation of Daphniæ, to which they had given birth, flourished perfectly well in the same medium." "We should interpret these facts," says the Professor, "as showing that the crustacean had been so profoundly affected in the lifetime of the individual as to produce young perfectly adapted to a changed environment. The germ-plasm may have been the vehicle, all the same, but the experiment is a case in favour of the neo-Lamarckian principle." Was ever argument less cogent? Here are animals supposed to transmit to their offspring a character before they have acquired it: to transmit to their eggs a capacity to endure salinity, to the non-acquisition of which they testify by their own deaths.

Moreover, the offspring, with respect to this character, are in a position in which it is impossible to judge of what, in their case, is inherited and what is acquired, for they are subjected to the new influence from their earliest birth. As in this instance, so in many others, Professor Packard walks round the point at issue. All instances of the power of environment to mould the individual are of the greatest value; and Professor Packard and others have done service in directing attention to a side of nature perhaps neglected. But, for this particular controversy, what is wanted is proof of the inheritance of the result of the moulding.

The actual occasion of this publication is a careful and valuable account of the complicated metamorphoses of many insects, accompanied by suggestions of causes in the environment which might have produced the characters of the stages in question. Students of evolution and of natural history will find this valuable, but we submit that none of it has a direct bearing upon the controversial problem placed as the title of the paper. In every case the argument goes no further than to suggest that, if acquired characters be transmitted, and if the Lamarckian factor in modification of organisms have the value assigned to it by Professor Packard, then the characters in these stages of larval history might be explained as due to the inheritance of acquired modifications. But all this, although it has a place, and a useful place, in scientific writing, takes us no further towards a solution of the problems in question, and gives support to the uneasy suggestion made at the recent discussion at the British Association that many of the opponents of Natural Selection do not understand the nature of the problems about which they write.

Zoology for Farmers.

Agricultural Zoology. By Dr. J. Ritzema Bos, Lecturer in the Royal Agricultural College Wageningen, Holland. With an Introduction by Miss E. A. Ormerod; Translated by J. R. Ainsworth Davis, B.A., Professor in the University of Wales. Pp. 256 and xx., with 149 illustrations. London: Chapman & Hall, 1894. Price 6s.

Dr. Ritzema Bos is so well known as a competent scientific naturalist who has specialised on what is called economic zoology, that we welcome an English rendering of his little book. It is designed to be a handy book for the practical farmer, and it deals with the animal kingdom in such a way that the private student shall have an intelligent idea of the general structure and organisation, and of the place in the scale of life of those animals that are harmful or helpful to agriculture. Reference to the domesticated farm animals is omitted, "for these are treated not by the zoologist but by the lecturer in stock-breeding." The translator, in his preface, says that he has made a few small additions and alterations, to adapt the book for those who live in England. Miss Ormerod, in her introduction, gives the book her blessing, and commends it to agriculturists.

The English is easy and intelligible. The illustrations, especially those dealing with insects and eelworms, are well adapted for their purpose. So far as the make-up of the book goes, we have praise for everything but for the lamentable absence of an index. The book ought to go into other editions. We hope that the technical instruction committees of County Councils will make large use of it for their teachers and for their students, but in another edition an index must be added—an index containing the common English names of all the animals referred to.

Dr. Ritzema Bos is a kindly man, and is ready to say a good word for wild animals as often as he can. Among mammals and birds he sets against each other the good and the harmful influences upon agriculture, and condemns unhesitatingly very seldom. Thus foxhunters will be pleased to learn that the fox "is perhaps generally of more use than otherwise to the farmer and the forester. It catches many rabbits, and also an enormous number of field voles, in the years when these become a pest. It also often eats insects (*e.g.*, cockchafers), worms, and snails." The stoat is decided to have a balance of good qualities, the weasel is almost uncompromisingly praised for its usefulness as a vermin destroyer; the common shrew, the lesser shrew, the hedgehog and the bats are all to be preserved. On the other hand, for the water shrew, the rats and mice, and the voles, Dr. Bos suggests a careful preservation of their natural enemies, and a bill of fare of which phosphorous paste, dressed in various attractive fashions, is the principal ingredient.

We are less satisfied with his treatment of birds. It is true that he advocates the preservation of all the British owls and of the kestrel and the buzzard. But he says that "predominantly harmful from killing domestic mammals" are the following species occurring in Britain: The sea eagle, the golden eagle, the peregrine falcon, the merlin, the hobby, the sparrow hawk, the goshawk, and the harriers. The result of the examination of stomachs proves that in many of these cases a large balance of good is done by the birds. As for the other cases, though it be true that *aquila non capit muscas*, the eagle destroys no insect pests, we have no sympathy for the naturalist who plays the informer. It is a poor consolation to know that few English farmers are likely to be driven by depredations on their flocks to set springes to catch golden eagles, or to go out with shot guns against sea eagles!

Among other birds the black list is less offensive to us, although we fear that the naturalist and the agriculturist will never quite see eye to eye in the matter of the destruction of any kind of birds. It includes the raven, the magpie, sparrows, and linnets. Because of its shortness it marks so great an advance on popular notions that we will pass it by in silence.

A good and much needed word is spoken for the persecuted toad. In addition to its attacks upon insects, the toad is a great destroyer of snails. "In the Research Garden attached to the Rouen Entomological Laboratory the snails were entirely exterminated in 1891 as a result of introducing 100 toads and 90 frogs."

Although we have occupied so much of the space at our disposal in discussing the part of Dr. Ritzema Bos' book that deals with Vertebrata, as a matter of fact the greater part of the book itself deals with injurious insects and worms. For all this we have nothing but praise. It would be of inestimable advantage to farmers and to gardeners were they to have an acquaintance with the facts about these common pests which are so clearly explained in this excellent treatise.

MONISM.

THE MONIST. A Quarterly Magazine. Vol. iv., no. 4. Chicago: The Open Court Publishing Co. London: Watts & Co. Price 2s. 6d. July, 1894.

WE fancy that monism, as yet, subtends a small angle in the minds of most of our readers; we may, therefore, take advantage of an article in the current issue of the *Monist*, written by the editor of the *Monist*, and

entitled "The Message of Monism to the World," in order to give a very brief account of the doctrine. Until a few years ago, in the minds of most people who reflected upon the meaning of things, and in the minds of each of the great mass of men who accepted with easy-going indifference what was thrust upon their consciousness, mental images of the universe were broken up among a series of separate identities. Thus, in the sphere of theology, there were the Creator and the things created; in psychology, there was the central ego, and the surrounding swirl of sensations and states of consciousness, there were mind and body acting and reacting upon each other. In science there was matter, and force acting on matter, while even to different kinds of matter and varieties of force there were ascribed separate existences.

In all these the attitude of the thinking world is slowly changing. Formerly, man attempted to express his sense of the dependence of all things upon a Supreme Being in the phrase "Omnipresence of the Deity"; now we attempt to express the same relations in the phrase "Immanence of the Deity." The change of phrase marks an attempt to destroy the sense of separation between the Deity and the universe, or, rather, to destroy the idea of the co-existence of two separate identities. Similarly, in psychology there is a tendency to be rid of a separate abstract metaphysical "ego," to merge the abstraction in the sum of states of consciousness. There is not an "I" which has sensations, emotions, and ideas; the sensations, emotions, and ideas are part of the "I." The change is better known in things scientific. When now we speak of matter and force acting and reacting upon each other, we are ready to admit that matter in the last analysis resolves itself into the action of forces, and that we know force only through its action on what we call matter. In the separate sciences the change of tendency is still plainer. We seek to interpret the elements in terms of each other, perhaps as unsuccessfully as Lord Salisbury seems to think, but with a hope greater than his, and we believe in the conservation of energy and in the transformation of chemical, electrical, physical, and perhaps even vital forces.

"Monism" is the pointed, progressive, and polemical expression of all this change of attitude. It is, to use the words of Dr. Paul Carus, "the new philosophy that is dawning upon mankind: the theory of oneness, which indicates that the world, we ourselves included, must be conceived as one great whole. All generalisations, such as matter, mind, and motion, are abstractions representing aspects of reality, but not entities or things-in-themselves, by a combination of which the universe has been pieced together. And all our notions of nature can be formulated in exact statements, which, when properly understood, form one harmonious system of natural laws." No doubt, like the active and aggressive form of every idea, it has coated itself with a certain brusque and repellent dogmatism; but soldiers actually on campaign are not beautiful objects: fine feathers, necessarily, are reserved for parade. But anyone who studies it may see that it is an honest attempt to combat the arrogant narrowness of agnostic and religious dogmatism.

Alaska.

Those interested in Alaska will find in the August number of the *Scottish Geographical Magazine* an epitome of the knowledge we possess of that little-known land, from the pen of Israel C. Russell. Mr.

Russell has made three journeys into the country himself, and he completes his account from the narratives of others who have travelled on various business in the country.

It is difficult at first to realise that Alaska is eighteen times the size of Scotland, and that it has a general coast-line of 4,000 miles, or, if the shores of the bays and islands be measured, of 11,000—12,000 miles. The river Yukon divides the country in two approximately equal portions. It rises in the north-western part of Canada, flows westward across the territory, and falls into the Behring Sea. The Yukon has a length of 2,000 miles, and drains an area equal to 440,000 square miles. It has every appearance of great age, and has been repeatedly disturbed and obstructed by volcanic action. The vast mountain system follows the coast, and is a continuation of the Cordilleras. The culminating peaks of the range are found in Alaska, and are Mount Logan, 19,500 feet, and Mount St. Elias, 18,010 feet. A host of other peaks, several of which exceed 14,000 feet in height, make this region one of the most rugged and impassable in the world.

The region is one of volcanoes, and eruptions of more or less magnitude are of frequent occurrence. Mr. Russell notes that Shishaldin, in the island of Unimak, is a symmetrical cone with gracefully curving sides of the same type as Fuji-San, and rises to the height of about 8,000 ft. The wreath of smoke proceeding from the crater is a well-known beacon to mariners. On the shores of Behring Sea and of the Arctic Ocean the lofty mountains disappear and give place to the "tundras" or low, nearly-level, moss-covered plains of 70—100 miles broad. During the summer these tundras are swampy and covered with mosses, lichens, small flowering plants, rushes, and ferns. The most conspicuous plants are the dwarf willows, which attain a height of about 2 ft. The soil is a black humus, and at a general depth of a foot or thereabouts is always frozen. This frozen subsoil is not confined to the tundras, but in many places along the Yukon river the banks are formed of horizontal sheets of ice many feet thick, covered with moss, and supporting a dense forest of spruce. In hot summer, when the temperature is from 90°—100° F. in the shade, one may brush away the moss at his feet and find solid ice beneath.

In some places a depth of 300 feet of black, peaty humus has been observed, and Mr. Russell notes the interest of these deposits in connection with the formation of coal.

The glaciers of Alaska are, with the exception of those of Greenland, the largest and most instructive in the Northern Hemisphere. The largest is the Malaspina, on the south of Mount St. Elias. Here numerous ice-rivers meet and unite in one great sheet about 1,500 square miles in extent. Mr. Russell states that the glaciers of Alaska are slowly retreating, and that this recession has probably been going on steadily for the past 100 or 150 years.

Mr. Russell points out that Alaska is not so densely forested as is generally assumed,—and that, in fact, the tree region is confined to the south-eastern part of the country, most of the western portion being treeless. The article concludes with some observations on the inhabitants, and will well repay perusal as a general summary of information on a region at present but little known.

WE have received from M. Augusto Nobre the number for July, 1894, vol. i., no. 3, of that enterprising Portuguese publication, the *Annaes de Sciencias Naturaes*. Among other interesting matter it contains a

plate representing a bizarre-looking Selachian, captured at Buarcos. Mr. G. A. Boulenger, of the British Museum, who was consulted, refers it to *Squalus rostratus*, Marc, which Dr. Günther identifies with *Selache maxima*. Dr. Lopes Vieira, who gives an account of the specimen, is as yet by no means satisfied with the identification. From the figure and details given we are inclined to agree that if it is *Selache maxima* it is a very abnormal specimen.

In the last number of the *Journal of Anatomy and Physiology*, Mr. F. G. Parsons describes the tendo achillis of mammals, as being formed by a rope-like twisting of the fibres derived from the two heads of the gastrocnemius, the soleus, and the plantaris. The twisting which can be made out in man he regards as being partly due to the rotation of the limb in the embryo, but more largely to the position of the foot in the uterus of the mother.

Dr. A. Grandidier has issued two more volumes of plates illustrating the natural history of Madagascar. These are the fourth and fifth volumes of "Histoire Naturelle des Plantes," By H. Baillon. Once more we regret to observe that the plates are unaccompanied by any text or explanation.

In the *American Journal of Science* for August, 1894, Mr. O. L. Simmons, of Tufts College, Mass., writes on the "Development of the Lungs of Spiders." He comes to the conclusion that "the lung-book of the spider, and presumably of all arachnids which possess one, arises at first as an external structure upon the posterior surface of the abdominal appendages. These appendages sink in without any inversion or complications, in exactly the manner theoretically deemed probable by Kingsley, so that there can no longer be any doubt as to the exact homology existing between the lungs of the spider and the first pair of gills in Limulus."

The beautiful little fresh-water alga *Volvox* is well-known to lovers of ditch and pond life. We see from the July number of *The Botanical Gazette* that Mr. W. F. Shaw has found in a ditch at Palo Alto, California, an allied plant, which he considers to be and describes as a new genus, *Pleodorina*. The name indicates its affinity with *Eudorina*. It resembles *Volvox* more than does the latter genus in the number of cells composing the individual and the specialisation of certain cells for the purpose of reproduction. No sexual generation has, however, yet been observed.

OBITUARY.

GEORGE HUNTINGTON WILLIAMS.

BORN 1856. DIED JULY 12, 1894.

ONE of the most prominent members of the new school of Petrography in America has been prematurely removed in the person of Dr. G. H. Williams, Professor of Inorganic Geology in the Johns Hopkins University, Baltimore. Though only in the prime of life, he succumbed in July to an attack of typhoid fever; his health, it is supposed, having been enfeebled by his arduous duties last summer in the department of Mineralogy of the Columbian Exposition. Professor Williams was a native of Utica, N.Y., and graduated at Amherst College in 1878. He subsequently resided for a short time in Berlin, and then studied under Rosenbusch in the University of Heidelberg, where he obtained the degree of Ph.D. in 1882. The following year he became connected with Johns Hopkins University, and was Associate Professor there from 1885 to 1892, when he was appointed to the Chair he held at his death. The Professor was an attractive teacher, and numbered among his pupils many of the most successful workers in Petrography of the rising generation in America. Apart from his numerous original memoirs and the "Geology of Maryland," which we reviewed some months ago, his well-known "Text-book of Crystallography" will long keep his memory in the mind of students; and it is a distinct loss to science that his projected work on the microscopic structure of American crystalline rocks, in progress at the time of his death, should have been thus abruptly terminated.

THE death of M. DUTREIL DE RHINS has robbed France of one of her best geographers. M. de Rhins had reached Tibet in course of a journey from Turkestan to China and was murdered during a dispute over some horses. It is a satisfaction, though a poor one, to learn from the newspapers that the Chinese Government have promptly ordered the recovery of his papers and effects, and further suggested some reparation to his family. M. de Rhins was only forty-seven, but he has left a monument behind in his account of the geography of Central Asia.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

PROFESSOR Dr. E. RATHAY has become Director of the "önologisch-pomologischen Lehranstalt" at Klosterneuburg, near Vienna, and Dr. C. Rechingen provisory assistant to the botanical chair at Vienna. Dr. René Koehler has been appointed Professor of Zoology to the Faculty of Sciences of Lyon, and Mr. Wm. B. Marshall, of the New York State Museum, Professor of Biology in Lafayette College, Easton, Pennsylvania. The Kansas State University has divided the subject of Geology between two chairs, giving stratigraphical and physical geology to Professor E. Haworth, and leaving palæontology in the hands of Professor Williston.

DR. WILLIAM KING, the late Director of the Indian Geological Survey, is on his way home to England. Mr. C. L. Griesbach has been temporarily placed in charge, and as he is the senior official, it is probable that he will succeed to the vacated post.

MR. PETER ESSLEMONT, the head of the Scottish Fishery Board, has just died. It is to be hoped that his successor will have a competent scientific acquaintance with the matters with which he has to deal. No more suitable successor could be found than Mr. John Murray, of the "Challenger." We understand that strong representations in his favour have already been made by a large number of scientific men, and we need do no more than endorse their recommendation.

DR. FELKIN has been appointed to represent the Royal Scottish Geographical Society at the meeting of the German Congress of Naturalists and Physicians, to be held in Vienna during the month of September.

DR. JULIUS WIESNER has been receiving the congratulations of his past and present pupils on the completion of the twenty-fifth year of his professorship, the twentieth year of his presidency over the Institute of Plant Physiology of the Vienna University, founded by himself, and, thirdly, on the occasion of his safe return from a long journey of exploration to Java. The Professor was presented with an address and a silver medal of great artistic value.

PROFESSOR DR. PFITZER, of Heidelberg, has been raised to the dignity of Geheim-hofrath. A committee has been formed in Gera to erect a monument to the deceased ornithologist, Karl Theodor Liebe. The Zoological Garden of Berlin, having reached its jubilee, proposes to erect a monument to the energetic director, Dr. Heinrich Bodinus. Subscriptions may be sent to Mr. E. Weigel, 141 Frankfurterstrasse, Berlin, O.

A CURATOR is wanted for the Museum of the Institute of Jamaica, at Kingston, Jamaica. The appointment will be for three years, and is renewable at the end of that period. Applications should be addressed to the Secretary of the Institute of Jamaica, care of Messrs. H. Sotheran & Co., 140 Strand, London, W.C., not later than October 15, and should be marked "Curator, Institute of Jamaica." Further information may be obtained from Messrs. Sotheran. The salary offered is at the rate of £400 a year.

THE Swiney Lectures on Geology, under the auspices of the trustees of the British Museum, will this year again be delivered by Professor H. Alleyne Nicholson, who will take for his subject "The Making of the Earth's Crust." The lectures, which are on Mondays, Wednesdays, and Fridays, from the 1st to the 26th of October, will be delivered in the Lecture Theatre of the South Kensington Museum, since the proposed and much-needed lecture theatre of the Natural History Museum is still *in nubibus*. We further understand that this is the last year, for the present, in which the post will be filled by the very popular lecturer, Dr. Nicholson. For the ensuing year, at least, the well-known anthropologist, Dr. J. G. Garson, has been appointed Swiney Lecturer, and is expected to deal with the Geological History of Man. Meanwhile the Natural History Museum at Torquay is to have its Lecture Theatre built out of the £1,360 subscribed as a memorial to the late Mr. W. Pengelly.

THE new Norwich Castle Museum, of which we published a detailed description (vol. i., p. 692), is now nearly completed, and the removal of the collections is in active progress. The formal opening, by the Duke of York, is announced to take place next month.

NEXT month the local collections of the Natural History Society of Tromsö, Norway, will be removed from the present inconvenient house to a fine new stone building, which has been occupied during the summer by the Tromsö Centenary Exhibition. The Museum is of two storeys, with convenient work-rooms and an attic for stores, and has been built by the town and district, aided by a Government grant. The Tromsö Society, though small, displays remarkable vigour and has formed one of the finest collections of a local fauna and flora to be met with in any Continental Museum. It has also obtained a large series of fossils from Spitzbergen and Novaya Zemlya.

MR. A. HALY, the Director of the Colombo Museum, Ceylon, has issued his report for 1893. The chief object of this museum is to give a full representation of the products of Ceylon, both natural and artificial, and Mr. Haly points out how necessary it is that such a collection should be accompanied by a complete collection of the literature relating to Ceylon. He does not, however, find much value in the "Descriptions of Ceylon species published in various scientific periodicals." "In most cases," he says, "the descriptions of insects are unaccompanied by any figures, and are quite useless for the purpose of naming the species described, apart from authoritatively named specimens. *Entomologists, please note!* This museum has recently acquired very fine specimens of the Dugong—male, female and young. The female is 11 feet long, and is said to be the largest ever seen. A new kingfisher has been found on the island, apparently allied to the S. Indian species, *Alcedo bavani* of Walden. A new starred tortoise has also been found, which appears to be peculiar to Ceylon. A fish of the genus *Duymaeria*, is the first representative of that genus recorded from the Indian Ocean. A process of preservation by gum and glycerine is now used for fish, crustaceans, starfish, and holothurians. Instead of cork for insect boards, Mr. Haly uses a mixture of sawdust and cocoa-nut fibre.

MR. ZENKER, head of the station at Taunde in the Cameroons, has sent a large collection of bird-skins to the Royal Zoological Collection in Berlin. Dr. Stuhlmann also has sent many interesting novelties from Usaramo, in East Africa. Both these collections will be described in the *Journal für Ornithologie*. *A propos*, we learn that Dr. A. Reichenow's work "Die Vögel Deutsch-Ostafrikas" will be published in the autumn of this year by Dietrich Reimer, in Berlin.

THE Penzance Natural History and Antiquarian Society has issued its *Report and Transactions* for 1893-94. The most important paper is a critical list of the Lepidoptera of Cornwall and the Scilly Islands, by the President, Mr. W. E. Baily. Many local lists have already been published, but this is a more general summary

and includes the results of the author's own work during the last ten years. The Cornish Lepidoptera, as a rule, are larger than their congeners in other parts of Britain. Among brief notes, it is announced that a fine example of Risso's Grampus *(Grampus griseus)* was stranded near Marazion in April last, and that the skeleton is being prepared for description in the next part of the Society's *Transactions*. Details of the external characters of the animal are given in the present note. Meteorological observations used to be made at Penzance by Dr. Giddy, but no regular records were made between 1887 and 1893. The Corporation have now instituted systematic observations for the borough, and hope to bridge over this gap from private sources.

THE Malton Field Naturalists' and Scientific Society has commenced the issue of a monthly record of observations on local Natural History, entitled *Naturalist Notes*. This is one of the most vigorous of provincial societies, and has already formed a museum in many respects unique; but we doubt the wisdom of its latest venture, which strikes us as an unfortunate substitute for a well-digested annual report. At the recent annual meeting Professor L. C. Miall retired from the Presidency, and was succeeded by Mr. A. S. Woodward, of the British Museum.

THE Norfolk and Norwich Naturalists' Society has issued its transactions for 1893-94—vol. v., part 5. Mr. C. B. Plowright, M.D., has been appointed president for 1894-5. The retiring president, Mr. T. Southwell, F.Z.S., whose address appears in the present number of the *Transactions*, took for his subject the past and present history of the society, which is now in the twenty-fifth year of its existence. Many important and interesting contributions appear in the number, and there is issued with it a photograph of a specimen of the tropic bird *Phaëton æthereus* found dead at Cradley, Herefordshire.

THE Midland Union of Natural History and Scientific Societies held its seventeenth annual meeting at Ellesmere, Salop, on Friday and Saturday, August 3 and 4. The proceedings commenced with the council meeting on Friday afternoon, and eleven delegates from the various societies in the Union attended. The report of the executive committee was received, and after considerable discussion adopted. It was decided among other items that papers competing for the Darwin Medal should be published within the area of the Union, and that the Union should be made primarily a record office for the Natural History of the Midlands.

The new executive committee consists of the secretaries or a representative of each society in the Union, and six elective members as follow: Professors Allen, Bridge, and Lapworth of Mason College, Messrs. Chase, Hughes, and Wilkinson, with Professor Hillhouse and T. V. Hodgson as secretaries.

The annual meeting was held later, and accepted the recommendations of the council; the other officers were unanimously elected, viz., Brownlow R. C. Tower as president, and E. de Hamel as treasurer. It was also decided to admit photographic societies into the Union.

A conversazione was held in the evening at St. Oswald's College, and attended by upwards of 200 members and friends. There was a fair number of exhibits, chiefly local, and the college museum was thrown open. One paper was read by Mr. Bellamy, of Oxford, on the Migration of Birds.

Saturday was taken up by two excursions, one for archæologists to Chirk Castle and Valle Crucis, Llangollen, conducted by Mr. A. T. Jebb; the other for biologists, round the meres and peat mosses of the neighbourhood, led by Messrs. H. J. E. Peake and J. A. S. Jennings. Both excursions were well attended and very successful, notwithstanding the heavy rain which came down in the afternoon.

The botanists succeeded in finding most of the local plants, including *Nuphar pumila*, *Lobelia Dortmanna*, *Alisma natans*, *Utricularia minor*, etc., etc. The meres were all found to be "breaking," Ellesmere and Whitemere with *Glæotrichia echinata*, Colemere and Crosemere with *Aphanizomenon flosaqua*; in

spite of this *Leptodora hyalina*, *Daphnia jardinii*, and *Bosmina coregoni* were taken. The two latter do not appear to have been recorded from this district before.

Several members of the Union prolonged their visit over the Bank Holiday, and were able to make a more exhaustive search in certain areas with good results.

The hospitality and generosity of the Ellesmere people were unlimited, and afforded striking evidence of the capacity of a small local society.

DURING the week ending August 11 the members of the Geologists' Association visited Shropshire on their long excursion, under the direction of Professor Lapworth and Mr. W. W. Watts. The headquarters during the whole visit were at Shrewsbury. On Sunday, July 29, many of the members visited the Breidden Hills. During the rest of the week the programme was as follows:—Monday, Visit to the Wrekin, under the direction of Dr. Callaway; Tuesday, Visit to the Longmynd with Professor Blake; Wednesday, the eastern portion of the Shelve area, under the direction of Messrs. Lapworth and Watts, who were also the leaders on the following day, when it was arranged to visit the more westerly parts of the same district and to see the laccolite of the Corndon. Unfortunately heavy rain delayed the start, so that the former part of the programme had to be omitted. On Friday the famous Onny Section was examined, under the guidance of Professor Lapworth and Rev. J. D. La Touche. On Saturday morning the Mayor of Shrewsbury received the members at the Museum, which is of considerable interest. Later in the day Buildwas Abbey and the Severn Gorge at Coalbrookdale were visited, and the excursion brought to a close.

A valuable pamphlet on the "Geology of Southern Shropshire," by Lapworth and Watts, was issued to the members, and will shortly be published in the *Proceedings* of the Association.

WHILE we are finding coal and ironstone at Dover, and while Professor Boyd-Dawkins is advocating a search for coal in Oxfordshire, arguments similar to those employed by English geologists for England have led the geologists of New South Wales to the discovery of coal under Cremorne in Sydney Harbour. Of late years the existence of coal under Sydney has been regarded by local geologists as almost a certainty, the only divergence of opinion being about the depth at which it was likely to occur. A seam of coal has now been found by boring in the above locality, at a depth of 2,801 and 2,917 feet, which varies in thickness from 7 feet 3½ inches to 10 feet 3 inches, and contains only minute bands of shale. The coal is good and especially suitable for steaming purposes. An account of the various borings is given by Messrs. T. W. E. David and E. F. Pittman in the *Records of the Geological Survey of New South Wales*, vol. iv., pp. 1–7.

AT a meeting of the Linnean Society of New South Wales on June 27, Mr. Hedley exhibited the shelly tubes of *Kuphus arenarius*, collected by Mr. Brazier on a coral reef at Florida Island, Solomon Islands. As the affinities of this animal are somewhat problematical, visitors to the Solomons should endeavour to procure it.

PRINCE HENRI D'ORLEANS arrived at Tamatave some weeks ago, and proceeded north to the place where Mr. Muller, the French explorer in Madagascar, met his fate a few months ago.

THE library of Professor Carl Vogt, of Geneva, has been purchased by the Government of Roumania.

MR. FRANK FINN, M.A., F.Z.S., has been appointed First Assistant-Curator in the Zoological Department of the Indian Museum at Calcutta.

DR. HENRI FILHOL has been nominated to the chair of Comparative Anatomy at the Natural History Museum, Paris, lately vacated by the death of Mr. Georges Pouchet.

CORRESPONDENCE.

The Art of Cataloguing.

Sir,—Whoever reviewed Mr. Seward's Catalogue of the Mesozoic Plants in the British Museum in your August number (p. 147) is no doubt a competent botanist, but can know very little about museum work. In cataloguing a series of fossils, whether plants or animals, "a bare quotation of the register numbers," would by no means serve the purpose, though your reviewer seems to think it would. In the first place, register numbers have an unfortunate habit of coming off, and it is exceedingly useful to have some subsidiary means of identifying the specimens. Secondly, a register number alone conveys no information to a worker at a distance; and it is surely advisable that such an one should be informed whether a specimen is a mere fragment, or shows some structural features, so that when he comes to the Museum he may be able to say at once exactly which specimens he wishes to see. At the worst, the system adopted by Mr. Seward, as by some other cataloguers before him, can do nothing more terrible than take up the space of which your reviewer seems so jealous; and I venture to submit that the space is by no means wasted.

Geological Department,
British Museum.

F. A. Bather.

Nascent Elements.

With regard to the question of nascent elements, has it been considered whether the conditions which apply to several gases in the above state also apply to them when solid, *i.e.*, if hydrogen could be evolved under conditions consistent with a solid state, what peculiarities different to solid hydrogen, evolved and then solidified, would it exhibit? Also when, say, carbon is freed from a compound does *it* exhibit any nascent phenomena?

Would not a solution of this problem shed light on the supposed action of chlorophyll in the formation of starch from CO_2 and H_2O in plants?

Hoping you will discuss this question in your valuable magazine.

Jno. E. Hardman.
E. P. Greenhalgh.

78 Chorley Old Road, Bolton.

[Whatever may be the conditions requisite for the evolution of solid hydrogen, those existing in the leaves of living green plants must be so vastly different as to render comparison useless. As regards carbon, it is extremely improbable that the free element enters at all into the process of formation of organic matter from carbon di-oxide and water; it is generally supposed that the CO_2 loses only half of its oxygen, the remaining group (CO) combining with the hydrogen of the water to form a compound containing the three elements in these proportions (CH_2O). This compound is a much simpler one than starch, which is only deposited by the protoplasm when the amount of carbohydrate material present in the cell is in excess of that needed for the immediate use of the plant.—Editor.]

Natural Selection.

In answer to Professor Henslow and "Entomophilos," I may perhaps state that the various facts on which my statements were based are contained in my own and other papers, to which I must refer them. The word "apparently" in the phrase they object to might be replaced perhaps by "possibly" or "probably."

Natural Selection, like Inheritance of Acquired Characters, is a convenient working hypothesis, and has still to be *disproved*.

The first part of Entomophilos's letter I do not follow. By the statement I made I did not intend to imply more than a resemblance between our flora and those of Northern Europe. It has been abundantly shown that the circle of visitors to a given flower is of a very definite nature, and from the fact that in our fauna there is a "natural preponderance of flies over other insects," it does not follow that there will be a similar preponderance of flies among the insect-visitors of certain flowers.

JOHN C. WILLIS.

DISCUSSIONS AT THE BRITISH ASSOCIATION.

I WAS present at the discussion on Natural Selection in the Subsection of Zoology, and feel strongly that the method adopted was not that most calculated to lead to useful result. After a number of interesting objections to Natural Selection had been urged, the most notable defenders of Darwinism contented themselves with implying either that their adversaries did not understand what they were talking about, or that they were unacquainted with the writings of Darwin. I wish to support the judicial proposal of Sir Edward Fry, that these discussions should be conducted like a case in a court of law, and that the counsel on either side should be kept to the point at issue. I venture to suggest that, instead of lumping together a somewhat heterogeneous set of papers, and then taking a discussion on the lot, it would be well for the secretaries of the organising committees to settle beforehand on definite subjects, or, better still, on definite propositions, and then to get specialists to contribute arguments from their own knowledge on either side of the case

GALLIO.

CORRIGENDUM.

C. H. B. WOODD, of Roslyn, Hampstead, calls our attention to an unfortunate slip in the July number. NATURAL SCIENCE, vol. v., p. 16, 6th line from bottom of page; for "were interchangeable terms," read "were not interchangeable terms."

TO CORRESPONDENTS.

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NOTES AND COMMENTS.

Protozoa from pre-Cambrian Rocks.

ONE of the most interesting contributions to geological and biological science of late is Monsieur L. Cayeux's report on the supposed Radiolaria and Foraminifera from the pre-Cambrian rocks of Brittany. About two years ago we alluded to Dr. Charles Barrois' discovery of these remarkable fossils, and announced the preparation of a detailed report by M. Cayeux. The results have now appeared in the *Bull. Soc. Géol. France* (ser. 3, vol. xxii., pp. 197–228, pl. xi.), and in the *Comptes Rendus* (vol. cxviii., pp. 1433–1435); and there is a valuable, cautiously-worded notice by Dr. George J. Hinde in the last number of the *Geological Magazine* (Dec. 4, vol. i., pp. 417–419). It appears that the supposed organisms occur in thin layers of flinty rock interstratified with the pre-Cambrian schists, and they are irregularly distributed, sometimes being met with singly, sometimes grouped together in clusters. They are much more minute than any known Radiolaria, the average size of those of the Palæozoic rocks being about seventeen times as great. The majority are simply spherical in form, some are ellipsoidal, and a few are inflated or bell-shaped. Some specimens exhibit radial spines, while others display an inner concentric shell connected by rays with the outer test; and nearly all of the best preserved examples have a lattice-like structure. M. Cayeaux recognises forty-five different forms of these fossils, and assigns the majority of them to known genera, while the whole may be comprised in the subordinal divisions of Radiolaria established by Haeckel.

Many of the minute bodies found with the Radiolaria, and originally supposed to be of the same nature, are now regarded by M. Cayeux as Foraminifera; and these also, it is interesting to note, differ from later forms in being much more minute. Some of the shells are

simple, others compound, and the latter consist of from two to seven spherical or ovoid chambers of different sizes aggregated together. The walls are very finely perforate, and some of the chambers are provided with one or more short blunted spines.

Much discussion will doubtless arise in reference to these new problematical bodies, and, so far as we can judge, there is some hope that they will stand the test of criticism better than the ill-fated *Eozoon*. Much confirmation, however, is yet required, and we look forward with interest to the discovery of similar forms in the pre-Cambrian rocks of our own island.

Primæval Life.

Theories as to what the nature of the earliest life must have been are still flourishing, and in the August number of the *Journal of Geology* Professor W. K. Brooks has a fascinating, interesting, and exceedingly suggestive, if not absolutely convincing, article on the subject. The paper bears so directly on the discussions at the recent meeting of the British Association as to the distribution of marine animals, that we give here an outline of its main points, which we hope will send our readers to the original. In many respects it reminds us of the views of the late Professor Moseley.

The variety, distinctness, and advanced structure of the animals found in the very lowest fossiliferous rocks have long puzzled geologists, and have seemed, on the theory of evolution, to necessitate an enormous backward extension of life before the beginning of the Cambrian period, although no tangible proof of the existence of such life is forthcoming in rocks where it ought to be found. It is this difficulty that Professor Brooks attempts to explain.

In a few vivid pages we are shown how the ocean is almost destitute of plant-life, how its flower gardens are stocked only with animals, and how its very herbs and lichens are corallines and sponges; we learn how the vast animal armies of the sea attack and devour other animal armies, but have never a plant for their forage; and thus, by descending steps, we are brought to the conclusion that "the basis of all the life in the modern ocean is found in the micro-organisms of the surface," lowly animals and plants so abundant and prolific that they meet all demands. These minute pelagic creatures must have been the first to exist, and where they first appeared there they have ever since remained, undisturbed by varying environment or the stress of competition, and therefore retaining their primæval simplicity.

The early pelagic fauna gave rise to a few simple types, such as we now know only in the larval forms of higher animals. But further development never took place here.

On the contrary, we recognise that all highly organised marine animals are products of the bottom or of the shore, or, as in the case

of the whales, of the land; even the pelagic animals of higher grade have come back again to the surface from ancestral homes elsewhere.

There was, however, a time when the bottom and shores of the ocean were untenanted; when, perhaps, physical conditions made their population impossible. Life at that time occurred only in simple forms and near the surface of the central sea. At last the bottom was discovered and colonised. The earliest settlements were not in shallow water, where food supply was scanty and mixed with sediment, nor in the great depths as yet unfavourable to life; but it is inherently probable, as well as confirmed by palæontological evidence, that the bottom life first found a footing in the deep water around elevated areas. Colony after colony may, it is true, have been "swept away by geological change like a cloud before the wind"; but when once the outlines of our modern continents were blocked in, then "the first fauna which became established in the deep zone around" them "may have persisted and given rise to the modern animals."

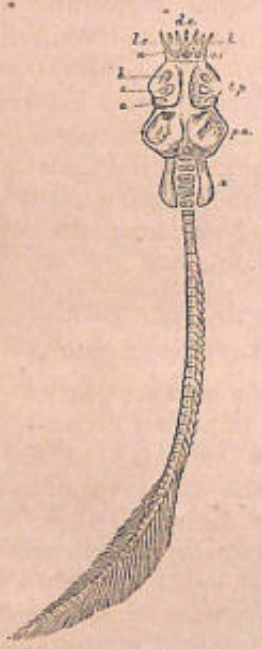
The sudden and enormous change of conditions accompanying this change of residence soon made itself felt on the creatures of the deep. The results were increase of asexual multiplication and the establishment of colonies, crowding followed by competition and more exacting selection, the acquisition of hard parts, and increase of size. Progressive evolution along these lines would have required no lengthy period to develop, out of those pelagic and soft-bodied ancestors that geologists will never discover, a fauna in all respects similar to that which geologists have already discovered in the Lower Cambrian rocks.

Thus those æons which the geologist has loved to imagine as his own, which the evolutionist has demanded, but which Lord Kelvin has denied and Lord Salisbury ridiculed,—they for Professor Brooks are but the baseless fabric of a dream, unproved, unreal, and unneeded.

The Primæval Lampreys Again.

Even of the earliest animals which possessed well-developed hard parts, we still know very little; and it is strange how peculiarly accidental are the few important discoveries that have been made. The little Devonian lamprey *Palæospondylus*, for instance, is known to us merely through the circumstance that the Caithness quarrymen happened to alight upon a little patch of good flagstone at a remote spot named Achanarras. It has been found hitherto only in that one quarry and is restricted to one narrow layer of rock. *Palæospondylus*, however, revolutionises our old ideas of the Class Marsipobranchii, and we have already published communications in Natural Science pointing out its significance. Quite lately still more information has been obtained on the subject, and Dr. Traquair's latest restoration of

the skeleton, taken from the newly-issued part of the *Proc. Roy. Phys. Soc. Edinburgh* (vol. xii., pp. 312–320, pl. ix.), is reproduced below for comparison with those previously attempted. It now appears that the animal had a median opening or ring, surrounded with feelers, at the front of the head; this being presumably comparable with the nose of a modern lamprey. It is also evident that the fin-supports at the end of the tail are longer than previously suspected; and those at



Palæospondylus gunni; new restoration by Dr. R. H. Traquair, somewhat enlarged. *d.c.*, dorsal cirri; *l.c.*, lateral cirri; *n.*, nasal opening; *p.a.*, periotic region of skull; *t.p.*, anterior part of skull, with indents marked *a.*, *b.*, and *c.*; *v.c.*, ventral cirri; *x.*, problematical (? branchial) plates.

least of the dorsal aspect are proved to bifurcate towards the top, exactly as in a lamprey. That *Palæospondylus* is a primæval Marsipobranch is thus suggested still more forcibly than ever, and the next important step is to discover the links between this little organism and some of its yet more remarkable contemporaries, *Pteraspis* and *Cephalaspis*.

The Armoured Fore-runners of the Chordata.

Remarkable additions to our knowledge of the last-named animals, now commonly assigned to a subclass termed Ostracodermi or Ostracophori, have lately been made by Dr. J. V. Rohon and Dr. Friedrich Schmidt in the publications of the St. Petersburg Imperial Academy of Sciences. The descriptions, both of the form of the shields of *Trematapsis*, *Auchenaspis* (*Thyestes*), and fragments of allied genera, are supplemented by a detailed, well-illustrated account of

their minute structure. Moreover, the explorations of Herr Simonson in the Upper Silurian Limestone of the Island of Oesel, in the Baltic, have recently furnished most of the principal European museums with good specimens. The fossils are thus within reach of most workers. The greatest sensation of late, in connection with this subject, however, is the announcement by Mr. William Patten, in the *Anatomischer Anzeiger*, that certain parts of the shield of the existing king-crab, *Limulus*, are almost identical in intimate structure with a certain layer in the shield of the Devonian *Pteraspis*. We have not yet had the opportunity of verifying the observation; but if the facts can be established Palæontology will, after all, furnish some support to the idea that there is a close connection between the primitive Chordata and the so-called Arachnids of the *Limulus* tribe. We prefer, however, at present to leave the question of the ancestry of the Chordata to the Embryologists, and we cannot do better than allude next to recent researches in this direction.

The Ancestry of the Chordata.

The researches of the past ten years have shown a marked tendency towards a closer and closer *rapprochement* between those three groups of enterocœlous animals, the Echinoderma, the Enteropneusta, and the Chordata. The idea of an affinity between the two first groups dates from the time when Agassiz in America and Metschnikoff in Europe discovered that *Tornaria*, which had been regarded by Johannes Müller as a starfish larva, was in reality the larva of the worm-like *Balanoglossus*. Metschnikoff even went so far as to homologise the proboscis of the adult *Balanoglossus* with an ambulacral tentacle, and the proboscis-vesicle he regarded not simply as a rudimentary, but as an actually vestigial representative of the water-vascular system of the Echinoderm. It is chiefly to Mr. W. Bateson, on the other hand, that we owe the establishment of the Chordate affinities of the same remarkable animal; while the whole progress of Echinoderm research, in the able hands of Ludwig, Semon, Bury, and others, has revealed a continually-increasing series of fundamental resemblances between the Echinoderm and the Chordate types. The idea of a real affinity between the three groups has taken deep root, especially in England, and the somewhat ill-founded opposition of Spengel to the view of the Chordate relations of *Balanoglossus* has effected little beyond the strengthening of the position of those who dissent from his conclusions.

Mr. Garstang expounded a theory of the inter-relationships of these groups, which he takes to present a simple and consistent explanation of the structure and development of *Balanoglossus* and the lower Chordata.

Recent research has shown that the larvæ of Echinoderms are all derivable from one common type, which can be imagined as a

simple *Auricularia* larva, possessing a terminal blastopore, a continuous circumoral (longitudinal) ciliated ridge, with a subjacent central nervous system, and an adoral ciliated band, which extends round the anterior and lateral margins of the mouth, and is drawn inwards and backwards in the mid-ventral line to form a ventral œsophageal ciliated loop.

The *Tornaria*-larva of *Balanoglossus* is equally derivable from the *Auricularia*-type; and now Mr. Garstang suggests that the true Chordata also trace their origin from the same primitive pelagic form, thus following out in a definite manner the interesting views of Brooks in this direction. (See NATURAL SCIENCE, vol. iii., p. 222.)

The basis of his theory is the homology which he proposes between the medullary folds of vertebrate embryos and the paired circumoral longitudinal ridge of *Auricularia*, the homology being founded on the identity of the relations of these two sets of structures to the mouth, the apical pole, the blastopore, and the central nervous system. In the Chordata the ciliated medullary ridges eventually fuse to form the medullary or neural canal, thus enclosing the sense-organs of the apical pole (optic pits) and the blastopore.

In the Echinoderma, as was shown by the beautiful researches of Semon, the central nervous system becomes segregated at an early period from the ciliated ridges, and migrates ventrally to form the circumœsophageal nerve-ring, while the ridges eventually atrophy and the blastopore persists as the anus.

An especial importance naturally attaches to the relations of the nervous system and ciliated bands of the developing *Balanoglossus*. The diagrams exhibited at the reading of the paper, and the new point of view from which the author regarded the course of the nerve-tracts in *Balanoglossus*, undoubtedly revealed some most significant, if not decisive, evidence in favour of his theory. Unlike his predecessors, Mr. Garstang holds that the dorsal nerve-cord in the trunk of *Balanoglossus* is not the morphological continuation of the collar-cord, but is —along with the ventral cord—a nerve-tract that is entirely confined to the group Enteropneusta, developed *pari passu* with the posterior elongation of the body. He holds, on the other hand, that the collar-cord, though ontogenetically median and unpaired, has been phylogenetically derived by the fusion of two lateral cords, and he finds support for this view in its divarication posteriorly to form the nerve-ring which runs along the posterior rim of the collar. The collar-ring is upon this view homologous with the periblastoporal portion of the nerve-plate in vertebrate embryos, and with the posterior transverse loop of the central nervous system of *Auricularia*.

As to the medullary folds of the collar which give rise to the neural canal, it was pointed out that, according to the researches of Morgan on *Tornaria*, the medullary folds do not cease at the posterior limit of the collar-cord, but bend round the sides of the body and *persist through life as the posterior rim of the collar*, the area of separation

in the mid-dorsal line being reduced and obliterated by the union of the longitudinal portions of the folds to form the neural canal. Attention was also drawn to the facts that the medullary folds of *Tornaria* not only arise in close relation with the degenerating posterior portion of the ciliated band, but take a course which is identical with it; and that the region of the neural tube is just that area where the convoluted ciliated bands approximate most closely in the dorsal region of the body.

The position of the gill-slits in *Balanoglossus* behind, and in Chordata in front of, the neural loop was attributed to that peculiar change of position which the primitively cæcal gill-pouches of *Balanoglossus* undergo during the period of metamorphosis, as observed and described both by Agassiz and Morgan.

Pending the appearance of the author's complete account, we need say no more here, except, perhaps, that Mr. Garstang finds in the peculiar adoral ciliated band of *Auricularia* the exact precursor of the combined peripharyngeal bands and the marginal bands of the endostyle of the Tunicata and *Amphioxus*.

The Ancestry of Modern Mammalia.

In the *American Journal of Science*, Professor O. C. Marsh is continuing his valuable brief notes on the remains of Tertiary mammals from the western regions of North America, while Professors Cope, Scott, and Osborn, and Mr. Earle, are engaged upon more elaborate memoirs. Progress is thus rapid in the northern half of the New World, and Mr. Lydekker has spent the autumn in further work upon the remarkable extinct Tertiary mammalia, chiefly sloths and armadillos, in its southern half. Of greatest interest, however, is the announcement that the general public in North America will soon have the opportunity of seeing these extinct quadrupeds displayed in an easily comprehensible manner, so that they can judge of the beasts for themselves. Except at Princeton, the collections of mammalian remains have hitherto been stowed away almost like goods in a store; but the American Museum of Natural History has now for some time secured the services of several good collectors, including Mr. J. B. Hatcher, and numerous entire skeletons of the most typical Tertiary animals are, as a result, at present being mounted for the exhibition rooms. Professor Osborn, of Columbia College, is superintending the work, and as the mounting proceeds, drawings of the actual specimens will be published.

European Pleistocene Mammalia.

In Europe similar studies are also progressing favourably, and an enterprising dealer, we understand, has been making extensive excava-

tions in the Pliocene deposits of Pikermi and the Island of Samos. Dr. Forsyth Major's memoir on the Pliocene Mammalia of Samos will soon appear in the *Palæontographica*, and his collection from Olivola, in the North of Italy, is now being extricated from matrix at the British Museum.

Much greater progress, however, has been made of late in the study of Pleistocene Mammalia. M. Edouard Harlé has been especially energetic in the South of France, and has found much of interest in the caverns of Haute Garonne (*Bull. Soc. Géol. France*, ser. 3, vol. xxii., pp. 234–241, and *L'Anthropologie*, vol. v., pp. 402–406). His most interesting discoveries consist in further evidence of the occurrence of the striped hyæna, and of the elk in Southern France. Professor Lartet, thirty years ago, was of opinion that the striped hyæna migrated northwards from Africa beyond the Pyrenees, at the time when Europe and Africa were connected by barriers; but considering that teeth almost identical with those of the striped hyæna have also been found in the Pliocene both of France and England, M. Harlé now regards it as much more likely that the animal met with in the caves was a direct descendant of its predecessor in the European region. The confirmation of the discovery of the elk in south-west France is interesting, because the evidence has not previously been very satisfactorily described. Mr. Harlé now gives a figure and description of an upper molar tooth, which he considers to be unmistakable.

Pleistocene Mammalia from the Thames.

In England, one of the most interesting discoveries of Pleistocene bones lately made is that described by Messrs. J. R. Leeson and G. B. Laffan in the last number of the *Quart. Journ. Geol. Soc.* These authors have found a bed of loam deep in the Thames gravels at Twickenham, and here, or immediately above it, there occur the remains of the reindeer, bison, and oxen. The only known fragment of the Saiga Antelope found in Britain was also obtained from the same neighbourhood. One reindeer antler and a skull of bison are especially fine, and these specimens have been presented by Dr. Leeson to the British Museum. The most interesting feature of the discovery, however, is the possible occurrence of the Celtic shorthorn in association with the above-mentioned remains. We are by no means convinced that the authors have made out their case for this modification of current views; for all discoveries hitherto have tended to show that the shorthorn was introduced into Britain by the Neolithic people who invaded the country after it became an island. Nevertheless, the mode of occurrence of the bones at Twickenham is suggestive of their all belonging to one period, and there seems to be no doubt that some specimens do belong to the small ox in question.

Acclimatised Rats and Cats.

A new interesting case of the gradual adaptation of animals to their surroundings is quoted in a recent number of *Public Opinion* from the *Pittsburg Dispatch*. It appears that in the cold-storage warehouses in Pittsburg there were originally no rats or mice. The temperature in the cold rooms was too low. The keepers soon found, however, that the rat is an animal of remarkable adaptability. After some of these houses had been in operation for a few months, the attendants found that rats were at work in the rooms where the temperature was constantly kept below the freezing point. They were found to be clothed in wonderfully long and thick fur, even their tapering snake-like tails being covered by a thick growth of hair. Rats whose coats have adapted themselves to the conditions under which they live have domesticated in all the storage warehouses in Pittsburg. The prevalence of rats in these places led to the introduction of cats. Now, it is well-known that pussy is a lover of warmth and comfort. Cats, too, have a great adaptability to conditions. When cats were turned loose in the cold rooms they pined and died because of the excessive cold. One cat was finally introduced into the rooms of the Pennsylvania Storage Company which was able to withstand the low temperature. She was a cat of unusually thick fur, and she thrived and grew fat in quarters where the temperature was below 30 deg. By careful nursing a brood of seven kittens was developed in the warehouse into sturdy, thick-furred cats that love an Icelandic clime. They have been distributed among the other cold storage houses of Pittsburg, and have created a peculiar breed of cats, adapted to the conditions under which they must exist to find their prey. These cats are short-tailed, chubby pussies, with hair as thick and full of under fur as the wild cats of the Canadian woods. One of the remarkable things about them is the development of their "feelers." These long stiff hairs that protrude from a cat's nose and eyebrows are, in the ordinary domestic feline, about three inches long. In the cats cultivated in the cold warehouses the feelers grow to a length of five and six inches. This is probably because the light is dim in these places, and all movements must be the result of the feeling sense. The storage people say that if one of these furry cats is taken into the open air, particularly during the hot season, it will die in a few honrs. It cannot endure a high temperature, and an introduction to a stove would send it into fits.

Living Specimens of Protopterus.

Those zoologists who attended the conversazione given by the local committee to the members of the British Association in the University Museum, had the opportunity of seeing a zoological exhibit of very unusual interest. Dr. H. O. Forbes, the Director of Museums, Liverpool, exhibited on behalf of the Derby Museum living specimens

of the African dipnoid *Protopterus annectens*, encased in the blocks of mud in which, during the dry season, these singular fishes bury themselves. They had been obtained for the museum from one of the West African rivers, at a distance of nearly 500 miles from the sea, through the exertions of Mr. Ridyard, of Liverpool. The fish had been in their mud-cocoons for over five months, having been kept in England for more than two months. Dr. Forbes prepared in the large lecture room of the University Museum glass jars of tepid water and into these the cocoons were placed in the presence of visitors. The hard rock-like clay became softened, and one of the fish soon came out in a lively condition and swam about freely. Mr. Forbes stated that while some specimens in his possession had emerged in five minutes, others had taken at least an hour. Of course, living specimens have been brought to Europe in this condition before now —there are at present a number of specimens in the reptile-house of the Zoological Gardens—but we fancy that very few of the members of the Association have had before, or are likely to have again, so interesting a practical demonstration of the habits of these animals.

The Plague Bacillus.

An authoritative account of the bacillus of the Oriental plague is at length to hand from the pen of its discoverer, Professor S. Kitasato of Tokio. We learn from the *Lancet* of August 25 that it is a capsuled bacillus, scarcely, if at all, motile, and exhibiting a tendency to bipolar staining. It is present abundantly in the blood and internal organs, and especially in the spleen and bubonic swellings of victims of the disease. It can be grown readily on suitable cultural media at the temperature of the human body, and rodents inoculated with pure cultures die in from two to five days, with symptoms and pathological changes resembling those of the natural disease in man. Rodents seem, indeed, to be highly susceptible, rats and mice having died in large numbers in Hong Kong during the epidemic, as they are related to have done during the historic visitations of the disease in this country.

All these facts are in complete harmony with what might have been anticipated, and the excellent work done by Professor Kitasato in Koch's laboratory in Berlin should leave little room for doubt that his discovery is a genuine and an important one. As was natural, however, the supremacy of this particular organism has not been allowed to pass unchallenged, and other claimants for the doubtful honour of being the real cause of the plague have been put forward by Dr. Yersin and others. Final judgment as to the respective merits of the rival microbes must be postponed till fuller details are forthcoming. No doubt securely bottled samples will in time arrive in this country, where it is to be trusted they will be kept under lock and key. Professor Kitasato has, however, already shown that the powers

of resistance of his plague bacillus are not very high, as it seems to form no spores. Four days desiccation kills it, as does exposure to sunlight for three or four hours in the dried condition. The temperature of boiling water is fatal in a few minutes, and a one per cent. solution of carbolic acid kills in an hour. So that we shall know in a measure how to deal with this bacillus in case it ever does break loose among us. It seems, moreover, that the plague is essentially a filth disease, and would stand little chance in presence of decent modern sanitation.

Small-pox.

The recent outbreak of small-pox in London, though the prompt measures taken seem to have been effectual in confining it within moderate limits, may serve to remind us how difficult a thing it is to fight against an enemy whose exact nature we do not know. Empirically we have learned much as to the prophylaxis and treatment of the disease, and now that experiment has demonstrated that vaccinia is merely small-pox modified by transmission through the cow, we have a rational scientific basis for the practice of vaccination. Yet, though small-pox is always with us, and in spite of the fact that experimental inoculation of mankind with its attenuated virus is rendered compulsory by law, the actual nature of the virus remains still under dispute. It is rather the fashion nowadays, when no bacterium can be demonstrated in a given specific disease, to invoke the Protozoön. That Protozoa may cause disease we know—witness malaria—but the utmost caution is required in interpreting the appearances to be seen in epithelial cells stained with fancy dyes, and the protozoan origin of small-pox, as of cancer, rests at present on a very slender foundation. And indeed there is now some evidence that the virus of small-pox may after all be a spore-bearing bacillus which has hitherto eluded observation by its habit of turning into an unstainable spore at an earlier period than that at which anyone had thought of looking for it. Both Dr. Klein and subsequently Dr. S. Monckton Copeman have independently discovered that such a spore-bearing bacillus is to be demonstrated in the lesions of small-pox and vaccinia, if search be made at a sufficiently early stage of the disease, and though cultivations have uniformly failed, there seems at least to be a possibility that this organism may prove to be the true cause of small-pox.

In a supplement dated 25th May last the *Leeward Islands Gazette* reports a paper read by Mr. C. A. Barber at the inaugural meeting of the St. Kitts branch of the Leeward Islands Agricultural Society. The paper deals chiefly with a disease affecting the sugar-cane, known as the Rind-fungus (*Trichosphæria Sacchari*), and described as the most

dangerous and widespread enemy of the cane cultivation of the present day, occurring in greater or less abundance probably everywhere where the plant is grown. Associated with the fungus is a moth-borer caterpillar; in fact, the insect seems to be the first cause of the disease, as the fungus cannot penetrate the sound plant. The young grubs immediately after hatching upon the leaf-blades make their way down to the protecting leaf-sheath, where they burrow in the soft inner tissues till strong enough to penetrate the cane. The burrows of the moth-borer give entrance to the fungus, and are noticeable as centres of infection. The fungus then rots the cane Various suggestions are made for the destruction of the moth-borer, and the selection of canes capable of resisting the attacks of the fungus.

Let the humble student of British botany rejoice. A month or two ago we referred to the Brambles as the happy hunting grounds of a little band of clerics. But the Hawkweeds seem even more remunerative, for in the *Journal of Botany* for August, Mr. F. J. Hanbury, who is tardily and expensively monographing the genus, diagnoses no less than seven new species and two-and-twenty new varieties. As their names indicate, two, *Hieracium surrejanum* and *H. cantianum*, grow close at home, in fact, within fifty miles of London. We do not dread their extermination, in spite of such proximity, except by accident, for like the Brambles and Roses these species are beyond the ken of the ordinary collector, and to quote Mr. Hanbury himself in his mild censure of his Scandinavian colleagues, we "feel it necessary to express a grave doubt as to the practical utility of of such subdivision," and our fear that the study of these genera will be rendered an impossibility, except to the few specialists who may devote their entire lives to their elucidation.

A note on the germination of seeds in sawdust in the *Botanical Gazette* for August will be of interest to teachers of Botany. When all goes well there is no better way of getting clean straight roots for class or experimental purposes; but, unfortunately, the sawdust has frequently a detrimental effect, and roots of plants germinated therein are seen to be in a very unhealthy condition. This, Mr. Stone points out, is due to the presence of tannin, and if only care be taken to select dust free from this, results will be quite satisfactory. Roots grown in sawdust containing tannin show a reddish colouration, are crooked, and much reduced in size, and this abnormal appearance may be brought about by saturating any sawdust which has been proved suitable for normal growth with a one per cent. solution of tannin. Oak and chestnut dust are, for this reason, to be avoided, while that obtained from Conifers has no prejudicial effect. The writer adds: "We have never experienced difficulty with any sawdust which failed to give the tannin reaction, and during the past year we have used the same sawdust continually without changing."

In the same journal is a *résumé*, by J. C. Bay, of observations and experiments in the formation of crystals of ice on plants. The splitting of wood and the appearance of ice-crystals in the fissures are phenomena well known to tree planters. As a result of the cold, the tissues of the whole plant contract, and consequently their turgescence is much diminished, as well as the permeability of the cell-walls to water. The contents of the peripheral ends of the medullary rays freeze, expand, and are pressed forward, causing a split in the stem at the point of least resistance. The ice forms a layer covering the whole surface of the wound. The internal pressures continue to supply water, by which the ice-sheet is continually augmented.

An ancient oak formed the subject of an interesting exhibit at one of the last meetings of the Linnean Society, when Mr. Carruthers showed some photographs of an old tree which still grows at Conthorpe, in Yorkshire. Prints prepared some hundred years ago show no departure from the normal habit of a fine oak, but recent photos illustrate a remarkable change in mode of growth. The successive ramifications of the thick lower branches no longer spread in all directions, but the latter produce a number of erect shoots giving the appearance of young trees growing from the horizontal main branch, which may be compared with a rootstock sending up vertical shoots, as happens in the Solomon's seal, species of *Polygonum*, and many creeping plants.

German botanists are still busily working at the tropical African flora, to which the recently-issued number of Engler's *Botanisches Jahrbuch* (vol. xix., parts 2 and 3) supplies a further contribution. This includes a large number of new Labiatæ, Orchids, Thymeleaceæ, and others. In the *Journal of Botany* for September, South African botany is represented by a paper by R. Schlechter, who describes some new species of Asclepiadaceæ from Natal and elsewhere.

Some time ago we gave an account of Magnin's work on the flora of the Jura Lakes. During the summer of 1893, under the auspices of the Michigan Fish Commission, the flora and fauna of Lake St. Clair have been studied in a similar manner. Mr. A. J. Pieters, the botanist of the party, states that the flora was found arranged in zones, limited by the depth of the water, each with certain characteristic plants, but Magnin's Nupharetum did not exist.

News has arrived from Mr. G. F. Scott Elliot, in a letter dated Ruwenzori, the 24th of May, in which he states that he has been on the mountain for nearly two months, collecting and exploring under

somewhat unfavourable conditions. In spite of fever, which has kept him to his bed for half the time, Mr. Elliot has been able to get to 11,000 feet, where he found a violet and a tree heath. A deciduous forest extends from 7,600 to nearly 9,000 feet, above which are bamboos. The forest is damp to a degree, a cloud usually hanging over it the whole day, making the processes of collecting and drying rather difficult.

The English Dialect Society is anxious to include the supplement to the *Dictionary of English Plant Names* among its publications for 1894. Those who have any additional names or notes are invited to send them as soon as possible to Mr. James Britten, 18 West Square, Southwark, London, S.E.

A collection of twenty-three human skulls made by Dr. Machon in the caves and old cemeteries of Patagonia, is the subject of a detailed illustrated description by Dr. R. Verneau in the last number of *L'Anthropologie* (vol. v., pp. 420–450). Seven specimens have been presented to the Paris Museum of Natural History. Dr. Verneau confirms the conclusion already arrived at by Dr. F. P. Moreno, that several distinct races of men existed in Patagonia before the arrival of Europeans. It is, however, still impossible to decide definitely whether any or all of these races lived in the country simultaneously, or whether they flourished successively at different periods. Some of the artificially deformed skulls are curious.

The members of the energetic Field Naturalists' Club of Trinidad are continuing to publish valuable annotated lists of the fauna and flora of the island. We have just received the latest of these lists, dealing with the Reptiles and Batrachians, compiled by Messrs. R. R. Mole and F. W. Urich, with the assistance of Dr. O. Boettger and Mr. G. A. Boulenger. De Verteuil's work on Trinidad enumerates twenty-six reptiles, but the species of most of these are not to be recognised, except by the local names. Messrs. Mole and Urich are more scientific, and the list before us is only preliminary, to be followed by a series of notes on the life-history of the various species. There are six tortoises, twenty-five lizards, thirty-three snakes, and twelve batrachians, amounting in all to seventy-six forms, of which twenty-one are recorded for the first time. Dr. Boettger describes one new gecko (*Sphærodactylus molei*) and a frog (*Hylodes urichi*).

Dr. W. H. Dall has been investigating the genus *Gnathodon*, Gray, a series of bivalve mollusca inhabiting subtropical America in the gulfs of California and Mexico, and preferring, like oysters,

brackish to either fresh or salt waters. By Gray and Desmoulins this genus was referred to Mactridæ, while the fact that in certain features it recalled Cyrenidæ was duly recorded. Fischer, in his "Manuel," under the synonym of *Rangia*, places it next to Cyrenidæ, at the same time raising it to family rank. A careful examination of the animal, however, has convinced Dr. Dall that the earlier naturalists were right, and the genus must be classed with the Mactridæ. Six genuine and six spurious or doubtful species are recorded in this monograph, which appears in the *Proceedings* of the U.S. National Museum, xvii., pp. 89–106; 1 pl.

Mr. R. W. Chapman and Captain Inglis are continuing their valuable observations on the tides of Port Adelaide, South Australia, and we have just received their brief report presented to last year's meeting of the Australasian Association. At that time they were proceeding with a second analysis of the Port Adelaide curves, with the aid of Professor G. H. Darwin's computing apparatus.

The lake formed a year ago by the great landslip at Gohna in the Himalaya, burst through its dam at the end of August, and thus has terminated one of the most gigantic geological catastrophes of recent years. Mr. T. H. Holland, of the Indian Geological Survey, has favoured us with a copy of his detailed illustrated report (*Rec. Geol. Surv. India*, vol. xxvii., pp. 55–65, with 5 plates and 2 maps), and from it we learn that the slip was due to the ordinary cause. The rocks on the side of the valley which collapsed dipped at a high angle towards the valley, and the landslip naturally followed heavy rains. On a small scale, as Mr. Holland points out, such valleys as that of Cheddar illustrate the same principle; the side on which the rocks dip towards the valley has a very gentle, stable slope, while the opposite side stands equally firm as a great precipice.

We are informed that a boring for coal will shortly be undertaken in the valley of the Stour. At a meeting held in Ipswich on the 28th August, the Chairman of the Directors (Mr. R. C. Napier) intimated that over £5,000 had been promised, so that there was no reason for further delay. It will be remembered that slaty rocks, older than Coal-measures, but of Carboniferous age, have been pierced at Harwich, and it is considered likely that coal basins may occur in the folds of the old rocks between the east coast and London.

Geologists of the rising school are continually attacking the orthodox theory of the origin of coal-seams; and it is quite likely they are correct in most cases, though the exceptions to the rule of growth *in situ* are never proved to be more than local. Another contribution

is made to the subject by Mr. Herbert Bolton in the last part of the *Trans. Manchester Geol. Soc.* (read June 12, 1894). He describes remains of plants and fishes from the Jarrow Colliery, Co. Kilkenny, Ireland, and concludes with an explanation of the well-known fact that a good deal of the anthracitic coal in this region does not rest upon the ordinary fire-clay, but upon a black shale. He considers that the coal thus situated represents the overflow of peaty matter from a lagoon in the neighbourhood, and thinks the case may be analogous to that of the bursting of a modern peat-moss.

For the past thirteen years Dr. Hermann Credner, Professor of Geology in the University of Leipzig, has contributed an important series of memoirs to the *Zeitschrift* of the German Geological Society, on the primitive batrachia and reptiles met with in the Lower Permian Formation near Dresden. The series being now apparently completed, he has just conceived the happy idea of presenting to those who have received the reprints a title-page and table of contents to preface the volume the memoirs will form when bound together. There is a list of eleven species of Stegocephali, mostly new, and the reptiles comprise the two important forms, *Palæohatteria* and *Kadaliosaurus*. Dr. Credner's very fine series of careful drawings are only rivalled by those of Dr. Anton Fritsch, in his well-known *Fauna der Gaskohle*, and the one work supplements the other.

We have received from Messrs. Dulau & Co. a catalogue of Zoological works, Mammalia, which contains some 2,500 entries.

I.

The Origin of Species without the Aid of Natural Selection.

A REPLY.

I AM much indebted to Mr. Wallace for his interesting paper (NATURAL SCIENCE, vol. v., p. 179). As he was the joint propounder of Natural Selection with Darwin, I could not hope for a weightier critic. Still, I am not in the least shaken in my opinion by it.

In reply, I would first observe that I take the terms "definite" and "indefinite"—which Mr. Wallace says he does not understand—from Darwin himself, who says: "The direct action of changed conditions leads to definite or indefinite results;"[1] while of the former he writes: "By the term definite action, I mean an action of such a nature that, when many individuals of the same variety are exposed during several generations to any change in their physical conditions of life, all, or nearly all the individuals, are modified in the same manner. A new sub-variety would thus be produced without the aid of selection."[2]

These words really strike at the root of Darwin's theory; and, indeed, the whole of my contention, if it were not founded on facts and observations, might be based on this passage; for Darwinism may be compared to an inverted pyramid, the apex being the *mistake* Darwin made in supposing variations in any seedlings of a plant (or variety) in *nature* being "indefinite." *They are always definite.* Though hundreds may perish, *the survivors all vary in the same direction*, viz., towards adaptation to the environment.[3]

In a correspondence with the late Professor Romanes last spring on this subject, he wrote me as follows: "Of course, if you could prove that indiscriminate [*i.e.*, indefinite] variations have not occurred in wild plants, but only under cultivation, you would destroy Darwinism *in toto*." (Hyères, March 12, 1894.)

Having stated my case thus briefly, I will proceed to remark upon Mr. Wallace's criticisms.

[1] "Origin of Species," 6th ed., p. 106.

[2] "Animals and Plants under Domestication," ii., p. 271.

[3] See, *e.g.*, "Origin of Species," pp. 72, 175, 176.

Mr. Wallace writes: "It is, of course, admitted that direct proof of the action of Natural Selection is at present wanting." "At present"—why is it still wanting if it really exist? Has not one of the many biologists who have studied nature all over the world, during the last five-and-thirty years, been able yet to find one single proof?

On the other hand, I venture to say and to prove, *in the strictest sense of the term*, that Natural Selection is not wanted as an "aid" or a "means" in originating species.[4]

In the *elimination* of superfluous weaklings, in the *delimitation* of specific forms, and in the *distribution* of plants, Natural Selection may be largely credited with the results, but in the *origin* of species it is not wanted.

Darwin says that "Natural Selection has no relation whatever to the primary cause of any modification of structure"[5]; and the question with which I am solely concerned is to try and find out how and by what means variations in structure originate in plants; for new sub-varieties, varieties, sub-species, species, and genera are all based upon morphological variations; these being the only things systematic botanists trouble themselves with at all. *Then*, whether Natural Selection exists as a "means" or an "aid" in establishing these differences is a separate question altogether, as Darwin insists. To answer *this*, one looks to see, not only if Nature supplies those data upon which Natural Selection is supposed to act, but if they are of any use in the process. Mr. Wallace tells us what they are, for he says: "Offspring resemble their parents very much, but not wholly—each being possesses its individuality. This 'variation' itself varies in amount, but it is always present, not only in the whole being, but in every part of every being. Every organ, every character . . . . is individual; that is to say, *varies* from the same organ, character . . . in every other individual."[6] Now, is there any evidence, direct or indirect, that any such slight morphological differences as are here alluded to are of the slightest consequence to a *seedling* so as to enable it to survive in the struggle for life? What attempts have been made experimentally to test the truth or the reverse of this hypothesis?

Let it not be forgotten, too, that specific and generic characters are more often taken from the flowers and fruits, organs which are totally undeveloped when the "slaughter of the innocents" takes place, and, therefore, must be all put out of court so far as Natural Selection is concerned in bringing about the survival of the fittest. It has been suggested that a plant survives because, say, of some superiority in the structure of the flower, this feature being correlated with a more vigorous constitution than that of the other seedlings, which die in a premature state. I reply this simply begs the question,

[4] The title of Darwin's book is "The Origin of Species by means of Natural Selection."

[5] "Animals and Plants under Domestication," ii., p. 272.

[6] "On Natural Selection," p. 266.

or is putting the cart before the horse. *A seedling survives solely because it is vigorous.* This is capable of proof, and whatever flowers it may subsequently bear, it must be contented with them, whether they be the "best" or not for fertilisation or otherwise. In corroboration of the above, I would add my own experience with small and large seeds. These show that the better nourished have a much greater chance of starting and crowding out the rest by growing into larger plants, and that if small seeds be selected for some years, they either die out altogether or a tiny race of beings is for a time procured. Hence, for the word "fittest," *i.e.*, morphologically, I would substitute "strongest," *i.e.*, constitutionally.

I note here that Mr. Willis says (NATURAL SCIENCE, v., p. 240) that "Natural Selection has to be *dis*proved." No one, however, can be called upon to "prove a negative." It is for Darwinists to prove that the Origin of Species *does* really require the aid of Natural Selection.

On the other hand, it is for me to prove that the Origin of Species can take care of itself; in other words, to establish the truth of Mr. H. Spencer's observation: "Under new conditions the organism immediately begins to undergo certain changes in structure, fitting it for its new conditions,"[7] and that what is true for the individual is true for its offspring, the result being, to adopt Darwin's words, a new sub-variety without the aid of Natural Selection is produced.

I will now give illustrations of "definite" and "indefinite" variations. In 1847, Professor J. Buckman sowed seed of the wild parsnip in the garden of the Agricultural College at Cirencester. The seedlings began to vary, but in *the same way*, though in different degrees. By selecting seed from the best rooted plants, the acquired "somatic" characters of an enlarged root, glabrous leaves, etc., became fixed and hereditary; and "The Student," as he called it, having been "improved" by Messrs. Sutton & Sons, is still regarded as "the best in the trade." This is *definite variation*, according to Darwin's definition, for those weeded out did not differ from the selected, morphologically, except in degree, the variations towards improvement not being quite fast enough to entitle them to survive.

M. Carrière raised the radish of cultivation, *Raphanus sativus*, L., from the wild species *R. Raphanistum*, L., and moreover found that the turnip-rooted form resulted from growing it in a heavy soil, and the long-rooted one in a light soil.[8] Pliny records the same fact as practised in Greece in his day, saying that the "male" (turnip form) could be produced from the "female" (long form), by growing it in "a cloggy soil." Both forms are now, of course, hereditary by seed.

When a plant has been *long cultivated*, the relatively fixed nature, characteristic of most wild forms, generally breaks down; and the seeds from one and the same individual plant cannot always be

[7] "Factors of Evolution."

[8] This has been corroborated by M. Languet with the carrot. *Soc. Roy. et. Cent. d'Agricult*, 2nd ser., vol. ii., 1846–7, p. 539.

depended upon "to come true." Thus, an eminent agriculturist once said to me (a trifle hyperbolically, of course) speaking of the varieties of wheat: "You can almost get a different variety from every grain in a single ear."

Sir J. D. Hooker records no wild variety of the cabbage (*Brassica oleracea*, L.). Theophrastus (300 B.C.) only knew three cultivated forms. Pliny speaks of six, but who will count them now? It would seem as if plants underwent two courses of variation. First, in adaptation to it, by responding at once to a new environment, *i.e.*, definite variation. Then, when this has been thoroughly established, as with all of our ordinary vegetables, they may vary indefinitely, but why they do so no one can tell. Still, taking a broad view of the whole process, it is obvious that all such variations were primarily due to the environment of cultivation; because *they never occur in the wild state.*

Hence, to test the reality of specific characters of wild plants, as Mr. Wallace describes, by their degree of stability under cultivation in a garden, cannot possibly give but the most untrustworthy results. Some may resist for a time the influences of the new artificial environment, others may succumb to them; but it will be *the very best means of forcing them to change*; for, as Darwin and Weismann assert, cultivation induces variability. Suppose this test had been supplied to the wild and tall *Cineraria cruenta* with its small flowers; what would a systematist now say if he had never known the origin of the modern dwarf kind with large flowers of innumerable colours? He would undoubtedly call it a new species.

The rule may be laid down that *a species may be constant as long as its environment is constant, but no longer.* I have changed the spiny *Ononis spinosa*, L., the Rest-harrow, both by cuttings and by seed, into a spineless form undistinguishable from the species *O. repens*, L., in two years; but it would have, I doubt not, at once reverted to *O. spinosa* if I had replanted it in the poor soil from which I took it. It seems, therefore, to be a very hazardous and fallacious method of testing the value of specific or other characters by cultivation. A wild plant may or not change at once. Thus the carrot, *Daucus Carota*, L., proved refractory with Buckman, but not with Vilmorin, who converted this *annual* to a hereditary *biennial*, by sowing the seed late in the season, till the character of flowering in the second season became fixed.

Indeed, the proposed test is not unlike trying a man's guilt by making him eat an ordeal bean!

Mr. Wallace illustrates his remarks by the case of species of *Arabis*, but quite fails to perceive that it goes to prove my contention altogether. He says: "*A. anachoretica* has tissue-papery leaves—*due to its growth in hollows of the rock*" (my itals.). "Seeds of this plant, when cultivated at Kew, produced the common species *A. alpina*. The same thing occurs with many plants, as every cultivator knows."

If the rocky environment is to be credited with species-making in the one case, so must Kew be in the other. In both cases there is neither mention made nor need of any selection at all. Mr. Elwes told me that the various bulbous plants he introduced from the East into his garden at Preston, Cirencester, changed so greatly in a few years in all their parts that he could scarcely recognise them again.

Mr. Wallace adds: "Other forms, with no greater peculiarities externally, preserve their characters under cultivation, though exposed to the most varied conditions."

This is equally and quite true; but any investigation into the causes of the origin of species by *variation* has nothing to do with any other question of the causes of preservation of the type-characters, or *heredity*. Evolution accounts for all living beings by variation; but it does not attempt to offer any explanation of the existence of "survivals." *E.g.*, *Nautilus* and *Lingula* have lived on from the Silurian days till now; *Equisetum* has flourished from, at least, the Carboniferous epoch till to-day. Therefore *change is not absolutely necessary in organisms under changed conditions;* but when it does occur, then I maintain, with Dr. Weismann, that all changes are primarily due to external influences. He says: "We are driven to the conclusion that the ultimate origin of hereditary individual differences lies in the direct action of external influences upon the organism."[9]

Mr. Wallace is good enough to call attention to my book, "The Origin of Floral Structures by Insect and Other Agencies,"[10] and attacks, very rightly, what I fully admit may be regarded as a weak point in it; *i.e.*, I can bring but few positive illustrations to demonstrate my view that irregular flowers have been formed through the direct action of insects from regular ones; but he quite ignores the whole line of argument running through the book in support of the probability. It is one which Dr. Weismann recommends in support of evolution, which "may be maintained with the same degree of certainty as that with which astronomy asserts that the earth moves round the sun; for a conclusion may be arrived at as safely by other methods as by mathematical calculation."[11] It is the well known argument of the accumulation of coincidences which can furnish probabilities of so high an order that they may be regarded as an equivalent to a demonstration. Thus, physicists tell us that they *know* the composition of the sun, but their knowledge is solely based on the coincidences between the lines of the solar spectrum and those of vapourised substances.[12] Similarly with flowers: when we find innumerable coincidences all tending in one direction, coupled with an indefinite capacity for varying in response to forces in all parts of

[9] "Essays on Heredity," etc. Eng. trans., p. 279.

[10] *International Scientific Series*, vol. lxiv.

[11] "Essays on Heredity," etc., p. 255.

[12] The "fact" that udders have become enlarged by hand-milking is based on a similar accumulation of probabilities.

plants, I still maintain that "Mr. Henslow's theory [does not] utterly break down." Mr. Wallace contends that the negative evidence derived from "regular" flowers, as gentians, tells against me, as they ought to have long ago become irregular, since their "lower petals have been always subject to irritation and have never developed irregular flowers." This is scarcely fair; for not only do all botanists believe—on precisely the same grounds of probabilities—that all irregular flowers have descended (somehow) from regular ones; but that, if he will refer to the chapter on "Peloria," he will see that existing regular flowers, being mostly "terminal," have no "lower" petals at all, but are so situated as to offer access to insects from all points of the compass. Moreover, whenever a plant with normally irregular flowers (which are always situated close to the axis, so that insects can only enter them in one way) produces a blossom in a terminal position (as foxglove, larkspur, horse-chestnut, etc., often do), it at once becomes quite regular. These differences between regular and irregular flowers represent two of those groups of coincidences respectively, to which I referred.

Mr. Wallace adds: "The very first essential to this theory is to prove that modifications produced by such irritations are hereditary." Quite so. But this proves itself, if my contention be right; for plants with irregular flowers *are* all hereditary. So that there is no need to prove this point, provided the "previous question" as to the origin of irregular flowers themselves be answered. But the converse change can be readily shown; for flowers, normally irregular in nature, often revert to their ancestral regular form under cultivation in the absence of insects, and then come true from seed, as do Gloxinias. Unfortunately, one cannot make a regular flower become irregular. How long it required in nature to do so no one can tell; but all the innumerable minute details of structure coincide to one end; a multitude of correlations all fit together for one effect; so that we may put the alternative thus—Which is more likely, that some one common cause has set up these minute, often microscopic, details in unison together; or that they have arisen by selection out of innumerable wasted variations, which no one ever saw in nature, nor can even ever see a trace of under cultivation?

When, however, we come to variations in the vegetative system of plants, there is nothing easier than to prove, first, the direct action of the environment, and secondly, the hereditary persistence of the result. I need go no further than to take Buckman's parsnip, Carrière's radish, Vilmorin's carrot, or anybody's variety of cabbage. What are all these and many other instances but experimental verifications.

Mr. Wallace alludes to my last paper on "The Origin of Plant-Structures by Self-Adaptation to the Environment, exemplified by Desert and Xerophilous Plants,"[13] and attacks my inferences with

[13] *Journ. Linn. Soc. Bot.*, xxx., p. 218.

regard to spinescent processes of desert plants; but he again ignores the primary argument of innumerable coincidences; while in the case of vegetative organs this argument has been in many cases "verified by experiment." When, however, Mr. Wallace calls in question my statement that spines are correlated with a dry soil and atmosphere, he controverts those of Belt, Aitchison, Scott Elliott, Grisebach and others, for he says: "There is no such general coincidence of aridity of soil and atmosphere with abundance of spiny plants, as very little enquiry will show." Having seen and gathered them myself in the Libyan desert and even on our own sandy heaths, I cannot accept this statement; and if those eminent travellers I have named are misleading us, where are we? He then mentions the Galapagos and other islands, where, though of a desert character, plants are *not* spinescent. Here, again, I am not concerned with what does *not* occur, but with what *does*. Moreover, any cause that may tend to arrest an axis likewise may tend to render it spinescent, and more than one cause may produce the same result,[14] so that it is not altogether strange to find spinescent processes away from deserts; but I *do* maintain that spinescence is one and an important element in the *facies* of hot and arid deserts with a barren soil.

Mr. Wallace advances the well-worn theory of the interaction of mammals and spines. In the first place, if I may still believe in the prevalence of spines in deserts, they occur where no herbivorous quadrupeds live. Secondly, if a mammal wishes to eat a spiny plant, it somehow often gets over the difficulty; thus donkeys knock off the spines of *Opuntia*; horses eat gorse. I had a cow which was partial to holly, another rejoiced in nettles! But all this is beside the question. It seems to me that there is a lurking element of teleology in this view: for any structure which arises *in anticipation of its use* savours of natural theology[15] rather than of evolution by natural processes alone. I fully admit that plants, when once they have got their spines, may be able to keep animals more or less at bay; but they originate, I maintain, as a mere accidental and inevitable result of an arrest of the organ in question, such arrest being mainly due to drought.

If teleology in its old dress of *Design in anticipation of Use* is, and ought to be, extinct, we may accept Darwin's form of it, that Evolution is the Deity's method of creation. Let us, then, recognise protoplasm as having been impressed with the power of self-adaptation—such being the inference from direct observation of its behaviour; and, consequently, enabled to build up structures in an automatic response to the environmental forces, whenever it is necessary to bring about a better degree of equilibrium between the internal and external forces.

[14] I observe Mr. Osborne makes a corresponding statement. NAT. SCI., p. 223.

[15] Indeed, such anticipation is absolutely necessary for the theory of Natural Selection in general.

On the last page but one of his paper, Mr. Wallace alludes to the case of the hard shells of nuts, and asks if the direct agency of birds, monkeys, etc., has anything to do with them. He admits the question is absurd. I do not therefore know why he asks it. I have not myself written a line on this branch of the subject, but will suggest, from what one knows of all other parts of plants having the capacity of varying, that I see no reason for inferring that hard coats of fruits should be subject to any different law. Soft fruits vary readily enough, as melons, pea-pods, apples, as well as pears in their degrees of "stoniness." Moreover, under cultivation, varieties of forms of nuts and walnuts have arisen, as well as of olives, almonds, and dates, and other hard-coated or hard-seeded fruits. The fact seems to be that cultivation affects the whole organisation of the plant; for the environment is not always solely concerned with an isolated bit of a plant, as a nut or a root. Many visible changes are due to secondary causes within the individual; but in all cases, as I believe with Dr. Weismann, they are *primarily* attributable to the direct action of the environment, simply because *they never occur unless the environment itself is changed.*

Finally, to return to my starting point. The whole question lies within a very small compass. Thus, first, no one disputes the fact that the environmental forces can act upon an organism. Secondly, that the organism can respond to those forces. But now follow two views. Darwinites say that the resulting variations are indefinite in Nature, just as they so often are in cultivation; and that the environment *selects* the best fitted *to survive*. I say that they are always definite in Nature: and not only exceptionally so, as Darwin thought; and that the environment *induces* the best fitted *to arise*.[16] Therefore, Natural Selection has nothing to do in aiding the Origin of Species.

For additional facts I would refer the reader to a paper entitled, "A Theoretical Origin of Endogens from Exogens, through Self-Adaptation to an Aquatic Habit"[17]; and to a companion volume to the "Origin of Floral Structures," which I hope will be shortly published in the "International Scientific Series," and entitled "The Origin of Plant Structures by Self-Adaptation, in Response to the direct Action of the Environment." In this, similar lines of argument, with illustrations, will be applied to Desert, Aquatic, Maritime, Alpine, and Arctic, as well as Climbing Plants, and to the Origin of Peculiarities of Roots, Stems, and Leaves.

George Henslow.

[16] See "Animals and Plants under Domestication," ii., p. 272.

[17] *Journ. Linn. Soc. Bot.*, xxix., p. 485.

II.

Horticultural "Sports."

THE immense variety existing at the present day in the cultivated flowers, foliage plants, fruits, etc., which figure in our horticultural shows, while evoking the admiration of the many, is rarely appreciated at its true value as the outcome not only of infinite labour in culture and careful selection, but as largely due to vagaries and freaks of nature quite outside the scope of any known laws. Thus, we see flowers such as tulips, hyacinths, dahlias, and others exhibiting all, or nearly all, the colours of the rainbow in conjunction with most complex variations in the form of the blossoms, all of which have been developed from single-flowered normal wild plants, with one-coloured petals. Now, a large part of this development is undoubtedly due to the careful selection and accumulation, generation after generation, of comparatively small differences which appear in the seedlings, but it is a remarkable fact, of such general occurrence as almost to constitute a law, that when a wild plant is brought under culture and thus subjected to more or less artificial surroundings, it becomes in some way subtly modified, so that not only is its offspring likely to vary more widely than before, but even individual buds of the plant itself may produce modified foliage, flowers, or fruits capable of being propagated and so forming a new and permanent variety. Probably the most striking instances of this are seen in the origin of Nectarines and their varieties. These have all sprung direct as perfect nectarines from peach trees or peach stones. Old peach trees, after producing peaches only for many years, have produced fruiting branches bearing true nectarines, and since nectarine stones in their turn may yield true peaches, it would seem that it is really a case of dimorphism, akin to the two forms of leaves sometimes seen in plants. This phenomenon has repeatedly occurred in the peach, most of the many varieties of which have been raised from stones, though some have originated by buds, and it is curious that in a few of these cases the fruit has only varied in the direction of being earlier or later in ripening—a feature which is constant. A well-authenticated case is recorded of a gooseberry bush being so sportive in its buds that four widely different sorts were produced upon it. Cases of this description, however, bring us to the question of cross-fertili-

sation, by which means the horticulturist often succeeds in blending or modifying the colours and forms of two very different varieties or even species, one result of which is a tendency to revert, and the gooseberry in question may have owed its diversity to this. The tuberous Begonia, for instance, which now forms one of the most salient features of our shows, began its upward career with a blend of two distinct species. This blend, in its first results, yielded plants of very different habits to either parent. Fortunately, the alliance was a fertile one, hence propagation by seed and selection of offspring presented no difficulty. It was speedily seen that the two species were sufficiently at variance in the blood of their joint progeny to induce an immense divergency of type, and seed being sown from the most diverse forms, the final result was that no two of a batch were quite alike. Here, of course, was the cultivator's opportunity, and in the hands of Mr. J. Laing and Mr. H. Cannell, in the course of twenty years most magnificent strains of huge double and single flowers, of all habits and all tints of white, salmon, and red, have culminated in constituting the naturally insignificant begonia a fair rival to the very Queen of Flowers herself, barring her perfume. Here is, indeed, a triumph of Art over Nature, so far as the art of selection is concerned.

What, however, the public do not see, though they feel it in the high prices necessarily demanded for the results, are the thousands upon thousands of worthless plants which accompany the good ones. Often and often, and especially in the earlier stages of development, the cultivator has to raise to blooming point, or, worse still, in the case of trees, to fruiting point, an enormous number of seedlings in which he may vainly search for a "break" in the right direction. This "break," obtained, however, he starts under ever-improving auspices, until—as in the case of Begonias in question—he finds his best flowers barren, their extreme development being at the cost of their reproductive energy.

Then, too, it is a rare thing indeed for the breeder to be perfectly satisfied. Frequently, when his flower is perfected as regards the blossom beyond his utmost expectation, the stalk is too short to display it properly or too weak to keep it upright. A heavy double flower with a pendulous habit will not do. It must look you boldly in the face and display its charms effectively. In time this is generally arrived at by judicious crossing, or it may be by simple selection of seedlings, provided always the utterly barren stage has not been reached.

Fashion plays a large *rôle* in horticulture. Dahlias, for instance, starting from a simple single starlike bloom were transmuted by long years of selection into huge spherical masses of regularly folded petals, and decorated with any colour desired except the unattainable blue. Suddenly the æsthetic craze steps in with the sunflower as its floral deity, and lo! the dahlia must be single and the cultivators

have an opportunity of marketing their wastrels, at the expense, of course, of their choicest. Then the Juarez or Cactus type turns up from Mexico, and becomes the rage, and in a season or two we find it evolved in all tints, and largely from the old forms, many being christened Cactus, which, in the breeder's opinion, had simply "gone wrong," and, but for the fashion, would have decorated the dust-heap.

So far, we have touched mainly on the minor "sports," which need to be accumulated to be striking; but in many cases, as in that of the nectarines, among the fruits we owe some of our best flowers to wide and sudden departures from previous types. The beautiful double crimson hawthorn, for instance, appeared as a bud-sport on a pink one, a branch resulting which was entirely covered with dark crimson flowers. Many of our best roses we owe to bud-sports. The white moss-rose came as a sucker on the red, and, when severed for propagation, the striped moss-rose sprang from a bud near the cut. One variety of chrysanthemum yielded, in the same way, no less than six distinct forms. Generally, varieties originating in this manner remain constant when propagated by cuttings, but are very liable to revert when reproduced by seed. Variegated forms innumerable have originated as bud-sports on normal green plants, and these, when cut off and propagated by buds, retain their peculiarities. The writer recently saw a plumose form of *Polystichum angulare* of which six fronds out of seven were profusely splashed with pure white, yet the plant was several years old, and neither it nor its parent, of which it was an offset, ever showed the slightest tendency to variegation before. This case is very peculiar, so much of the plant being suddenly affected, and to such an extreme extent. Seeds of variegated sports of this class generally produce normal green and wholly white plants, the latter of which are very short-lived, owing to the absence of chlorophyll. In very rare cases, a graft has been known to affect the stock, and form a graft-hybrid. *Cytisus Adami*, a hybrid laburnum, is, perhaps, a unique oddity in this way, being a quasi combination of a purple and a yellow laburnum. The leaves of each species differ materially, as does the general habit of growth, and in specimens of this, which may be seen at Kew, the tree is constantly sending out branches and twigs of each distinct species side by side, and mixed up in the most curious fashion; the foliage and habits peculiar to each accompanying the characteristic bunches of yellow or purple flowers.

The well-known Cockscomb, a densely fasciated form of *Celosia*, is ordinarily a rich crimson, but at a recent show of the Royal Horticultural Society, one grower who had made a hobby of this for years exhibited fifteen distinct colours of it, including a yellow, and every plant was furthermore crimped and crested in a fashion of its own. Patience is indeed a virtue to the selective cultivator. Tulips, to wit, frequently remain for many years in their "unbroken" stage. The seedlings of this family, when they first bloom, present very different

characters from the parental form, but after a time they "break" or assume their proper character quite suddenly, and until this occurs it is impossible to say whether they are valuable or not. Orchids owe their vast variety chiefly to unaided Nature, and the efforts of skilled travellers who ransack their native habitats to find new ones. Of late, however, much has been done by crossing allied species, and so obtaining flowers embodying their united charms. Imported Orchids form somewhat of a lottery, and many a prize has been acquired unwittingly in the shape of a dormant pseudo bulb, costing a few shillings, and fetching, when its true character has appeared in its bloom, a handsome sum in guineas. Such a case occurred not long since, when an orchid was put up in a thumb-pot, with a tentative reserve of a few pounds, and was knocked down, after a spirited competition, at one hundred and forty guineas. Such plants may, of course, be new species, but usually they are varietal forms of well-known ones, and are thus simply natural "sports" of precisely the same character as those already described.

Chas. T. Druery.

III.

On the Geology of the Plateau Implements of Kent.[1]

THIS subject has been fully treated by Professor Prestwich in his several memoirs in the *Quart. Journ. Geol. Soc.*, vol. xlv., 1889 (the old worked flints at pages 282–286); vol. xlvi., 1890 (the general geology of certain drift-deposits, and the geological stages of their formation, pages 166–168 and 179); vol. xlvii., 1891 (the same subjects, pages 126–159, particularly pages 157–159, and plate 6, and the peculiar flints at page 160, with plate 8). Also in the *Journ. Anthrop. Instit.*, vol. xxi., 1892, pages 246–270, with plates 18–21.

Mr. W. J. Lewis Abbott has more lately contributed to NATURAL SCIENCE (vol iv., no. 20, April, 1894) an interesting paper "On the Plateau Man in Kent," containing a clear account of the old rude implements found on the Chalk Plateau, illustrated with two plates.

It is shown in these memoirs that certain superficial soils on the North Downs, between Sevenoaks and Rochester, contain numerous rudely-worked flints, which are connected with a gravel area, of very great antiquity, and probably derived originally from the side of the old Wealden Range, which once rose 2,000 to 3,000 feet above, where Crowborough and other hills in Sussex now are. Those heights were long since removed by natural agencies, such as rain, rivers, sea, frost, and ice, probably with more than one time of energetic action, and portions of their *débris* were distributed by old streams or otherwise to and over a plateau of Chalk at a lower level on the flanks of the range.

These rude old implements have an ochreous tint, and are associated with limited patches of much-worn gravel having the same colour, and not forming well-defined beds, by no means thick enough for the making of a gravel-pit. Nor are the implements under such conditions as would lead to the supposition of their having received

[1] This paper was read before a combined meeting of the Anthropological and Geological Sections of the British Association, on August 10, 1894, in consequence of a suggestion made by the Anthropological Institute that it was desirable to know more of "The evidence afforded by the 'Plateau Gravels' relative to the Antiquity of Man."

their staining in their present position. A few only have been found beneath the surface.

It is very desirable that excavations should be made at proper places on the plateau to discover, if possible, the extent and thickness of the implementiferous soil.

The peculiar reddish-brown colour of these rude implements, with ochreous stain and some adherent particles of limonite, was probably obtained when they were lying in an old ferruginous gravel in their original place; and some of this gravel has accompanied them to the plateau. Their colour and greater wear distinguish them from the few palæolithic and neolithic worked flints found on the plateau. Some have a strong resemblance to the "valley forms," and were most probably transitional in character and condition. Many are very much water-worn—an indication of their long and distant travel. Some are *scratched*,[2] by very close pressure, either with or without the intervention of ice-action.

The evidences borne by these peculiarly-shaped flints of their having been fashioned by the hand of Man, have been carefully dealt with by Professor Dr. Prestwich (see above), Messrs. B. Harrison (their discoverer), De B. Crashaw, W. J. Lewis Abbott, A. Montgomerie Bell, and others. Mr. J. Allen Brown has succinctly referred to them as "Eolithic: roughly-hewn pebbles and nodules, and naturally broken stones, showing work, with thick ochreous patina; found on the plateaux of the Chalk and other districts, in beds unconnected with the present valley-drainage"; and "under conditions which clearly indicate that they are older than the usual valley-drift implements." (*Journ. Anthrop. Instit.*, vol. xxii., 1892, pp. 93, 94).

In "The Ground Beneath Us," 1847, pages 71–79, many interesting points bearing on this subject were already illustrated and explained by Professor Prestwich. The great changes of land-surface in this part of the European area were referred—firstly to the movements, accompanying the elevation of the Pyrenees, when the Chalk became dry land, with its uplands, valleys, and estuaries; and he showed that at that time the Wealden area formed an island in the Thanet-Sands Sea.

Some rivers afterwards brought down the clays and sands which now constitute the "Woolwich and Reading" beds. With some submergence other Tertiary beds were formed, probably extending over a part of what is now the Wealden area.

Secondly, this area, with its stratified coatings, was raised up (after the formation of the London Clay) by movements accompanying the elevation of the Alps by the lateral pressure caused by the earth's contraction. The other Tertiary beds (of Paris, Bracklesham, etc.) had helped to shallow the open sea surrounding the island of the

[2] On one and the same specimen are some scratches quite raw, and some that have been coloured with red ochre, also some merely linear streaks of red hæmatite.

Weald, which was ultimately to be an elevated, elliptical, weather-worn, and sea-eaten dome of great height.[3]

The successive stages were :—

1. When the elevation of the Wealden area attained its maximum, there was certainly a considerable thickness of Chalk on the surface, and this was necessarily exposed to marine and atmospheric denudation.

2. The immediate result of this was the wearing-away of the



FIG. 1.—Diagram showing roughly the relative position of the formations constituting the Wealden Anticlinal between the North and South Downs (1. Limpsfield, 876 ft.; 2. Oldbury, 620 ft.; 3. Crowborough, 803 ft.); also their successive denudations; and the original place of the Old Gravel (4), some of which was brought down by natural agencies to the Chalk Plateau (5) now existing.



FIG. 2.—Diagram showing the possible position of the Lower Greensand outcrop (*C*) when the Old Gravel (*A*) was being transferred from the higher (*A*) to the lower (*B*) level of the Chalk by natural agencies along a continuous surface. The strata are set at too high an angle in Fig. 1 to show what is required here.

(*A*) Old Gravel in place. (*B*) Plateau Gravel derived from the Old Gravel.
(*C*) Outcrop of the Lower Greensand.

Chalk, and the trituration of the washed-out flints; and thus the formation of a great bed of shingle—that of the Thanet Sands.

3. With continued wave-action, these shingle-banks were washed away and distributed at a lower level, to be the Pebble-beds of the "Woolwich and Reading" series, which had been formed in the meantime by rivers from the hill-ranges. The pebble-beds can now be seen at Addington, Blackheath, etc., extending to the very edge of the present Chalk escarpment.

4. The deposits of this stage were next removed in part; and in course of time the Diestian beds were laid against the flanks of the lowered and perhaps sinking range. Of these strata some limited patches, such as the Lenham beds, remain here and there on what are now the Chalk Downs.

5. One or more accumulations of Chalk-flint *débris* were formed

[3] A neat little diagrammatic section across the Wealden area, showing the high elevation of the anticline (about 2,000–3,000 feet; Topley, *Mem. Geol. Surv. Weald.*, 1875, p. 217), was published in Professor (Sir A.) Ramsay's "Lectures on the Physical Geology and Geography of Great Britain" (five editions, 1863–1878). At the time of this elevation of South-eastern England, the Ardennes attained a height of about 18,000 feet, as shown by MM. Cornet and Briart, *Mém. Soc. géol. Belgique*, vol. iv., p. 71.

at about this stage of the history of the old Wealden Range or Island, and were probably characterised by the presence of iron-oxide in greater quantity than in the common ferruginous gravels of the South-east of England at present. Man, being present, used such pieces of the flint as suited his requirements. Probably, at first, with little or no alteration of their form; but afterwards he applied them with definite modification of their shapes to meet his wants in killing, skinning, cutting, fire-making, rubbing, pounding, scraping, drilling, knocking, breaking, chopping, digging, etc., that is, in tooling and other processes. Such implements he made and left there, on that old, very old, probably pre-Glacial ground. (*See* Fig. 1.)

6. This gravel extended down the side of the "dome," perhaps tailing down the slopes of uplands by slips, slides, and slushings, probably by more than one stage, after its formation; or, during a succeeding age, the encroaching sea cutting away the lessening dome, or torrents scoring the hill-sides, removed the more or less extensive deposits of ferruginous gravel, with the rude implements left upon it, and spread out the much-worn relics on the slopes of the Chalk below. Here they are now found on the isolated plateau; and they lie on the red Clay-with-flints, that had been in process of formation previously, for ages, by the gradual solution of the Chalk below, and the settlement of argillaceous and sandy matter from the overlying and gradually disappearing Tertiaries. This was co-extensive with the Chalk-surface; and on it lies some of the transported ochreous gravel, together with Tertiary pebbles, less-worn flint-stones, and some *débris* of the Lower Greensand, which the wave-line had then reached. The presence of chert fragments from the Lower Greensand proves that the current of driftage (or the tailing of the gravel) must have passed over the outcrop of the Lower Greensand, and therefore here from south to north, on a continuous surface. (*See* Fig. 2.)

7. Subsequently the outlying Chalk (now the plateau above referred to, sloping from an elevation of about 800 feet on the south, to 400 feet, and less, on the north) was cut off, by denudation in the Glacial Period, from the remaining uplands of the once lofty range; the Holmesdale (or Weald-clay Valley) lying below the escarpment of the Lower Greensand at the foot of the diminished dome, and the Gault Valley at the foot of the Chalk escarpment. (*See* Fig. 1.)

The Diestian or Lenham beds were formed in the early Pliocene period, and the denudation probably began directly afterwards, at about the time of the Red or the Chillesford Crag, in late Pliocene or post-Pliocene times; and as the old ferruginous gravel had not only been formed but had been brought to a lower level before that time, it must be regarded as of pre-Glacial age.

A similar series of occurrences and geological results evidently took place on the south side of the old Wealden uplands, giving origin to the brown-coated rude implements at Friston, near Eastbourne, in Sussex.

The probably great changes of climate, due to rearrangements of land and water and of high and low ground, form part of Professor Prestwich's interesting history of the Wealden hill-ranges, in the sculpturing of which not only aqueous, but glacial agency must have had a large share.

It must have been a great pleasure to the veteran geologist, Professor Dr. Prestwich, to find that his conclusions (in 1890) as to the Pliocene Tertiaries and gravels on the flanks of the diminishing island of the Weald fitted so truly, as consecutive history, with his early views (1847) of the probable conditions of the Wealden dome in Eocene times.

Perhaps it may be asked :—

I.—Why may not the Rude Implements belong to the Red Clay-with-flints, since some few have been obtained in shallow diggings on that bed?

The reply is :—1. Because that clay was not ever moved about, much less *drifted*.

2. Because that clay occurs over other and larger areas without containing any such implements, but only unrolled flints separated from the Chalk by its dissolution.

3. Because the implements are accompanied with chert and ragstone from the Lower Greensand, which *débris* must have come from a higher level—namely, from the outcrop of the Lower Greensand on the side of the hill-ranges of the "Dome," *before* the denudation of the present Gault valley.

4. If the Implements had *settled* down from above *with* the "Clay-with-flint," they must have *either* been in the Tertiaries (!) to be let down, as the Chalk dissolved below (possibly when the Chalk had a somewhat higher slope against the "Dome"),—*or* they must have *been on the* Chalk *before* the Tertiaries were formed there!!

5. The "Red Clay-with-flints" is of local origin, whereas the rude implements, with the sprinkling of brown gravel, are foreign to the place of their occurrence. And on the surface, where these implements are found, chert and ragstone from the Lower Greensand always occur.

6. Professor Prestwich informs me that he has examined hundreds of sections of the "Red Clay-with-flints," and has never found the implements in it. Some of the reputed cases of their presence in this clay are doubtful. In other cases, mentioned by Mr. Harrison (see the list on next page), the depth is so small that the implement may have worked itself down, or may have fallen into a hole.

7. The flints in the red clay are not stained. The crust of the flint retains its white colour; or, if the flint has been broken, the surface of fracture has weathered white. The red clay does not stain; it seems to have removed the water-of-crystallisation, and left a white surface on the flint.

8. Thus both the travel-worn gravel and implements must necessarily have had their source in the flints of Chalk situated at some *higher* level.

II.—Why not belonging to a *local* ochreous drift, deposited originally on the plateau?

The reply is:—1. Because the plateau is out of reach of any present (or late) water-level.

2. Because there is too little of the ochreous gravel there to constitute such a local drift.

Notes.

1.—The old brown "plateau implements" are not found beyond the plateau, except in a few instances when they have been derived from the wear of the plateau, and probably this may be said also of other plateaux.

2.—As to these being *natural* forms, where can any such forms be shown to occur in a river-gravel?

3.—The extremely worn condition of the ochreous flints and implements indicates long-continued movement and great water-wear, if not violent transport; and no well-marked, definite deposit of such a gravel occurs. The tail of the old deposit on the Dome, however, may have extended from the original slopes down to the Chalk level (now the plateau), by more or less violent slides and *débacles*, before the bounding or dividing valleys were excavated in the Glacial Period.

The following is a list (from letters) of the Rude Implements found *in situ* by Mr. Harrison. For the localities, see the Map, *Quart. Journ. Geol. Soc.*, vol. xlv., pl. 9, and *Journ. Anthropol. Instit.*, vol. xxi., pl. 18.

1.—One flake, rude.—From an old sand-pit (now a pond) at Ash.

2.—Three rude implements.—From a trench nearly three feet deep, in sand, with green-coated flints (probably a remnant of Thanet or Woolwich-and-Reading Sands), at Ash.

3.—Two rude.—From a trench in similar sand, 2½ feet deep (for laying in mangold wurzel), at Ash.

4.—Two rude.—From deep-red clay at the Vigo, two finds with an interval of two years.

5.—One rude.—From a hole (for finger-post) in red clay at the Vigo Inn.

6.—One rude—From red clay thrown out in digging a grave at Cudham churchyard.

7.—Several rude.—From trench (for water-pipes), crossing a patch of ochreous gravel on red clay (?), at Dunstall Hill, Shoreham, Kent; three tips of neoliths (?) and many rude implements, all water-worn.

8.—One rude (?).—From a post-hole, 2½ feet deep, at Kingsdown.

9.—One rude.—Fine large implement, at 2½ feet, Ash.

10.—Several rude.—From a deepened dew-pond, near the 775 feet summit level; about thirty rudely-worked flints, and some Oldbury Stone (Lower Greensand).

11.—Some rude.—From a large pipe in the Chalk, filled with deep-red clay, at 720 feet, in an old Chalk-pit, a little below the crest of the escarpment, some deep-tan-coloured implements.

12.—Some rude.—From the grave for a horse on the crest of the escarpment, at about 775 feet; the surface soil was opened out for five feet, and "old implements" were found in the clay and gravel thrown out.

Hence it can be seen that there is evidence of the existence of the Rude Implements locally beneath the surface, and that systematic excavation is required.

T. Rupert Jones.

IV.

The Effect of Temperature on the Distribution of Marine Animals.

THE question of the influence of temperature on marine animals is a comparatively old one. Twenty years ago and more it had been already discussed in England and America, but even at the present day it is far from being solved. On the contrary, very conflicting views prevail as to the importance of temperature on the distribution of marine life. Some authors regard it as the sole important factor, others do not attribute any value to it at all. This diversity of opinion is less the result of a lack of facts, our store of which has been greatly increased by the last years' expeditions; but it is rather to be attributed to an incorrect grouping of the facts. My intention, on the present occasion, is not so much to bring forward new data as to re-arrange those we already possess.

The conflict of opinion may be seen in the work of two investigators, whose particular line is geology, but who have occupied themselves especially with marine biology as a territory intermediate between the sciences of Zoology and Geology. I refer to Walther and Heilprin.

Walther says, at the end of a detailed argument, after enumerating many facts, which are to prove the influence of temperature, that the latter is not only *one* factor, but the exclusively determining one, in comparison to which all others, like light, atmospheric pressure, etc., are quite in the background.

Heilprin, in a discussion of the fauna of the deep sea, does not attribute so great an influence to temperature, and brings forward the following train of argument. Reef-building corals, which flourish best at 70–75° F., and which do not grow below 68°, would find this temperature in many oceanic parts, even at a considerable depth, in the Red Sea, for instance, even down to the bottom; nevertheless, they occur everywhere only at the surface. Conversely, the fauna of the deep sea begins in the lesser depths (100 fathoms if there is a bottom), and, though the temperatures are much higher there than in the real abysses, the fauna is the same. The beginning of the fauna of the deep sea is always to be recognised at a certain depth, which is about the same in metres everywhere, but which must have a very

different temperature in the different seas and different latitudes. Moreover, if temperature is the determining factor, Polar animals ought to be the same as abyssal ones in lower latitudes, which is denied by Heilprin.

In the whole of this argument no clear distinction has been made between three classes of facts and relations :—

1. In the first place, no distinction has been made between pelagic animals, the Plankton and the Benthos, *i.e.*, the life confined to the shore or ground, animals either sessile or capable only of creeping, in contrast to the Nekton, the freely-swimming animals, capable of considerable movements, able to stem the tide and migrate at will.

2. Secondly, without further consideration a parallel has been drawn between the horizontal and the vertical distribution with reference to temperature, while the latter becomes complicated by other factors, too, as pressure, want of light, etc. Moreover, the gradations in the change of temperature in different depths are not all proportional to the amount of the depths themselves.

3. In the above argument no distinction has been made between eurythermal and stenothermal animals.

A few words ought to be said to explain these terms (first adopted by Moebius). We might give a definition by saying that eurythermal animals can bear great differences of temperature, stenothermal animals cannot, and this can be represented in a graphic manner. Each animal has a certain optimum of temperature, at which it flourishes best. A number of degrees above it its metabolism cannot go on ; and in a similar manner, at a certain degree below this optimum, the metabolism ceases also. Thus we obtain for each animal three points in the thermometrical scale—optimum, maximum, and minimum. Now in some animals the space on the thermometrical scale between maximum and minimum can be very great : these are eurythermal animals. In other cases it is small : these are the stenothermal animals.[1]

It is clear that eurythermal animals cannot be made use of either as a proof or a contradiction of the influence of temperature. Many animals of the shore, especially molluscs and echinoderms, are proved by experiments to be eurythermal. If a bivalve, for instance, is found both in the North Polar Sea and in the lower strata of the German or English Sea, nothing is proved *for* the influence of temperature—just as little as the occurrence of a eurythermal animal at the surface both in the Mediterranean and at Spitzbergen would be a proof *against* it. This has often been overlooked.

Moreover, it is easy to understand that it is necessary that the

[1] It is obvious that warm blooded animals can bear greater differences of temperature because they have their mechanism of heat regulation in their own body ; they are therefore *eurythermal*. The poikilothermal animals, on the contrary, to which the animals of the sea belong almost exclusively, are more stenothermal *à priori*.

animals of the Nekton, *i.e.*, the animals which swim well, should not be too sensible to temperature; for by their power of moving either in a horizontal or in a vertical direction, they pass easily into very different conditions and have to be adapted to them, if the faculty of swimming is to be of any use to them at all. As a matter of fact, we find that the large swimming animals, such as crustaceans and fishes, are, in general, eurythermal.

The geographical distribution of whales seems at first to contradict this consideration, but in reality it does not. For if we find some of their species chiefly inhabiting the high north or south latitudes, it is not because they are unable to endure higher temperatures, but because only in those regions can they find the enormous quantity of organic substance, of Plankton, which they require for their gigantic bodies.

Here we touch another point of importance. In high north and south latitudes, the production of Plankton, the primordial food, the algæ, the small crustacea, etc., is extraordinarily great, because the variations of temperature are here small, and therefore the life-conditions are very favourable, in spite of a low degree of temperature. This also has contributed to obscure the question of the influence of temperature. If an animal cannot exist below or above a certain degree of temperature, it is clear that the determination of the *average* temperature of a place is of small zoögeographical value; for this average temperature can just as well be the mean between two temperatures suitable for the animal as between two which are harmful or fatal to it. The attempt has therefore been made to draw lines of greatest cold as decisive for the question, and these are termed isocrymal lines by Dana. Not much, however, is gained by that; first, because we ought to regard the lines of greatest heat as well, and secondly, because it is not the degree of temperature alone, but the extent of variation which has a harmful influence. It is proved, in some instances, that the same animals can live at very different temperatures, at 3°, 7°, 15° Centigr., if only this temperature at the respective localities remains fairly constant to 3°, 7°, 15° Centigr.

These are eurythermal animals, but they are, nevertheless, stenoi (restricted), not with regard to temperature, but with regard to its variations.

I think a strange occurrence in marine life, upon which Semper laid stress, can be explained in this manner. Some Hexactinellids, typical sponges of the deep sea, occur in the Pacific Ocean, in very different temperatures; even at a comparatively small depth in tropical parts, in which the temperature is about 10° higher than in greater depths of the China Sea, in which they are to be found also. The factor which all these localities have in common, besides the absence of currents, is slight variation of temperature.

We have seen that the Nekton animals, being eurythermal, cannot be appealed to in *proof of anything* regarding the influence of

temperature; but the case is different with the two remaining biological groups of marine life, the Plankton and the Benthos.

The difference between the latter is the second point hitherto often neglected in the question of the influence of temperature.

By the term *Benthos* is meant any animal which passes its life in connection with the ground, whether it be the bottom of the deep water or the shore of the shallow water. We may distinguish, according to Haeckel, sessile Benthos, *i.e.*, animals totally fixed, and ambulatory Benthos, *i.e.*, animals capable only of creeping and crawling over rocks and sands, but not of swimming great distances at will like the Nekton.

Plankton is capable of travelling long distances, not at will, but only floating or drifting about with the currents. We might further distinguish a littoral Plankton, the Plankton of the shallow water, the shore, and the Plankton *par excellence*, that of the High Sea.

Distinct as these groups are, there may, nevertheless, be transitions between, even in the life of one single species. A Medusa, as fixed at the stem of a Polypoid, is a Benthos animal—when set free, an animal of the Plankton. But such cases occur only among the animals of the littoral Plankton, not in the Plankton of the high sea; there are other Medusæ which never have a sessile stage in their life, and are, like Salpæ, Radiolaria, etc., Plankton animals *par excellence*.

These different groups should not be confused with regard to their relations to temperature. Among the animals of the Benthos there are, as I have said, a number of eurythermal ones, as shown by experiment. But the majority are tuned, as it were, to a certain degree of temperature, which, just like the relations to the light, to the character of the bottom, and so on, must be reckoned among the peculiarities of the species. The Benthos animals are mostly stenothermal, and hence the influence of the temperature on them is very remarkable.

The coasts of the sea might, therefore, be divided, in the first instance, according to latitude or isocrymal lines, into *zones*, which are broken up into *areas* by a longitudinal division according to the continents.

Dana, Günther, and Agassiz have given us very valuable guidance for the animals of the shore. It is true that, by adding the longitudinal division to the zonary, *temperature*, which effects the arrangement into zones, does not remain the only factor in the distribution of the Benthos, as the facies of the bottom, etc., are not to be neglected. Still it is regarded as the most important one.

The distribution of the pelagic life of the open sea is not so clear. The longitudinal division is now without significance, because the influence of the continents does not come into the question. It would, however, be incorrect to think that the true Plankton animals are quite cosmopolitan.

From the *à priori* point of view, the warmth of the sun, different

in different latitudes, ought to regulate the distribution of real pelagic life; a system of belts would result parallel to the degrees of latitude; but this system of parallelism becomes disturbed by the action of currents and winds. On the other hand, by these currents sharper regions are defined; for, otherwise, all these zones would pass into each other gradually, which is not the case in reality.

Both north and south of the equator three circles of currents may be distinguished from 0–10, 10–50, 50–80° of latitude, and between them islands, as it were, of quiet water are formed, the so-called Halistases. These circles, *i.e.*, depending upon the temperature in them, constitute the most important factor in the distribution of pelagic life.

A current always shows differences of temperature at its outer limit, and by this circular arrangement the pelagic animals, as Brandt first laid stress upon, are kept within temperatures of a fairly constant degree; being always carried back to the circle-current of the temperature, and not being able to escape and flourish in the adjacent colder or much warmer water. This, as I wish to emphasise, is valid for the stenothermal animals. Eurythermal animals could be carried away to other regions, and continue to flourish there. Currents, therefore, act in two manners, either as helps to wandering, or as barriers to spreading.[2] In general, the latter will be the case for Plankton animals.

A division of the open sea into zones can, therefore, be accomplished. How great the difference can be on each side of a current may be seen chiefly at the Gulf—or Florida—Stream, as is illustrated by some instances noted by the Plankton Expedition.

While working out the Craspedota of this expedition, I could ascertain that no pelagic species of Medusa, occurring to the north of the Gulf Stream, is found also to the south of it, and *vice versâ*. The well-known *Aglantha digitalis* (described first by Forbes) has been caught constantly and in abundance from North England to Greenland. To the south of the Gulf Stream it ceases at once to occur. The same may be said of a Narcomedusa, *Solmaris multilobata*. On the other hand, *Aglaura hemistoma*, which has not been found in the north district, occurs with great regularity in the middle and equatorial part of the Atlantic, and the same can be said for the well-known *Rhopalonema velatum* and some species of *Liriope*.

Similar observations have been made in other groups of animals, for instance, in the pelagic annelids. The Tomopteridæ are abundant

[2] The occasionally abundant occurrence of animals generally rare or totally wanting at the British coast, can thus be explained. The animals in question are stenothermal animals of the Plankton. In ordinary years the Gulf Stream forms a sharp limit for them. If they are driven in the colder water by the current, they cannot grow there, but perish. Now, such uncommon occurrences have been recorded in years of uncommonly warm weather. Then the animals carried away from the south find a suitable temperature at the surface in the north, too, and the action of the current in such cases is dispersing.

in the north district, the Alciopidæ are wanting there, and occur only south of the Gulf Stream. For other groups, such as Appendicularia and Decapods hitherto worked out, the same sharp limit has been found. Most sharply defined limits are seen in the interesting pelagic copepod genus *Copilia*,* which is wanting in the north district, but occurs not only abundantly but in many species in the equatorial part.

It is remarkable that, in the eastern district near Ascension, other species than those occurring in the same latitude near Brazil have been found. I have stated a similar fact for some Medusæ.

The explanation of this is rather simple in connection with the influence of temperature. Here a southern colder stream passes; stenothermal animals which are adapted to colder water find here a temperature that suits them, while they cannot grow and flourish in the tropical waters near Brazil. The influence of this same cold current has been stated to exist by Studer and by the "Challenger."

The family of Geryonid Medusæ in general, to which the genus *Liriope* belongs, are a very good example for the influence of temperature on Plankton animals, as Corals are for the Benthos. The Geryonids seem to require warm water and occur only at a certain latitude. They are abundant in the Mediterranean, but are wanting totally in the German Sea and the north district in general. They are abundant in the equatorial part of the Atlantic, and the same may be said of the Pacific. I have obtained them in great number from the warmer currents, which the "Albatross" expedition encountered. I also got them from the Red Sea and the Indian Ocean, but none from higher southern latitudes.

It is in harmony with this evident need of warmth that the Geryonidæ are typical animals of the surface. This is obvious from the so-called "Stufenfänge," *i.e.*, captures made at the same station in 50, 100, 200 m. depth; and they occurred even in the cylindrical surface-net of the Plankton expedition, where many other animals which had been found in 100 m., etc., were wanting. It is known by direct observation how their umbrellas project above the surface of the water, especially during contraction, so that Italian fishermen call them "capelletti di mare," or "little sea-caps."

From Agassiz's last three cruises there is the same result. A *Liriope* was the most abundant Medusa, always appearing at the surface, even when *Stomobrachium* and other species had retreated to a depth of 100–200 m., to avoid the influence of daylight and warm sun.

Here we arrive at the third and last point, the distinction between vertical and horizontal differences of temperature, between which a parallel has been drawn hitherto, but upon insufficient grounds. Down to the depth of 400 m. it is allowable to make a comparison between the changes in the life-conditions, especially temperature in different depths and the changes of the temperature of the surface in different latitudes; but in greater depth this becomes impossible, the light, the

atmospheric pressure becoming quite different. Even the temperature alone cannot be compared, for near the surface it diminishes very rapidly, and then more and more slowly until we reach the temperature of the abysses. Moreover, at a given time the temperature of a certain depth may be the same as that of the surface in a higher latitude, but the variations are quite different. In the Mediterranean the daily variations cease totally at 18 m., the seasonal at about 400 m., and as we have seen, the variations are much more important than the average temperature.

It was formerly concluded on theoretical grounds that there could be as well many vertical belts of different life as there are horizontal ones, but this comparison is, as I have shown, not correct or can only be carried to certain depths, not exceeding 400 m. This is not a theoretical conclusion only. All the unerring data, proving a relation between the diminishing of temperature in horizontal and vertical direction, are obtained in depths within the 200 m. limit.[3]

Considering the above facts, I do not think the horizontal distribution of the pelagic fauna is compensated by the vertical differences, so that pelagic animals would find the same temperature that they have near the surface in higher latitudes in a low latitude in greater depth. Vertical migrations cannot be so extensive (owing to the small power of locomotion of the Plankton animals); and in the greater depth conditions prevail which exist nowhere at the surface, in no latitude, and in no time of the year.

Somewhat striking are the observations of Chun, who found the surface animals of the winter in the Mediterranean at considerable depth in the summer. Setting aside the question as to whether his apparatus was adequate to prove with certainty the depth from which the animals are supposed to come, his observations can be explained by the influence of temperature.

In the Mediterranean the deep water is much warmer than in the Atlantic; the retreating animals would, therefore, not find such extremes of temperature as in the open ocean (which is, as Agassiz has stated, in some places, at least, totally destitute of pelagic life in such depths). In the Mediterranean, therefore, an intermediate pelagic fauna would be able to exist under the special life-conditions.

This is in full harmony with the investigations of the Prince of Monaco on the survival of the deep-sea animals of the Mediterranean. Formerly it was thought that the great atmospheric pressure is the most important factor in deep-sea life, and it had been concluded that the deep-sea animals coming to the surface would be killed principally by the great difference of the atmospheric pressure; but that has been refuted by the Prince. Atlantic animals

[3] *Echinocardium kurtzii* } occur in the Florida stream in a depth of 25 m., in North
Moira atropos } Carolina at the surface.

Aeginopsis mediterranea { in the Mediterranean near the surface, in the tropical Atlantic in 200 m.

from 2,000 m. died when they came to the surface, while Mediterranean animals from 2,000 m. survived.

The Atlantic forms came from 0° to 20°, while those of the Mediterranean came from 13° to 20°. Hence it is obvious that temperature is here the most important factor.

In so far as real pelagic life is concerned, I therefore think that the existence of an intermediate fauna is not to be regarded as proved, neither in theory nor fact, and I would like to repeat Wyville Thomson's old sentence:—"The fauna of the deep water is confined to two belts, one near the surface, the other at the ground."

According to my opinion, the abyssal fauna is chiefly a Benthos fauna, more or less in connection with the bottom; a kind of *littoral* Plankton may be associated with it, but the true pelagic fauna does not exceed a certain depth.

I do not deny there is an intermediate zone of life, but it might be supposed to consist, scanty as it is, principally of Nekton, that is to say, of swimming animals, to which vertical distances are of no importance, and which are not sensitive to the changes of temperature. All of these are eurythermal, while, in contrast to them, most of the animals of the Benthos and Plankton are stenothermal.

In conclusion, after so much *theory*, I would merely repeat the *fact* that no case illustrates the great influence of temperature on the distribution of marine animals better than the well-known example of the Corals for the Benthos animals and the new one of Geryonid Medusæ for the Plankton.

Otto Maas.

V.

On the Dispersal of the Nutlets in certain Labiates.[1]

ONE of the commonest methods of seed dispersal in plants with capsular fruits is what is sometimes called the "censer mechanism." The seed-vessel is open at one end, and the seeds which lie loose at the bottom are thrown out by the swaying of the plant in the wind or by the disturbance of a passing animal. A familiar instance is afforded by the larkspur *Delphinium*, where the seeds collect at the bottoms of the follicles, and are easily scattered by shaking or striking the plant. Other instances are to be found in Hildebrand's well-known book on the dispersal of seeds (1). In some of the Labiates a condition of things biologically similar exists, but here the calyx plays the part of the censer, and it is the nutlets, not the seeds, that are shaken out.

In some cases arrangements are found which seem to render the dispersal more difficult, but it has been suggested that there is here a contingent advantage to the plant, because the period during which the seeds continue to be scattered is prolonged, besides which all the seeds are not likely to be thrown out in one direction, which would be the case if they fell out easily at the first waft of wind. The most usual arrangement is that the capsule stands upright on its stalk with a small opening at its upper end. Then, again, it often happens that the seeds are not free, but adherent to the placenta, so that a somewhat violent shock is needed to displace them. In the case of the Labiates it seems possible that the hairs which line the calyx in some species play the same part.

As a further refinement come those cases in which the capsule does not remain permanently open. Steinbrinck (2), Kronfeld (4), Beck (5), and others have described cases in which the mouth of the capsule closes in wet weather and opens again when the air is dry. A biologically parallel case has been noted by Rathay (3) in certain Compositæ where the involucre bends in and encloses the achenes in moist air. A similar state of things is described lower down among the Labiates.

The special advantage gained by these adaptations is not obvious. We have no evidence that the plant profits by the seeds or fruits

[1] Read at the Oxford meeting of the British Association, 1894.

being protected from wet, which is a possible end for such an adaptation.

Lastly come those cases of somewhat rare occurrence, in which the capsule or calyx, as the case may be, opens in damp air and shuts up when dried. These movements, with which others are co-ordinated, form the subject of the present paper. How far they are of biological importance to the plant it is not at present possible to say. It is only by the collection of a large number of instances that we can hope to arrive at a conclusion.

Hygroscopic movements, apparently adapted for the dispersal of seeds in wet weather, are confined chiefly, although not by any means entirely, to plants indigenous to countries having long periods of drought, during which the seeds, if dispersed, would have little chance of germinating.

Familiar instances of this phenomenon are found in the Crucifer, *Anastatica hierochuntina* (Rose of Jericho) and the Composite, *Asteriscus pygmæus* (Coss et Dur), both of which are inhabitants of deserts.

In 1878 Steinbrinck (2) drew attention to the fact that the capsules of two species of Veronica open more widely when sprinkled with water, and in 1883 he gave an account (6) of a similar movement in the capsules of *Caltha palustris* and several species of Mesembrianthemum and of Veronica. This was found by MacLeod (7) to hold true of *Veronica arvensis* and *V. serpyllifolia*. Volkens (8) and Schinz (9) have described other cases of hygroscopic movements of a like character.

Verschaffelt (10) has described the striking instances of *Salvia horminum*, *Brunella vulgaris*, *B. grandiflora*, and *Iberis umbellatum*. Finally, Ascherson (11) has given to these movements the name of hygrochasia; and, after reviewing critically the literature on the subject, he describes two new cases, *Lepidium spinosum* and *Ammi Visnaga*.

On examining about forty Labiates in fruit I have found fourteen species which show hygroscopic movements. Of these there are eleven species which move in such a way as apparently to favour the dispersal of their nuts in wet weather.

The method of observation was in each case as follows. The plant in the dry condition was drawn to scale. Water was then sprinkled on it, and when the consequent movements had taken place, a drawing was again made. In order to observe the more minute movements of the calyx-teeth, a single calyx was pinned down to a cork, under a microscope furnished with an eye-piece micrometer, and the change in the position of the teeth noted, when the calyx was wetted.

The movements may be classified in the following way:—

I.—The movements closely resemble those of *Brunella vulgaris* described by Verschaffelt (10).

The axis of the calyx is turned upwards so as to be almost vertical (the mouth being up), and the opening is more or less com-

pletely closed by the calyx-teeth. In rain the pedicels bend outwards until the axes of the calyces are nearly horizontal. At the same time the calyx-teeth bend out so that the mouth is open.

1. *Ziziphora capitata* (Linn.), Figs. 1–4.

A herbaceous annual, with erect stem. The flowers are borne in verticillasters, forming a dense terminal head. When in fruit, the four leaves immediately beneath the head are folded up round it, so as almost to enclose it. The calyces are almost erect, and the mouth quite closed by the teeth. The calyx is tubular, elongated, hairy outside, glabrous within, except for a thick ring of hairs at the throat. Rain or moisture cause a rapid movement to take place. The leaves folded up round the head bend out rapidly; the calyces bend outwards, so that they become more horizontal, and the calyx-teeth open. By sharply shaking the plant the nuts are thrown out to some distance.

EXPLANATION OF FIGURES.

Fig. 1. *Ziziphora capitata*; a capitulum in fruit; in the dry condition $\times 1\frac{1}{4}$ c.c.)

Fig. 2. The same; wet condition.

Fig. 3. Calyx of *Z. capitata*; dry condition.

Fig. 4. The same; wet condition.

Fig. 5. *Ziziphora tenuior*, a spike in fruit; dry condition. ($\times 1$.)

Fig. 6. The same; wet condition.

Fig. 7. A small portion of the spike of *Z. tenuior*, most of the leaves and calyces having been removed; dry condition.

Fig. 8. The same; wet condition.

Fig. 9. The mouth of the calyx of *Z. tenuior* in the dry condition; longitudinal section, showing the ring of hairs in the throat and the teeth closed.

Fig. 10. The same; wet condition.

Fig. 11. *Lallemantia peltata*; a node of the spike, all the calyces and bracts having been removed except two; dry condition. ($\times 1\frac{1}{4}$.)

Fig. 12. The same; wet condition.

Fig. 13. Mouth of the calyx of *L. peltata*, seen from the front; dry condition, the teeth closing the opening.

Fig. 14. The same; wet condition; the teeth having moved slightly apart.

Fig. 15. *Ocimum campechianum* (?); part of the spike in fruit; dry condition. ($\times 1\frac{1}{2}$.)

Fig. 16. The same; wet condition.

Fig. 17. A calyx of *O. campechianum*; dry condition; the others in same whorl having been removed.

Fig. 18. The same; wet condition.

Fig. 19. The same; longitudinal section, showing the hairs at the throat.

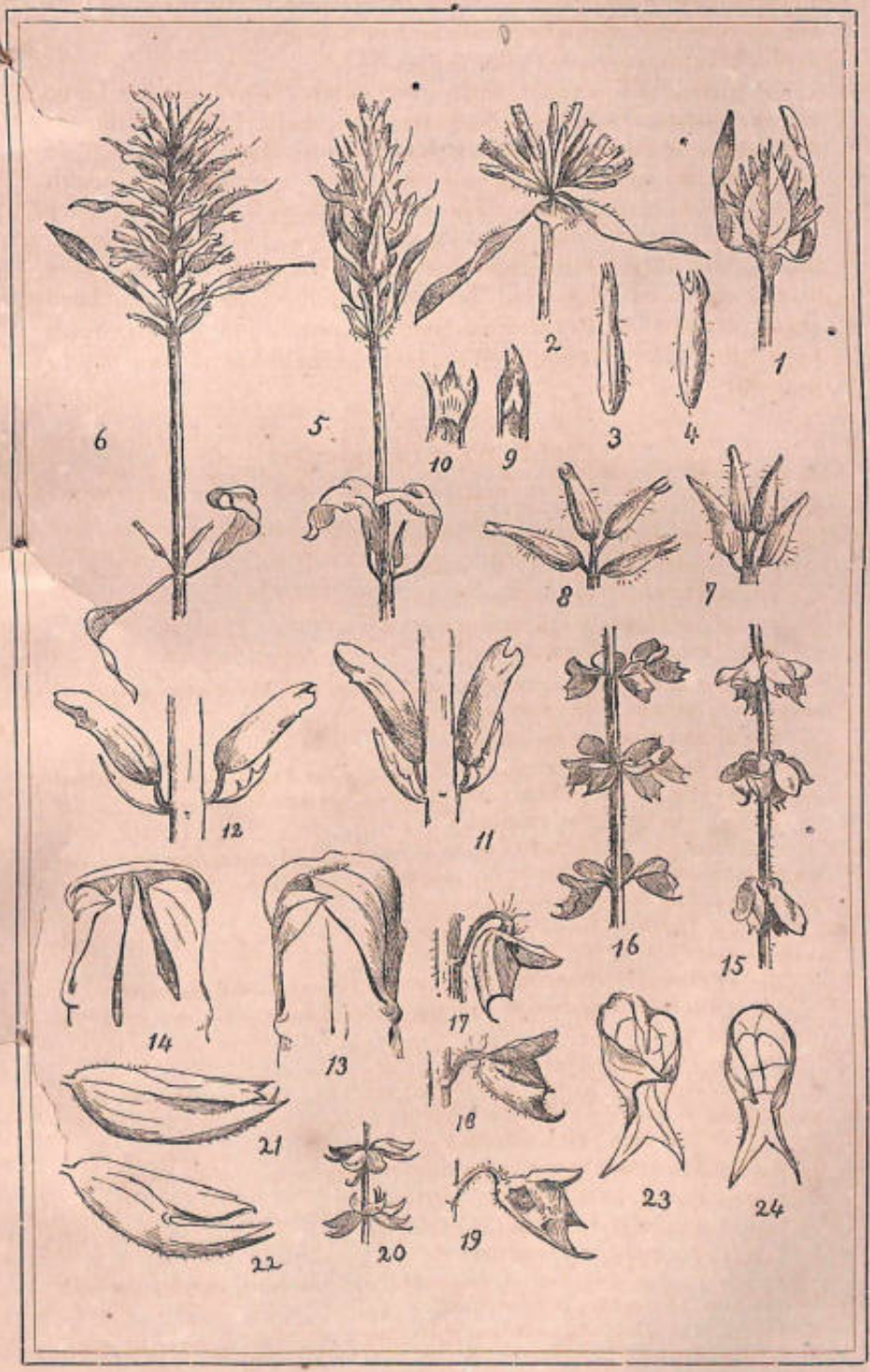
Fig. 20. Part of the spike of *Salvia gigantea*. ($\times 1$.)

Fig. 21. A calyx of *S. gigantea*; dry condition.

Fig. 22. The same; wet condition.

Fig. 23. *Lamium flexuosum*. A calyx seen from the front, showing the teeth curved across the opening; dry condition. ($\times 5$.)

Fig. 24. The same; the teeth bent apart; wet condition.


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Habitat: Region of the Mediterranean and South Russia—fields and hills.

2. *Ziziphora tenuior* (Linn.), Figs. 5–10.

A herbaceous annual. The flowers are in few-flowered axillary verticillasters, forming a spike. The calyx is like that of *Z. capitata*, but more hairy externally. The movement is similar to that of *Z. capitata*. Both the floral leaves and calyces, which when dry are turned up round the main axis, bend out when wet away from it, and the calyx-teeth open.

Habitat: Region of the Mediterranean and South Russia—hills and uncultivated fields.

3. *Ziziphora hispanica* (Linn.). (Only a herbarium specimen of this was examined.) The stem is branching and herbaceous; the flowers grow in dense spikes. The movement is the same as in *Ziziphora capitata*.

Habitat: Spain.

4. *Ziziphora taurica* (Bieb. Fl. Taur. Cauc.). (Herbarium specimen.)

The flowers are in long leafy spikes; the movement the same as *Z. capitata*.

Habitat: Mount Taurus.

5. *Brunella hyssopifolia* (Lam. fl. fr.).

The movement is the same as that of *B. vulgaris* described by Verschaffelt.

Habitat: Region of Mediterranean Europe—open, dry places.

6. *Lallemantia peltata* (Fisch. et Mey.), Figs. 11–14.

An erect herb, with six-flowered verticillasters in the axil of sessile bracts. In fruit, the tubular calyces are nearly erect, and the bracts subtending them are folded up so as partially to enclose them. The pedicels of the calyces are flat and broad, being compressed in the median plane. The teeth are unequal, the posterior one being much the largest, flat and slightly curved forward over the four anterior teeth which almost close the mouth. In consequence of wet, the bracts and calyces bend down and outwards from the main stem until the axis of the calyx is at about an angle of 45 degs. At the same time, the four anterior calyx-teeth move slightly apart so that the opening is enlarged and the nuts are shaken out much more easily than when dry.

Habitat: Persia, Armenia, near the Euphrates, Kurdistan, Mount Elburz.

II.—The axis of the calyx points more or less downwards, and in consequence of wet it bends up until nearly horizontal. *Salvia horminum*, described by Verschaffelt, is the most marked example of this.

1. *Ocimum* [probably *campechianum*] (Mill), Figs. 15–19.

The stem is erect; the flowers are arranged in six-flowered verticillasters. The calyx when in fruit is bent downwards. The posterior tooth is broad and obtuse, with winged and decurrent

margins; the other teeth are ovate; the two lateral ones very shortly pointed, the two anterior ones more pointed. The mouth is open, but a ring of hairs closes the throat.

In consequence of wet the pedicel bends up so that the calyx becomes horizontal; the two anterior teeth bend slightly down, and the two lateral teeth move a little apart, so that the opening of the mouth is slightly enlarged. The movements of the teeth can only be seen under the microscope.

Habitat: Central America.

2. *Melissa officinalis* (Linn.).

The stem is erect; the flowers are in lax, few-flowered axillary verticillasters. The calyx is pedicellate, two-lipped, slightly inflated, the mouth open. The upper lip is spreading, truncate, shortly three-toothed; the lower lip is bifid, the two pointed teeth either pressed against each other or crossing each other. The throat is lined with a ring of hairs, which closes the mouth. The whole calyx is rather deflexed.

In consequence of wet the pedicels straighten, so that the calyces are raised to a nearly horizontal position. The anterior teeth are also lowered very slightly.

Habitat: S. Europe and M. Asia—woody hills.

3. *Salvia sclarea* (Linn.).

The flowers are arranged in six-flowered verticillasters. The calyx is shortly pedicellate, hairy, slightly bent downwards. The upper lip is tridentate, the lower bifid with acuminate teeth. In consequence of wet, the pedicels bend slightly up, so that the calyx is raised, and at the same time the teeth of the lower lip are very slightly lowered.

Habitat: S. Europe,—roads and dry places.

4. *Salvia gigantea* (virgata, ait. hort. kew.), Figs. 20–22.

The flowers are arranged in six-flowered verticillasters. The calyx is shortly pedicellate, campanulate, hairy. The teeth of the lower lip bend up so as nearly to close the mouth. The movement in consequence of wet is the same as in *Salvia sclarea*, but the downward movement of the lower lip is more marked.

Habitat: Italy.

III.—The movement is confined to the calyx-teeth.

Lamium flexuosum (Ten. fl. nap.), Figs. 23, 4.

The stem is erect; the flowers are in dense axillary verticillasters. The calyx is bent downwards below the horizontal, subcampanulate, lined with short hairs. The mouth is partially closed by the teeth, the posterior tooth bending down forwards, and the two lateral ones bending across, so as to meet.

In consequence of wet the posterior tooth bends up until it is horizontal, and the lateral teeth bend out sideways, so that the nuts can be shaken out more easily.

Habitat: Liguria.

Besides the eleven species described above, there are three which also move hygroscopically, but in a different way.

These three, *Hyptis apectinata*, *Elsholtzia cristata*, and *Nepeta kohamerica*, have the axis of the calyx almost erect or else horizontal, and the mouth not closed except by hairs, and in consequence of wet the teeth close slightly. This movement is probably not connected with the dispersal of seeds, but with their protection from wet.

1. *Hyptis pectinata* (Poit. ann. Mus.).

The calyx is almost regular, campanulate, the mouth open, with spreading pointed teeth.

The flowers are arranged in cymes, which, when in fruit, are elongated, unilateral and pectinate, with the calyx turned vertically upwards.

In wet the peduncles bend up so as to bring the calyces near the main stem, and the calyx-teeth bend up so as to close the mouth.

Habitat: Equinoctial America—dry chalky and sandy places. Mexico, Tropical Africa, etc.

2. *Elsholtzia cristata* (Willd).

The flowers are in many-flowered verticillasters forming dense unilateral spikes. The calyx, in fruit, is regular, campanulate, the exterior hairy and glandular, the interior glabrous except for long hairs on the margin of the teeth which almost close the mouth. The axis of the calyx is horizontal or inclined upwards.

In consequence of wet, the calyx-teeth close together partially.

Habitat: Asia, and Europe introduced.

3. *Nepeta kohamerica* (Regel).

Stem erect branched. Flowers in terminal spikes. The calyx is hairy and tubular, with sub-oblique mouth, the calyx-teeth lanceolate. In consequence of wet, the calyx-teeth close slightly together.

Habitat: Plains of Kohamyr.

With the exception of the three last cases, the movements described above appear to favour the dispersal of the nuts in rain, yet it is not easy in every case to demonstrate this fact. In *Lallemantia*, the nuts are far more easily shaken out in the wet condition, but in *Ziziphora tennior*, which has similar movements, this was not found to be the case. Possibly this may be due to the nuts not being ripe.

Salvia sclarea and *Melissa officinalis* have been placed with the others, but their movements are so slight that it is difficult to see how they can affect the dispersal of the nuts. In *Ocimum*, too, although the movement is very decided, yet the advantage gained is not evident.

In conclusion, I should like to express my thanks to Mr. F. Darwin, for having suggested that I should examine Labiates in regard ot the distribution of their seed, and also for his very kind help and criticism.

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DOROTHEA F. M. PERTZ.

VI.

Hertwig's "Preformation or New Formation."[1]

Part III.

DR. HERTWIG entitles the concluding part of his paper, "Thoughts Tending to a Theory of Development of Organisms." He professes, not to work out a theory in detail, but to indicate the direction in which guidance is to be obtained. Let it be remembered that in his criticism of Weismann's preformationist views there were two main points. First, he thinks not only that Weismann has failed to prove the existence of "heirs-unequal division," but that there are many grounds for supposing that all cell-division is a case of "heirs-equal" division. Hence there is no reason to suppose that the various elements of the germ-plasm are marshalled to their places in the course of an individual development, and that this sorting out of preformed determinants is the reason why an egg produces with imperative fidelity the thousand different cells of the adult. Every cell of the whole organism, so far as inherited dower goes, is identical. Next, Hertwig throws doubt upon the whole conception of determinants, pointing out that very many of the individual characters of organs and of organisms are not properties of cells at all, but are properties of aggregates of cells, and therefore cannot come into existence until aggregates arise, and certainly can have no preformed determinant within a single cell.

Hertwig is disposed to steer a middle course between preformation and epigenesis, to cull from either theory what seems reasonable and in consonance with observed fact, and to reject much of the speculation with which both preformationists and epigenesists have rounded off their conception of Nature. As yet, we are far from having a complete explanation of the problems, but it seems to him as if Nature were a follower neither of Weismann nor of his opponents, but evenly pursued a middle way.

It is necessary to suppose that each organism starts with a specific dower. However much can be attributed at present, or may come to be attributed, to the moulding forces of environment, there

[1] Zeit- und Streitfragen der Biologie. By Professor Dr. Oscar Hertwig. Pamphlet I. Präeformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen. Pp. 144, with 4 illustrations in the text. Jena: Gustav Fischer, 1894. Price 3 marks.

remains a large residuum, which can be explained only by the transmission of a specific plasm from the parent to the sexual cell. In the same tank of sea-water, surrounded by the same baneful and useful circumstances, there may lie two fertilised egg-cells not differing appreciably in appearance, in size, in chemical composition, or in physical characters. Yet one of these produces a sea-hedgehog, another a worm. The resemblances between the eggs of species of the same genus may be still more remarkable, and the distinctive characters may not appear till a late period in the course of the individual development; but the different specific characters do appear, although there is nothing in the environment to cause them. The egg of a barn-door fowl is as fully stamped with the character of its species as is the adult animal.

To explain the process of development, Hertwig believes that we must postulate the existence of a material which contains germs or incipia of a particulate character, and which possesses an extraordinarily complicated structure corresponding to the marvellously minute and complicated different ways in which it must react, as the growing organism is built up by cell-multiplication. This material differs from Weismann's conception of germ-plasm in many ways. First, it is not built up of determinants and so forth into a complicated architecture. Hertwig makes no supposition whatever as to the physical nature of his specific material. Next, it by no means contains all the determining causes of the organism. Many of these are due to external causes: many more to the reaction between external causes and the material itself. In this respect, it is true, there is no vital difference between the two theories. Weismann at first attributed too much to the germ-plasm and overlooked a number of ways in which the environment plays a direct part in ontogeny. But recently he has reviewed and accepted many external causes which he had hitherto passed by. Both Weismann and Hertwig regard this specific material as containing the residual factors of development. Weismann may understate, or perhaps Hertwig overstate, the forces which have to be allowed for before the residuum is reached; but both are agreed that there is a necessary residuum for which a specific inherited material must be assumed. We come now, however, to the great point in which the two views are wide as the poles asunder. Weismann, as everybody knows, attributes to the sexual cells, and to special regions called germ-tracks, the exclusive possession of the germinal plasm, making an exception only of a few cases of special and probably secondary adaptations. In consequence, he draws a sharp distinction between the cells which contain the plasm and the somatic cells. New organisms arise only from cells containing germ-plasm, and the somatic cells, when they become specialised, lose the power of reproducing the species as they contain, not germ-plasm, but a simpler derivative of it. Hertwig, on the other hand, totally denies this sharp distinction. All the cells in the body receive an equal dower of the specific

material, because in cell-division there is always an equal division. No doubt sexual cells, for the most part, arise only in special parts of the organism. But, in these cases, the fact that certain regions of the body give rise to the sexual organs is merely a part of the general ordering of the body into tissues and organs, and is not due to the presence in sexual cells of any special plasm. Hence it is not surprising to find in special cases diffused powers of reproduction or of regeneration, any more than it is surprising to find muscle-cells or gland-cells in unwonted situations.

In one matter, the possibility of the inheritance of acquired characters, a question which bulks more largely in popular controversy than in Weismann's theory, the two views do not differ theoretically. Weismann admits the possibility of the influence of outside forces modifying the germ-plasm in the cells, and as Hertwig's specific material is diffused through all the cells of the body, it too is not removed from the possibility of being altered by the environment. Hertwig, however, does not specially deal with the controversy as it is known, but one draws the inference from the general tendency of his views, that his specific material is supposed to have as much organic stability as the germ-plasm.

The epigenetic part of Hertwig's views about development consists of an account of many of the factors, apart from a specific plasm, which direct the development. The leading motive in his account of these is one diametrically opposed to Weismann's view that a cell must become what it is, and develops only in accordance with the determinants which it received from among the determinants in the parent cell from which it arose. Hertwig believes that a cell becomes whatever it is made to become by the forces round about it. The limit to this is given by the specific material with which it is endowed. A cell of one animal could not be made into the cell of another, but the cell of an animal might have been any other cell of the animal had its environment been different. In the words of Driesch, the specialisation of a cell is a "function of the locality of the cell in the organism." The causes of the elaboration of an organism from its germs or incipia lie in conditions outside the incipia in the egg-cell, but which, nevertheless, follow in regular and orderly sequence. The first of these causes is the continual change in the relations of cells to each other and to the external world as the organism is built up by cell-multiplication. Physiologically speaking, the differentiation of cells in different directions is due to the reaction of the organic substance to different external stimuli. The capacity of the cell to assimilate or to go through metabolic changes in the case of the fertilised egg-cell produces the cleavages by which the morula is formed. The particles of the protoplasm were arranged round the nucleus of the egg-cell as a single centre of forces. In the morula they are arranged round a number of new centres corresponding to the new nuclei. Thus, even in equal-heirs

division, differences arise in the cells according to their position and relations to each other, and the cells are different qualitatively from the original cell from which they sprang.

In many respects the form assumed by the cell-masses is a function of organic growth. The original cell, from the physical nature of the protoplasm, naturally has a spherical shape, and the retaining of this shape by the cell-mass produces differences in the shapes and surfaces of the constituent cells. The next important factor is the necessity that growth shall take place so as to secure the greatest possible extension of surface in proportion to mass. A crystal grows by the deposition of new particles upon the outside; organic matter grows by the intersusception of matter from the different constituents in the environment, and it is therefore necessary that as large a surface as possible shall be exposed to the environment in order that the varied substances may be picked up.

The organic material in process of growth can assume only those forms which allow it to remain in touch with the outer world. A cell-mass could not grow indefinitely thick without the inner cells being deprived by the outer cells of their relations to the environment. For this reason, cell-multiplication produces threads and layers and membranes, rather than solid masses. In the plant kingdom the cells, for the most part, are strengthened by a firm cell-wall, through which their liquid and gaseous food is able to pass. The possession of this cell-wall enables plant growth to remain, for the most part, in the form of threads and membranes, and the necessary extension of surface is produced in the simplest possible way. In the adult plant all the organs and tissues grow out so as to preserve this simple extension of surface. In the animal kingdom the method of nutrition by the absorption of solid masses into the interior of the cells prevents the formation of thick cell-walls. Animal growth, then, cannot rely upon the nature of the individual cells to secure the necessary rigidity and coherence of the whole mass. The cell-masses grow so as to form hollow bladders and tubes, the whole structures possessing a rigidity not found in the individual cells, and growth takes place by the infolding of the single layers of cells which form the walls of the spheres and tubes. In plant-tissues the food-supply comes from the water and the air of the outer world, and in consequence the extension of surface is as much as possible an extension turned to face the outer world. In animals the solid food is taken into an internal cavity, where it is broken up and ingested, and in consequence there is as great as possible an extension of this inner surface. Thus, the blastula is a hollow sphere, the walls of which consist of a single layer of cells which retain contact with the environment on their inner and outer surfaces. An inpushing of the wall of the blastula gives rise to the gastrula, and subsequent infoldings in the inner and outer walls of that give rise to muscle layers, nerve layers, cœlomic pouches, and so forth. But in all these

changes the cells in different places find themselves in very different circumstances; those lining tubes and hollows inside the body are brought in contact with very different external forces from those lining the outer side of the body. In correspondence with these varied stimuli the organic material, the same in all the cells, responds in different ways, producing cell-specialisation.

In the epigenetic modification the next process which helps to produce the specialisation of the organic body is of a physiological nature. Every cell is an elementary organism, endowed with the capacities of elementary organisms. But the cells resulting from the division of the egg to a certain extent lose their individuality in the new individuality of the whole. The purely morphological method of regarding cells tends to exaggerate their individual importance. Although the whole shape and form of the organism is produced by cell-multiplication, yet, considered as an organism, the multicellular adult is better regarded as a continuous mass of an organic material. Hertwig details many of the facts showing the intimate correlations which exist between different parts of organisms, and concludes that many of the elaborations of development must be set down to the observed fact of correlative growth. This part of his argument is perhaps more conclusive in so far as it is a criticism of Weismann's doctrine of the existence of associated determinant to account for correlations, than it is in itself satisfactory. Weismann may have failed completely in giving a plausible explanation, or he may have underrated the importance of correlation as a factor in growth. Hertwig simply associates the two observed facts of the physiological unity of organisms and the existence of correlated growths.

He elaborates at some length his view that different external influences cause the development of identical incipia into different final results. In course of this, he reviews the controversy between Spencer and Weismann relating to the polymorphism of social insects, and concludes with Spencer against Weismann that Natural Selection is not the factor at work, but that the different environments of the different forms have produced the divergent modifications. He sees in it the same phenomenon as the production of roots from stems, or of the upper part of a hydroid from the cut lower part. It is one of the results of the identity of the organic matter all through the cells and tissues of an individual, and of the capacity of this identical matter to respond in varied ways to varied stimuli.

The three great points of his theory are these. First, all organisms possess a specific plasm of exceedingly complicated nature. Secondly, the sexual cell, which is the origin of a new organism, possesses a share of this plasm, and, being an elementary organism, is able to grow and multiply. In this process of multiplication, the resulting cells, formed as they are by equal-heired division, receive each an unmodified share of the initial plasm, which, like the cells

containing it, increases in bulk by growth. Thirdly, the specialisation and differentiation of the organs and tissues is due to the action of varied external stimuli upon the same organic substance. In his own words :—

"My theory may be called *evolutionistic*, because it assumes the existence of a specific and highly-organised incipial plasm as the basis of the process of development. It may be called *epigenetic*, because the incipia grow and elaborate from stage to stage only in the presence of numerous external conditions and stimuli beginning with the metabolic processes preceding the first cleavage of the egg-cell, until the final product of the development is as different from the first incipia as adult animals and plants are from their constituent cells."

P. CHALMERS MITCHELL.

SOME NEW BOOKS.

A Living "Palæontology."

Éléments de Paléontologie. By Félix Bernard. Seconde Partie. 8vo. Pp. 529–1168 and i.–viii. Paris: Baillère & Sons, 1895 (August, 1894). Price 15 francs; price of the complete work 25 francs.

We expressed our appreciation of the first part of this book in April of last year (Natural Science, vol. ii., pp. 307–308), and after carefully examining this second part, which deals with Lamellibranchs, Cephalopods, Vertebrates, and Plants, we do not feel called upon to modify the opinions previously given. In many respects this text-book of palæontology is superior to any other with which we are acquainted, and we should hesitate to say that it is in any respect inferior to those others. Its virtues are its own; its vices are common to all similar works. It is, therefore, more useful as well as more pleasant to praise the former than it is to point out the latter.

Dr. Bernard's chief merit is that he treats extinct animals from the standpoint of the biologist, laying stress on such as are of morphological importance while passing lightly over those genera and species that have acquired a fictitious value as the tokens or date-counters of the stratigraphical geologist. Evolutionist as well as biologist, he bases his classification in each group on its ascertained phylogeny, and remembers that in this difficult study evidence must be collected from the palæontologist, anatomist, and embryologist alike. Thus every fact that finds its place in this book is of a definite value, and is quoted for a definite purpose. Joined by the thread of scientific theory, such facts are easily retained in the memory of the student, and the work, instead of being a dry cram-book or catalogue, assumes the form of an interesting narrative, where the deepest truth proves the highest art. Since we can never have the conclusion of the lamented Neumayr's "Die Stämme des Thierreiches," we are grateful to Dr. Bernard for so soon applying the methods of that distinguished writer to a working text-book for students.

Turning to the execution of this volume, it is plain that Dr. Bernard has taken much trouble to get up the recent literature, and has spared no pains to discuss all views of any importance. Thus, over two pages are devoted to an account of the Ammonite aptychus and of the various explanations of it, while the different theories as to the origin of the Horse are admirably summarised. The errors that mark the work of a compiler, as distinct from that of an original investigator, are, however, easily to be found. To ascribe to Smith Woodward the views quoted on p. 800 as to the crocodilian skull does no great harm; it merely shows that the author has obtained his knowledge of fossil crocodiles from Mr. Woodward's semi-popular summary rather than from the original

sources. Similarly, though Dr. Bernard makes a point of ascribing his figures only to their original authors, we note some curiously erroneous ascriptions; for instance, fig. 102 is not by Lydekker but by David Page, while Nicholson should hardly be credited with the well-known figure of the teeth of *Cochliodus* by Agassiz. To take one more instance, no man that had ever examined a *Sepia* shell for himself could have called the "pad" the homologue of the pro-ostracum of the Belemnite, or could have denied that the posterior end was septate. Lapses such as these will, however, always and inevitably be found in the work of those writers who, in Transatlantic phrase, bite more than they can chew. The critic is obliged to point them out, and when they rise above a certain percentage he is obliged to be severe, though some offending authors are of the contrary opinion; and possibly the authors are not wholly wrong in their protest, for after all the people that really deserve chastisement are those who encourage young and untried writers to grapple with a subject that demands the co-operation of numerous specialists. The authors are whipping-boys for these, the true offenders, so let the strokes be lightly laid on!

To detail the treatment of the various groups dealt with in this second section would be lengthy and wearisome. We shall only venture on a few remarks.

The account of the Cephalopoda is clearly influenced by Munier-Chalmas, a fact by no means to be regretted, for that naturalist, though he has undoubtedly thought much, has published little on this fascinating subject. Hence we find the order Ammonoidea removed from the Nautiloidea, and boldly placed alongside of the Decapoda and Octopoda. We admit that very much may be said in favour of this association; but it is possible to go too far. A short time ago Mr. H. Woods informed the world that the Ammonoidea possessed "two pairs of gills, two pairs of auricles, and two pairs of kidneys." Now Dr. Bernard states that they are "pourvus de 2 branchies et de 2 oreillettes," and that they have a "Système nerveux à ganglions bien délimités." It is clear that these gentlemen cannot both be right; and, as neither of them has as yet offered an atom of proof in support of his assertions, it is perfectly open to us to maintain that neither of them is right. Indeed, Dr. Bernard admits that if we knew the animals that inhabited the Ammonite shells we should probably attach less importance to names derived from the number of gills. Why continue to assume a knowledge though you have it not? Owen's classification was excellent and the obvious one, so long as attention was fixed on recent Cephalopoda; but the researches of Branco, Hyatt, Munier-Chalmas and others on extinct Cephalopoda have made its retention, even in a modified form, a conspicuous absurdity.

To the families that constituted the Dibranchiata of Owen the name "Belemnoïdes" is here applied, a name that has always struck us as singularly unhappy when thus extended to Cephalopods unprovided with a belemnon or guard and even in some cases without a shell. The account of this order is well up-to-date, but we regret still to find statements that we believe incapable of proof. *Spirula*, for instance, is surely not the "Belemnoid" most nearly allied to the Nautiloid and Ammonoid type. It is coiled in the inverse direction, while the statement that its body is for a considerable period contained within the shell is a pure assumption. It is probably descended, as Dr. Bernard admits, from *Spirulirostra*, and is therefore separated from the coiled Ammonite shell by all its

ancestors at least as far back as the Trias. We believe that the Belemnoteuthidæ form the starting point of the Chondrophora rather than of the Phragmophora or Osteophora; but the question admits of argument. Neither can we agree with Dr. Bernard when he states that without doubt the Tetrabranchs have given birth to the Dibranchs, or when he maintains that some Goniatites are directly derived from some coiled Nautiloids, or when he writes that the Chondrophora are manifestly descended from Belopteridæ and Sepiadæ.

In the pages allotted to Vertebrata, the author hardly acts up to his principles in devoting so small a space to the Amphibia or "Batraciens." The extinct Stegocephali possess a peculiar interest as vying with the Theromorpha for the honour of Mammalian ancestry. Dr. Bernard indeed inclines to derive Mammals from the Theriodont Reptiles, but there is much to be said in favour of their direct descent from Amphibia. It is not so much the peculiar skeletal characters, such as the two occipital condyles, the single temporal arcade, the bony symphysis of ischium and pubis, that are in the way of their descent from Reptiles, for these characters were present in the lowest Reptiles as well as in the highest Stegocephali. But it certainly seems easier to derive the Mammalia from the ancient Amphibia with primitive vertebral column, anteriorly directed ilium, five-fingered hand and glandular skin, than it does to derive them from any such forms as had advanced in the direction of the Reptilian type. At any rate, these are questions not yet settled, and for a more complete understanding of them the student desires a better account of the Amphibia than is given by Dr. Bernard, as well as more reference to the writings of E. D. Cope.

Passing over the remaining chapters on Vertebrata, we need merely mention that the dentition of the Mammalia is very clearly described, with figures and diagrams explanatory of the numerous terms now employed. This part concludes with a fuller account of ancient man than is usually found in works of this class and size.

Having devoted 800 pages to the animal kingdom, our author proceeds to compress the whole of Vegetable Palæontology into 87 pages, which 87 pages we have little hesitation in saying are simply wasted. No one that wishes to study the subject is likely to come here for his information, while the zoologist will only grumble at the additional size of the book. The classification employed in this part would have been the better for a little revision. The essay on the phylogeny of plants pays scant attention to the views of anyone but the Marquis de Saporta. No mention is made of Treub's work on *Casuarina*, or more recent work in Russia and in this country on some of the Cupiliferæ, all of which has a very important bearing on phylogeny. A mistaken sense of duty, probably on the part of the publishers, was no doubt the cause of the insertion of this inexact and insufficient chapter.

The book closes with a few additional notes, some errata, and an excellent index. We note that the author did us the honour to read our former remarks, though he does not seem to have fully understood them.

Till better methods enable a more accurate text-book of palæontology to be compiled, we recommend this to the notice of students. It is always interesting, often original, yet judicial, well illustrated, and on the whole very good value for a sovereign.

F. A. B.

THE PHYSICAL GEOLOGY AND GEOGRAPHY OF GREAT BRITAIN. By the late Sir Andrew C. Ramsay, LL.D., F.R.S. Sixth Edition, edited by Horace B. Woodward, F.G.S., of the Geological Survey. London: Edward Stanford, 1894.

A SCIENTIFIC book which goes through five editions in fifteen years has, we may be sure, either something very attractive about it, or contains a store of information for which there is a steady or growing demand, or has the good luck to combine both claims to popularity. That the last was the case with the late Sir A. Ramsay's "Physical Geology and Geography of Great Britain," all those who know the book, and this includes everyone who is interested in its subject, will be prompt to admit. It is not merely that it gave a summary, condensed but singularly clear, of the bare facts, stratigraphical and palæontological, connected with the geology of our islands; there are many who possess the knowledge that would enable them to do this perhaps equally well. But these dry bones were made to live; clothed, as it were, with flesh and muscle and organised tissue, when it was shown how intimate was the connection between them and the scenery, the climate, the industry, and the racial peculiarities of our native land. A further charm lay in the enunciation of sundry theoretical views, due largely or wholly to the brilliant ingenuity of the author, many of which hold their own, or have even been strengthened by the advance of knowledge, while some are still matters of dispute. And all was done in such a way that the book was a vivid reflection of the individuality of the author. Those who had the good fortune to know him intimately, had brought back to them the genial talk which was the echo of his strong convictions, and the enthusiastic emphasis, tinged and heightened by a steady glow of humour, with which he insisted on the doctrines that he held to so tenaciously. Time has weakened but little, for the writer of these words, the remembrance of those "dies noctesque deorum"; and for him, and many another, no lapse of time can weaken the admiration and love for the master that grew out of them.

But other things are more transitory. Editions become exhausted, to the profit, we may hope, both of publisher and author. The growth of knowledge demands, in the new edition, corrections and additions, maybe omissions, to bring it up to date. With the majority of books the revision presents no great difficulty, and requires no more than sound judgment and an acquaintance with the latest phase of the science they treat of. In the case of such a book as that now before us, there must be besides the sympathy which enables the reviser to feel wherein lies its individuality, and the tact which will put right what is in obvious need of modification without impairing the special flavour of the work.

It is gratifying to find that these points have been kept in view by the editor of the sixth edition. Without attempting a minute collation of it, and the edition that preceded it, we are convinced that every attempt has been made to place the book on the level of the knowledge of to-day. A sketch is given of the various views held as to the British pre-Cambrian rocks. *Olenellus* is figured; the discovery of *Conocoryphe viola* is recorded; notices of Overthrust faults, of Mr. G. Barrow's paper on Themo-metamorphism, and of the occurrence of Radiolarian Chert in the Glenkin Shales and Mullien Island are inserted. The researches of Mr. H. T. Brown on the Permians of Leicestershire, and of Mr. Clement Reid on the Cromer

Section, receive due recognition. These examples, picked at random, are types of what has been done to keep pace with the march of geological research.

Without wishing to be over critical, one of his points may be noticed where there is still perhaps room for improvement. It is hard to see why, in the table on page 400, the Devonian should be sandwiched in between the Upper and Lower Old Red Sandstone. In putting forward the conjecture that stratified rocks may possibly be liquefied by intense metamorphism (p. 36), a word of caution might have been usefully added. Surely Dura Den is not the locality for *Anodonta jukesii* and *Palæopteris* (p. 94); nor can there be any doubt now as to the age of the Reptiliferous Sandstone of Elgin (p. 128). The episodal character of the Corallian Beds is hardly brought out with sufficient distinctness; and in the question of nomenclature between Neocomian and Lower Cretaceous there is something like a halting between two opinions; Glauconitic would have been better than Chloritic Marl. In the case of the Upper and Middle Eocene, the time has come to make their representatives in Hampshire and the Isle of Wight, about which our knowledge is fairly complete, the type, in place of the Bagshots of the London Basin, about which we know (*pace* Dr. Irving) scarcely anything. In the case of the Oligocene, more might have been said about the great earth movements which culminated towards the end of that epoch. Some objection may be taken to what is said about the shell-bearing gravels of Macclesfield. It is hardly possible for anyone who has seen these beds in section and collected their fossils to believe they can have been driven up by an ice-sheet from an adjoining sea bottom. The perfect state of many of the shells may be accounted for by supposing that they were frozen into the matrix during their journey. But anyone who has noted the bedding and current-bedding of the gravel can hardly fail to be convinced that they are subaqueous deposits laid down where we now find them. That the shells indicate different depths of water is natural, for the gravels were probably formed during subsidence and an increasing depth of the sea.

The later chapters, though judiciously modified, are happily allowed to retain much that gave to the book from the first its distinctive character. Lastly, by omissions, none of any serious moment, and rearrangements, often resulting in manifest improvement, the book has been reduced from 639 to 421 pages.

A. H. Green.

The Royal Natural History. Edited by Richard Lydekker, B.A., F.R.S. London: Frederick Warne & Co., 1894. In monthly parts. Each, price one shilling nett.

Ten numbers of this interesting popular Natural History have now been published, and the public, Mr. Lydekker, and Messrs. Warne and Company must be congratulated. The get-up of the volume is all one could wish for, and the illustrations, of which some are taken from Brehm's well-known "Thierleben," while others are specially prepared for this work, are a great improvement on anything that has been published before. The coloured pictures, on the whole, we do not care for, though some of them are well done. The first two parts of the work are devoted to the monkeys, the third to the lemurs and bats, the fourth to the hedgehogs, moles, and cats, which latter continue into the fifth, and are succeeded by the genets, civets, and



BEARS WALKING.
(From Lydekker's "Royal Natural History.")

mongooses. In the sixth part are fossil hyænas, dogs, and foxes; in the seventh, bears, raccoons, and weasels; in the eighth, seals and their kind, and we are introduced to the oxen, which continue in part 9, and are succeeded by the sheep, goats, and antelopes. Part 10 carries us well into the deer. Mr. Lydekker's descriptions are distinctively and pleasantly written, and much interesting and anecdotal matter is incorporated. Special attention too has been paid to the occurrence of each form, when known, in the fossil state.

Taken as a whole, we do not know of any work that may be so satisfactorily recommended to those who wish an acquaintance with the mammalia. An additional interest will be found in the living animals at the Zoological Gardens if studied family by family by the aid of Mr. Lydekker's Natural History.

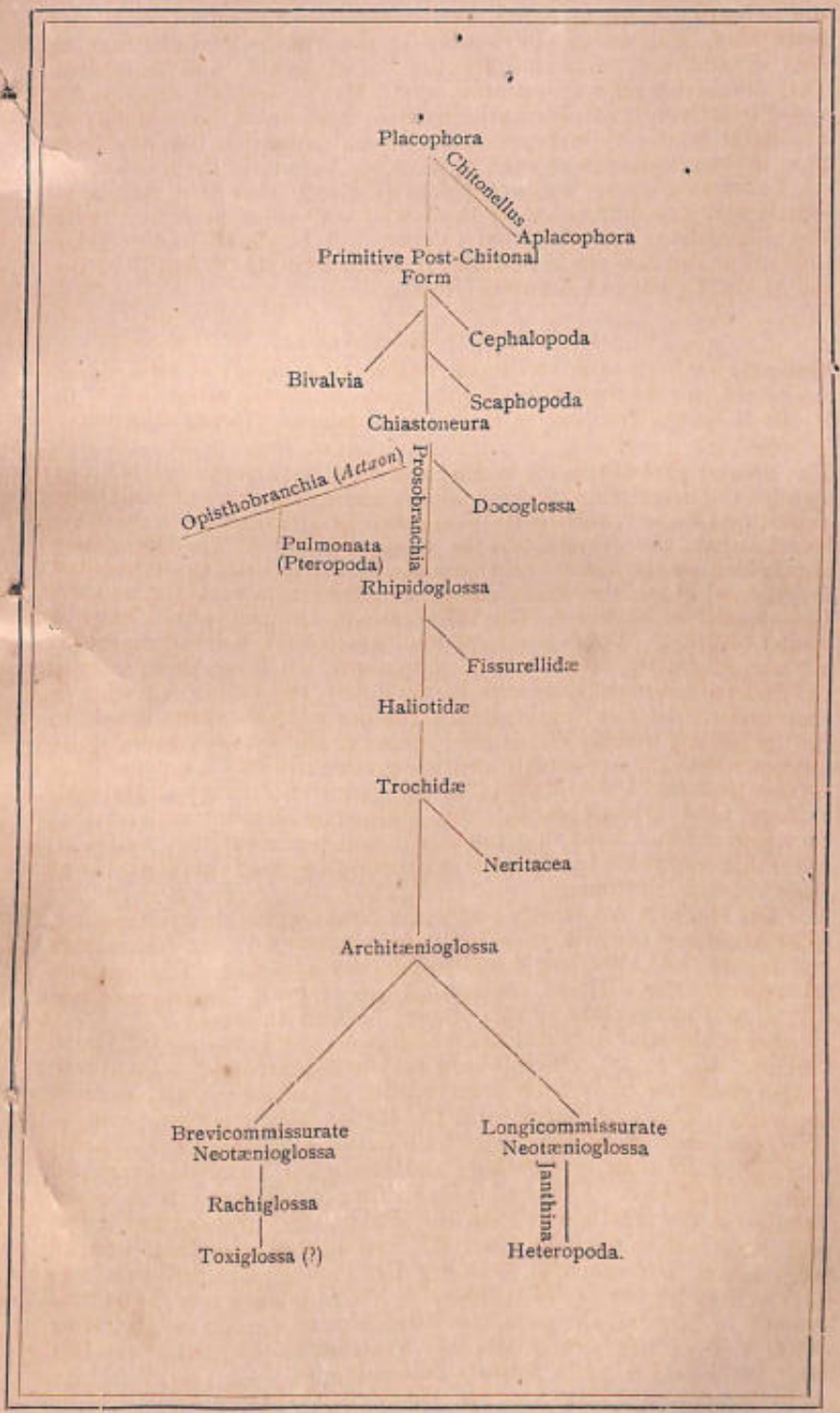
Limpets and their Allies.

"Studien über Docoglosse und Rhipidoglosse Prosobranchier nebst bemerkungen über die phyletischen Beziehungender Mollusken untereinander. By Dr. B. Haller. 4to. Pp. iv., 173; 12 plates, coloured. Leipzig: Engelmann, 1894.

The greater part of this interesting and most important work is taken up with the description of the anatomy and morphology of the Docoglossa, or Limpets, and the Rhipidoglossa, or group to which the Key-hole-Limpets, the Ormers, and the Topshells belong. The Docoglossa are divided by the author into Bibranchia, comprising the two-gilled *Propilidium*, Monobranchia, or those with the single neck gill, and the Cyclobranchia, or those, like the common Limpet, which have a circlet of gills. Where the Lepetidæ, which have neither the neck-gill nor gill-circlet, come in is not apparent. Of these three groups, the first named is considered to be the oldest, the others following in their order. Of the Rhipidoglossa, the genus *Pleurotomaria* is held to be the oldest, though Dr. Haller speaks of its anatomy being quite unknown, having apparently entirely overlooked Dall's account of it in the Bulletin of the Museum of Comparative Zoology at Harvard College, xviii. (1889), pp. 397–403, a circumstance not altogether to be wondered at considering the way in which the eminent American naturalist buries his best results in papers whose titles give no clue to their valuable contents.

Dr. Haller's last twenty pages are, however, the most interesting from a general point of view, containing as they do his conclusions concerning the phylogenetic relations of the various members of the Molluscan stem. These conclusions are of great importance, and will be best made clear by reproducing his own diagram.

On contrasting this with the tree drawn up by Pelseneer (Natural Science, iii., p. 37), which is now very widely accepted, certain prominent differences, apart from mode of drawing up, become apparent. Thus, to begin with, Dr. Haller takes the Chitons as his base, and derives all the others from it, including the Cephalopoda, although both their embryology and geological history are against such a supposition. Other equally startling conclusions, such as the classing of the Pteropoda with the Pulmonata, the sea-butterflies with land-snails, may be observed, and further comments can be safely left to the reader. Whether Dr. Haller's tree will come unscathed through the fire of criticism is doubtful, but every thoughtful attempt to unravel the genetic relationships of a group is of extreme value, and we are proportionately grateful to the author for this latest contribution to Molluscan literature.


Placophora
Chitonellus
Aplacophora
Primitive Post-Chitonal Form
Cephalopoda
Bivalvia
Scaphopoda
Chiastoneura
Opisthobranchia (Actæon)
Prosobranchia
Docoglossa
Pulmonata (Pteropoda)
Rhipidoglossa
Fissurellidæ
Haliotidæ
Trochidæ
Neritacea
Architænioglossa
Brevicommissurate Neotænioglossa
Longicommissurate Neotænioglossa
Janthina
Rachiglossa
Toxiglossa (?)
Heteropoda.

The Salmon of the Elbe.

Der Elbelachs: eine biologisch-anatomische Studie. By Professor Dr. Anton Fritsch. 8vo. Pp. 116, with 84 figs. and 1 coloured plate. Prague: F. Rivnac, 1894.

In the midst of numerous other occupations during the past twenty-five years, Dr. Anton Fritsch has been engaged in collecting information concerning the salmon and salmon-fisheries of the Elbe. So long ago as 1871 he published some preliminary observations on the subject, followed later by other notes; and now he has done good service by summarising the results of his long-continued research in the little profusely illustrated volume before us. The first part of the work is devoted to a sketch of the life and migrations of the salmon; the second and third parts deal respectively with economics and the anatomy of the fish; and a few remarks are appended in reference to the future of the salmon-fishery of the Elbe. The volume is issued with the aid of a grant from the Bohemian Parliament, and will prove of great value to those concerned in the industry of which it treats.

The account of the salmon fisheries, as might be expected, occupies the greater part of the work; but the chapter on anatomy is by no means meagre, and the descriptive details are illustrated with numerous drawings by the author. There are also sketches of external features which vary at different periods, and a few illustrations of the principal parasites known to infest the fish.

The Organisms of Ponds and Rock Pools.

Ponds and Rock Pools: with Hints on Collecting for, and the Management of the Micro-aquarium. By Henry Scherren. Small 8vo. Pp. 208, with numerous woodcuts. London: Religious Tract Society, 1894. Price 2s. 6d.

This attractive little volume comprises a series of articles recently contributed by Mr. Scherren to the *Leisure Hour*. It is adapted as well for the general reader as for the beginner, and differs from most works of its kind in being much more than a second-hand compilation. The author is an enthusiast in his subject, and has himself verified most of the facts he records; while all the hints as to collecting and the management of the aquarium are the result of wide personal experience. It is, in short, a noteworthy book, not only to be recommended as a guide to the amateur, but one likely to infuse enthusiasm in a youth who has previously had little opportunity of looking into the subject of which it treats.

Mr. Scherren's hints on "pond and rock-pool hunting" are, as we have said, thoroughly practical. He writes not only for the wealthy pedant, who can afford elaborate apparatus, but remembers also the humble school-boy who is obliged to make his own simple tackle. The introductory chapter on these matters occupies over thirty pages, and has nine illustrations. The second chapter deals with "the beginnings of life," and emphasises more clearly than usual the difference between unicellular and multicelluar animals. The remarks on classification are also surprisingly up-to-date for a popular work. There are good figures of the *Amœba*, *Vorticella*, and other typical Protozoa. Chapter III. treats of sponges, hydrozoa, etc., and is illustrated by several original figures, chiefly of marine

animals. The common *Hydra*, as usual, merits a full share of attention, and we are pleased to observe some reference to the correction of the commonly-accepted errors resulting from Trembley's experiments on this animal. The worms follow, and are described in the words of Huxley as a "heterogeneous mob." Rotifers, of course, are treated at considerable length, and there are good figures—some original, some after Hudson. The Polyzoa are also very well described. Chapter V. relates to the larvæ and more minute forms of starfishes, arthropods, and molluscs; but this would bear much expansion and further illustration with advantage.

The concluding chapter on the micro-aquarium will be found as useful as that on collecting, and interest is added to it by a reproduction of the original engraving of Trembley's study, showing the glass jars in which the well-known experiments on *Hydra* were made. A view of the author's arrangements forms the frontispiece. A good index is appended.

A History of the World.

Histoire du Monde: son Evolution et sa Civilisation. By Etienne Guyard. 8vo. Pp. 700, with Schrader's Planisphere. Paris: published by Author, 5, Impasse Nicole, 1894. Price 7 fr.

This is a somewhat strange work by a former professor in the Imperial Military Academy of Japan. It is the first volume of a projected series of five, intended to be a popular account of the astronomical and geological history of the globe itself, an outline of the evolution of its animals and plants, and a general synopsis of the history of man from primæval times to the conditions of modern political and social life. The volume before us treats of astronomy, botany, zoology, geology, anthropology, and the history of the progress of organic evolution, and bears evidence of having been most laboriously compiled from the best modern sources. So far as we have been able to test them, the statements are generally clear and reliable; while to those who desire only a superficial knowledge of the multifarious subjects treated, and do not object to a somewhat bald, encyclopædic style, the work will doubtless prove itself very useful. The great inconvenience in the book, in our opinion, is, that the author has adopted the Oriental custom of placing the title page at what we term the end, while the pagination is exactly the reverse of the ordinary. Moreover, there is a great dearth of illustrations. The appended maps, however, are very good, and the bulk of the volume would doubtless be too much increased if every section were illustrated in the manner customary in special treatises.

Plant Physiology.

Experimental Plant Physiology. By Dr. Walter Oels. Translated and edited by D. T. Macdougal, University of Minnesota. 8vo. Pp. 86, with 77 illustrations. Minnesota: Morris & Wilson, 1894.

This English edition of Dr. Oel's admirable little book will prove a useful guide to a series of elementary experiments in illustration of the most important principles of plant-life. The many clear and excellent illustrations form a valuable supplement to the descriptions. The contents are arranged under the following headings: derivation of nutriment from soil and water; transpiration; photosynthesis (a term suggested by Professor Conway Macmillan to express

the decomposition of carbon dioxide and water by chlorophyll in the presence of sunlight); respiration and metabolism; geotropism; heliotropism; warmth; growth; movement; relation of plants to animals. The last-named is treated in a very perfunctory manner, only three simple experiments being suggested, whereas it is a subject which offers an almost infinite scope, and is, moreover, of the highest interest. The book is nicely printed, though misprints are not rare, and sentences are occasionally a little involved. We think, too, it is unnecessary to speak of sodium chloride as cooking-salt in connection with plant nutrition. The apparatus required for the various experiments is fairly simple and will be found in any moderately well-equipped high school or college laboratory. As is suggested in the introduction, good plant material is an important part of the equipment, and it is therefore essential that every laboratory undertaking the work should have ready access to a greenhouse or small botanical garden.

The Carboniferous Insects of Commentry (Allier), France.

The Coal-measures of Commentry, Central France, have in recent years, under the skilful exploration of their director and engineer, M. Henry Fayol, yielded a valuable collection of fossils illustrating the fauna and flora of the Upper Carboniferous period.

The specimens were placed in the hands of specialists; the plant remains were delivered over to MM. Renault and Zeiller, who have already published the results of their examinations, so likewise the fishes, by Dr. Emile Sauvage, and the insects were entrusted to M. Chas. Brongniart, of the Natural History Museum, Paris.

M. Brongniart has now completed his share of the work, and has embodied his results in a monograph embracing Neuroptera, Orthoptera, and Homoptera.

The work forms a volume of about 450 pages in 4to, and an atlas of 37 plates in folio.

Sixty new genera and 100 new species are described.

The work is accompanied by a detailed examination of the nervation of the living Neuroptera, Orthoptera, and Fulgoridæ.

The value of this publication, and its interest to palæontologists and entomologists tracing out ancestral forms cannot be overstated, as it gives an insight into an insect fauna rich beyond compare, and proves that those "forests primæval" were neither tenantless nor voiceless.

Our knowledge of palæozoic insects has been hitherto scanty, owing to the rarity of specimens and the fragmentary condition, for the most part, in which their fossilised remains have been found. This has all been changed by the remarkable deposit at Commentry, which has yielded ten times more specimens than all other localities in Europe and America conjoined. Not only are the specimens more abundant, but they are found in a state of entirety and preservation most advantageous for study—a fact no doubt due to the fine matrix in which they are embedded and the conditions under which they were entombed.

Mr. S. H. Scudder, the eminent American entomologist (in *Amer. Journ. Sci.*, Feb., 1894), writing on these insects and M. Brongniart's drawings, which he has recently had the opportunity of examining in Paris (facts which I can confirm from my own inspection of them two or three years ago), says: "I have had the opportunity, through

the kindness of M. Brongniart, of seeing not only a considerable part of this collection, but also the illustrations prepared by M. Brongniart himself from the choicest specimens; illustrations made with a care and exactitude which leave nothing to be desired, and which are now nearly completed after a labour of ten years, so that we may hope soon to be favoured with his final work. Leaving the cockroaches out of account, to which M. Brongniart will give his attention later, the number of these illustrations, their variety, the extraordinary character of the insects themselves, and their rare perfection, leave not the least room for doubt that when his work appears, our knowledge of palæozoic insects will have been increased three- or four-fold at a single stroke, and an entirely new point of departure for the future opened. No former contribution in this field can in any way compare with it, nor even all former contributions taken together. Besides, it will offer such a striking series of strange forms as cannot fail to awaken the attention of the least incurious. One may not enter into details, but mention may simply be made of one species, regarded by M. Brongniart as one of the forerunners of the dragon flies, in which the wings have an expanse of considerably more than two feet (or about 70 centimetres), and of which several specimens are preserved. It is a veritable giant among insects."

MARK STIRRUP.

L'ANTHROPOLOGIE. Paris: vol. v., nos. 1, 2, 3. 1894.

THERE are several papers of geological interest in these three parts of "L'Anthropologie." Emil Cartailhac describes the flint implements which have been secured from L'Herm Cave, Ariège, and Marcellin Boule has a note on the lower jaw of *Gulo luscus*, and in a similar bone of an enormous *Felis* from the same cavern. This second jaw, which measures 295 mm., is larger than any yet recorded, and M. Boule names it *F. leo*, race *spelæa*. Ed. Piette writes some notes illustrative of primitive art of the horse and reindeer periods. Good illustrations accompany the paper, one of especial interest showing an auroch's head cut into a piece of stone. Another article by Salomon Reinach treats of European primitive sculpture showing Græco-Roman influence, but this, though of extreme interest, deals more or less with historic peoples.

IN a recent paper entitled "The Mammalia of the Deep River Beds" (*Trans. Amer. Phil. Soc.*, vol. xvii.), Professor Scott makes some valuable additions to our knowledge of the American Equidæ, and describes some forms of which only brief notices had previously been given. Of these, Desmathippus is notable since it fills almost the only important gap left in the phylogenetic history of the horse, coming midway between Miohippus and Protohippus. The teeth are short-crowned with the valleys partly filled by cement; the feet possess fairly well-developed lateral digits. Another interesting form is a true Anchitherium, *A. equinum*, nearly allied to the European *A. aurelianense*, but in some respects more horse-like. The author shows that these are good reasons for regarding Anchitherium as a side branch of the equine phylum which has left no descendants, and considers that the genus must have arisen in America, probably from Miohippus. *A. equinum* occurs in the upper division of Deep River Beds which is taken by the author as approximately equivalent in age

to the Miocene of Sansan, a somewhat lower horizon than that to which it has previously been referred.

The paper also includes descriptions of the Carnivora, Rodentia, and Artiodactyla from the same deposits.

We have received a "Geological Sketch Map of Western Australia, 1894, by Harry Page Woodward, Government Geologist, Perth." The Recent and Tertiary rocks form a fringe of varying width along nearly the whole of the coast, and are somewhat extensively developed in the southern portion, or Eúcla division. The Mesozoic rocks, bounded on the west by the Recent and Tertiary, and on the east by the Metamorphic and Palæozoic rocks, extend from near Gingin, north of Perth, northwards to Cape North-West. The Palæozoic rocks (Carboniferous and Devonian) are largely developed; they have an enormous extent in the neighbourhood of the Gascoyne, Henry, Ashburton, and Fortescue rivers, and also in the Kimberley division; they appear also on the Irwin river and to the north of Albany. The Metamorphic and Igneous rocks are shown to occupy a much greater area than even the Palæozoic; they stretch through the south-west and Gascoyne divisions as a broad band, nearly parallel to the western coast, and occur also in the north-west and Kimberley divisions. The various gold-fields are indicated, as well as the districts in which copper, lead, tin, and coal have respectively been found. This map should prove of the greatest value to all who are interested in the geology of Western Australia.

We may add that three other works on Western Australia are also reviewed in the September number of the *Geological Magazine*. It is rapidly taking a foremost place among the Australian colonies, and in extent of territory it greatly exceeds the others. A sum of £71,482 has lately been voted for public works and buildings, including £2,000 for the building of a museum in Perth.

In the first part of the new *Proceedings of the Victoria Institute of Trinidad*, Mr. R. J. L. Guppy gives an account of the edible molluscs of Trinidad. Two or three kinds of oysters are eaten by Europeans, but among the lower orders they are in little favour. *Asaphis deflorata*, when well cooked, Mr. Guppy pronounces most delicious; *Mytilus brasiliensis* and *Venus granulata* are common in the markets. The North American clam is a *Venus*, but is a very different species, and is absent in Trinidad. *Donax*, and that large bivalve *Pinna*, are commonly used, but Mr. Guppy has no personal experience of them. Nearly all the fresh-water molluscs are eaten. The land molluscs are less in request, although the large *Bulimus oblongus* is said by French writers to have flesh which, though somewhat leathery, possesses a delightful aromatic flavour. What Mr. Guppy believes to be the kitchen middens of the aboriginal inhabitants show that a larger number of molluscs were consumed by them.

Mr. R. J. Lechmere Guppy sends us a reprint of his paper on the Microzoa of the Tertiary and other rocks of Trinidad and the West Indies. The paper appeared in the *Journal of the Trinidad Field Naturalists' Club*. It sums up the results of former publications by the

same author, and adds many new observations. It is accompanied by a table showing the distribution of foraminifera in the "Cretaceo-Tertiary" rocks of Trinidad.

We congratulate the botanical seminar of Nebraska University on the work achieved in connection with the flora of the State during last year, as indicated in the Report for 1893. The additions number 182, and bring the total number of species reported up to 2,820. Mr. Clements describes several new species of Fungi, and new hosts are recorded for fifteen others. It would be well if some separation of the great groups were indicated; as it stands, Algæ, Fungi, Mosses, and flowering plants run on in one unbroken series. But the great fault lies in the unsparing use of a trinomial nomenclature; if a plant is a variety let it be so stated—the practice of stringing names together only leads us back to pre-Linnean times. The pamphlet also contains a revision of the nomenclature of the Nebraska Polypetalæ, according to the latest ideas; *Bursa bursa-pastoris* does, however, call forth a protest from the author, Mr. P. A. Rydberg. Mr. F. E. Clements supplies a list of botanical expeditions in Nebraska from 1803 up to date, and Mr. R. Pound a bibliography of the flora of the State.

The Orchid Review (August) suggests that in the hybrid Disas, of which we have seen and heard so much lately, we may get a race of easily cultivated common greenhouse orchids. They grow like weeds, only requiring suitable compost, plenty of water, a little shade, and protection from frost. They will certainly be a beautiful addition to the list of plants within the reach of the amateur. The great centre for Disas is at the Cape, whence they run up into tropical Africa, occurring as far north as the mountains of Abyssinia, and also in Madagascar.

Die Naturlichen Pflanzenfamilien, under Professor Engler's direction, progresses slowly. The last issue, a double one, containing nos. 106 and 107, completes section 6A of part iii., and includes the remainder of Cactaceæ, by K. Schumann, and Geissolomaceæ, Penæaceæ, Oliniaceæ, Thymelæaceæ, and Alæagnaceæ, by E. Gilg. Also a portion of Borraginaceæ, by M. Gürke, belonging to part iv.

A *résumé* of the Geology of Spitzbergen is being published in the *Feuille des jeunes Naturalistes*, by M. Gustave Dollfus. The writer gives a geological map after Nathorst, and reviews the previous writings on the geology of the island. The Tertiary, Jurassic, Triassic, Permo-Carboniferous, Devonian, Silurian (?), and Archæan rocks are represented, and various volcanic rocks have been recorded.

In the *Feuille des jeunes Naturalistes* for September, M. G. de Rocquigny has an interesting note on the pairing of butterflies of different genera. The forms noted were a male of *Satyrus janira*, and a female of *Vanessa urtica*. During the flight *S. janira* was carried by *V. urtica* and hung lifeless (*inerte*). The observation was made at Allier, on June 23, 9.45 a.m.

We have received from R. Friedlaender & Sohn, of Carlstrasse, Berlin, nos. 9 to 13 of their fortnightly publication. It professes to give a list of all new books and of the numbers of periodicals as they are issued which deal with Natural Science. It seems to us a publication to be commended to all scientific persons, and may be had post free for the modest annual subscription of four shillings.

Yet one more local scientific publication. *Madagascaria* is a monthly, dealing with the zoology, geology, botany, and anthropology of Madagascar, which Mr. F. Sikora intends to produce, starting on October 1, at Antananarivo, a place that as yet has not got beyond its well-known *Annual*. Yearly subscriptions of six shillings and fourpence (eight francs) may be sent to the Comptoir National d'Escompte in Paris, addressed, "Pour Mr. Sikora à Antananarivo."

OBITUARY.

FRANÇOIS CLÉMENT MAILLOT.

BORN 1804. DIED 1894.

SO commonplace is now the employment of quinine in the treatment of malarial fevers that the very names of those who first advocated its use are already omitted from our text-books, and in danger of passing from our memories.

The death of Dr. Maillot, at the advanced age of 91, recalls the services he rendered sixty years ago, not only to medicine, but to the world at large. Educated at Metz, he entered the career of military medicine, and in 1834 he was stationed at Bona, in Algeria, then newly colonised by France. The malarious climate was producing an enormous mortality among the new-comers, and it seemed an open question whether it would be possible to retain the colony. Maillot, who had already had some experience of malaria in Corsica, at once devoted himself to the study of the intermittent fevers of the district and their treatment by large doses of quinine, and with such success that the mortality from malaria is said to have fallen from 25 to 5 per cent. The actual discovery of the value of the cinchona alkaloids in the treatment of ague was, of course, long before this; but it may at least be claimed for Maillot that his advocacy of the employment of quinine not only popularised the remedy and saved thousands of lives, but demonstrated also the possibility of European colonisation of a highly malarious country.

A tardy recognition of his services to military medicine was made in 1888, when a pension of 6,000 fr. was conferred upon him; but it was not till last year that his numerous writings, dating from 1834 to 1887, and all devoted to this one subject, were collected and published in connected form.

REV. W. M. HIND, LL.D.

BORN 1815. DIED 1894.

WE regret to announce the death of the Rev. W. M. Hind, LL.D., Rector of Honington, Suffolk, at the ripe age of 79. For a long period he had devoted the greater part of his spare time to botanical field work, and was possessed of a wide and accurate knowledge of our British wild plants, many of the rarer forms of which he had cultivated and observed. Born in Belfast of English

parents, and educated at the Academy, he passed to Trinity College, Dublin, where he graduated B.A. 1839, M.A., 1842. In 1876 he edited an enlarged and revised edition of J. Cosmo Melvill's "Flora of Harrow," towards the original edition of which he had very largely contributed when a curate at Harrow. In 1875 he accepted the Chancellor's living of Honington, Suffolk, and immediately set to work to compile a flora of Suffolk, which he published in 1889. Having collected a large Herbarium of Suffolk plants, they were presented to the Ipswich and East Suffolk Museum. In 1870 he presented a very large and almost complete collection of British plants to the Trinity College, Dublin, Museum, and as an acknowledgment of the gift, was presented with the honorary degrees of LL.B., LL.D. by a special grace of the Senate. Dr. Hind was a man of high intellectual attainments and wide and varied reading. Essentially of a scientific mind, and an ardent and enthusiastic worker in his own subject, his wide botanical knowledge was always at the service of others.

The death is announced of Admiral Sir Edward Augustus Inglefield, K.C.B., who commanded three Arctic Expeditions, the first in 1852 in search of Sir John Franklin, and added much to our knowledge of the geography of the Arctic Regions. He had reached the advanced age of 74 years.

We have also to record the deaths of Pierre Louis Jouy, who had studied the ornithology of Japan, Korea, S. Arizona and Mexico; Dr. Erich Haase, Director of the Royal Siamese Museum in Bangkok, and a special student of the Arthropoda; Dr. Adolph Hannover, of Copenhagen, a well-known Anatomist and Histologist; Professor Michele Lessona, President of the Academy of Sciences and Director of the Zoological Museum in Turin; the nonagenarian, Dr. Louis de Coulon, one of the founders and, for most of its existence, the President of the Society of the Natural Sciences in Neufchatel.

The death of Professor Hermann von Helmholtz, on September 9, deprives Physiological Science of one of its most brilliant investigators. We hope to give some account of his work next month.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

DR. J. PLAYFAIR MCMURRICH has resigned his position in the University of Cincinnati, to accept the Professorship of Anatomy in the University of Michigan, at Ann Arbor.

SEVERAL botanical appointments have recently been made. Mr. T. H. Kearney, jun., succeeds the late Dr. Moring as Curator of the Herbarium of Columbia College, New York. Dr. W. Scott has been appointed Director of Forests and of the Botanical Garden in Mauritius. Dr. A. Zimmermann becomes Extraordinary Professor of Botany in the University of Tübingen; and Dr. Solereder is now a Curator at the Munich Botanical Institute.

WE regret to learn of the retirement of Dr. E. P. Ramsay from the Directorship of the Australian Museum, Sydney. Dr. Ramsay has been in failing heath for some time, and the duties of his office have fallen upon Mr. Robert Etheridge, jun., who has now been chosen as successor. Mr. Etheridge entered the Australian Museum as Palæontologist in 1887 on leaving the British Museum, and will assume his new office on January 1, 1895.

AT the Geological Congress at Zurich it was decided, upon the instigation of Captain Marshall Hall, supported by Professor F. A. Forel, to appoint an International Committee for furtherance and record of observations upon existing glaciers in different parts of the world. We will give further information so soon as the committee is constituted.

THE Superintendent of the Indian Museum, Calcutta (Surgeon-Captain A. Alcock), has issued an admirable "Guide to the Zoological Collections exhibited in the Invertebrate Gallery of the Indian Museum." It is a little volume of 155 pages, and forms a most concise introduction to Invertebrate Zoology.

THE Bristol Museum is closed for internal and external decoration, and is now being put into a thorough state of repair. Wires have been introduced for electric lighting, the dilapidated cases have been repaired, and the long-neglected collections will thus soon be again safely housed. The advent of municipal control has effected all these improvements, and it would be well if a few more of our languishing provincial museums could be similarly transferred to their respective towns. Mr. Edward Wilson, the energetic Curator, has just issued a new edition of his popular penny Guide, which continues to have a large sale.

M. STANISLAS MEUNIER has inaugurated an annual exhibition of geological specimens at the Museum of the Jardin des Plantes, Paris, for the purpose of assisting geologists to follow and verify the work of the year. The exhibition is to be

not merely national, but to contain contributions from the various foreign Geological Surveys. To illustrate the past year the Geological Survey of Great Britain has lent specimens of the Archæan and pre-Cambrian rocks of North-west Scotland, geological maps in course of preparation, and a collection of new geological works and photographs. Signor Pellati, the Director of the Geological Commission at Rome, has sent specimens illustrative of recent Italian researches. The United States and Queensland Surveys have sent copies of their publications; and the Imperial University of St. Petersburg has lent a variety of specimens of platiniferous rocks, the discovery of which is of great importance. A small guide-book of 24 pages is issued in connection with the exhibition.

Important changes are taking place in the Palæontological Department of the U.S. National Museum at Washington. The Honorary Curator of this Department is Mr. Walcott, the new head of the Geological Survey. Till last July there were five honorary curators, who, though they advanced the collections, had little time to spare for purely curatorial work. Now, however, Mr. Charles Schuchert has been appointed Assistant Curator, with a salary, and is able to devote his time to practical improvements which will doubtless bring the exhibition series of this Department to the high level attained in the rest of the Museum. For the present, exhibition will be limited to a good representation of the North American fossil faunas geologically arranged, but not as yet, owing to want of space, divided into geographical provinces. The invertebrates will be displayed in the table-cases, the plants and vertebrates in the wall-cases. As soon as possible Mr. Schuchert will begin a synoptic collection illustrative of all fossil genera, in which each class will begin with a series explaining the terminology, after the fashion of the Crinoids in the Geological Department of the British Museum.

In addition to much information that has already appeared in our pages, the *Annual Report of the Delegates of the* [Oxford] *University Museum* for 1893 contains the following items of interest. In the Ethnographical Department of the Pitt-Rivers Museum series have been arranged which illustrate Magic, the early development of Writing, Lighting, Smoking, Treatment of the Dead, and Primitive Pottery. As evidence of original research conducted in his department, the Professor of Physiology quotes a list of six papers by five authors. Similarly, the Professor of Comparative Anatomy enumerates three ladies and six gentlemen who have used his senior laboratory for original investigations. The Professor of Zoology states that Dr. F. A. Dixey has studied the Pieridæ in the Hope Collections, in the preparation of an important memoir upon the evolution of the wing-markings in this group of Lepidoptera; the specimens have at the same time been arranged in a single series. Colonel Swinhoe has continued his Catalogue of Oriental Heterocera in the Hope Collections, and the second volume, containing the Pyrales and the Noctuæ, will soon be issued. The Hope Professor is breaking the tenth commandment in respect to the Mathematical Professors, whose room is better than their company. The delegates concur. The fittings of the new geological laboratory are all but complete, and advanced teaching can now be carried on in comfort, and with greater thoroughness than heretofore. A rock collection for teaching purposes is being systematically arranged and illustrated by explanatory labels. We fully agree with the Professor of Geology when he says, "Re-arrangement of the fossils is urgently needed, and it is hoped that it may be begun shortly. It will be a long and heavy piece of work, and skilled assistance will be absolutely necessary to carry it out successfully." The Professor of Mineralogy is taking in hand the re-arrangement of the mineral collection; this, too, not before it was wanted.

By the provisions of the will of the late Dr. William Johnson Walker, two prizes are annually offered by the Boston Society of Natural History for the best

memoirs written in the English language on subjects proposed by a committee appointed by the Council.

For the best memoir presented a prize of sixty dollars may be awarded; if, however, the memoir be one of marked merit, the amount may be increased to one hundred dollars at the discretion of the committee. For the next best memoir, a prize not exceeding fifty dollars may be awarded. Prizes will not be awarded unless the memoirs presented are of adequate merit. The competition for these prizes is not restricted, but open to all.

Each memoir must be accompanied by a sealed envelope enclosing the author's name and superscribed with a motto corresponding to one borne by the manuscript, and must be in the hands of the Secretary on or before April 1 of the year for which the prize is offered.

The Subjects for 1895 are:—

(1) A study of the "Fall line" in New Jersey.
(2) A study of the Devonian formation of the Ohio basin.
(3) Relations of the order Plantaginaceæ.
(4) Experimental investigations in morphology or embryology.

The Subjects for 1896 are:

(1) A study of the area of schistose or foliated rocks in the eastern United States.
(2) A study of the development of river valleys in some considerable area or folded or faulted Appalachian structure in Pennsylvania, Virginia, or Tennessee.
(3) An experimental study of the effects of close-fertilisation in the case of some plant of short cycle.
(4) Contributions to our knowledge of the general morphology or the general physiology of any animal except man.

In all cases the memoirs are to be based on a considerable body of original work, as well as on a general review of the literature of the subject.

CORRESPONDENCE.

Plateau Man at the British Association.

In reading over the report of the British Association discussion on Professor Rupert Jones's paper in the last number of Natural Science,[1] I am struck with a series of curious coincidences, and as I consider they are calculated to propagate a very wrong impression, I venture to ask to be allowed to point them out. I do not for a moment suppose it was intentionally done, especially in the face of the opening sentences, still it is very strange that almost every adverse criticism raised by the speakers is given, while nearly all arguments for, and admission of, the antiquity of plateau man are overlooked, and a summing-up given which may not always be taken in the way which a further examination of the facts of the case would demand. This is all the more to be regretted, as every supposed argument urged against plateau man rises on the speakers not being in possession of all the facts of the case. If it were simply an instance of various interpretations being put upon well-known phenomena of the origin of which we know but little, differences of opinion are all that could be expected; but in this subject—as indeed is elsewhere suggested in reference to all others—the value of one's opinion is not to be estimated alone by qualifications in other branches of science, however nearly related they may be to the question at issue, but by the amount of careful, extensive, and prolonged research in and out of the field he has given to it, qualified by his ability to observe. I am very much tempted to answer the criticisms raised at the British Association meetings one by one, but for my confidence in the effect of the publication of the results of recent labours. Those who are in possession of the facts of the subject have but one opinion; those who differ from us will alter theirs as knowledge accumulates. Truth is doubtless as precious to them as to ourselves, although some people are apt at times to forget that change of opinion is an honest man's characteristic. I thus hasten to a further examination of the discussion. Now I think the way this can best be done is to go through the remarks of each speaker, in so far as they bear upon the point at issue. The first speaker was Mr. Whitaker, in whose speech I find the following:—"This certainly carries man back, locally at all events, beyond the time of the river gravels, which occur in the bottoms and along the slopes of the valleys." He appears (*Times* report) to have been followed by Mr. Montgomerie Bell, who said that, in his opinion, "they (the plateau relics) belonged to pre-Glacial or Pliocene times." The next speaker was Sir John Evans, who admitted "that the principal outcome of recent discoveries was, to his mind, the fact that the existence of Palæolithic man could be carried further back in time than the valley gravels, inasmuch as flints are found in gravels on the plateaux at far higher levels." Sir John was followed by Dr. Hicks—who has had far too much experience in this subject to deny the pre-Glacial existence of man in Britain,—and he pointed out the great concession that had been made by Sir John Evans, and also how the same had been wrung out of the veteran Professor Prestwich by the repeated discoveries of man's work in pre-Boulder Clay deposits. Then followed Professor Boyd Dawkins, who made some sweeping assertions, which, like the adverse criticisms of other speakers, can be flatly challenged; but so far as the question at issue is concerned, he said he agreed with what Mr. Whitaker and Sir John Evans had said, and made no exception to the extension of time they had granted. General Pitt

[1] Reprinted in full in the present number, pp. 269-275.—Ed.

Rivers, from another standpoint, argued the *necessity* of pre-Palæolithic man. Sir Henry Howorth gave it as his opinion that the only evidence available for testing the relative age of the plateaux gravels goes to show that they are older than the distribution of the so-called Glacial Drift. With reference to Mr. Clement Reid's remarks, I am pleased to say he is not informed up-to-date; there are numerous localities now in various parts of the kingdom which have produced these relics of greater antiquity. Among the latest I might mention the work of Dr. Blackmore in his own neighbourhood, and the yields of the hill-drifts in Essex; the Boulder Clay of Finchley, and of deposits older than these in Middlesex, Essex, Suffolk, and Norfolk. The theory suggested by the concluding speaker, variously reported as Lt.-Col. Godwin and Col. Godwin Austin, calls for the existence of huge glaciers in the Weald, which would certainly take us back very much further in time than many anthropologists have been in the habit of admitting for man.

I cannot help thinking that a perusal of these facts will better emphasise your opening remarks and give a truer idea of the state of scientific opinion of to-day upon this point, and show that the verdict of the discussion is that opinions are divided, but that the affirmatives are in excess of the negatives; this in proportion to the greater the knowledge of the subject possessed by the respective speakers. It must also be admitted that it was quite lamentable to observe upon how little practical knowledge of the deposits the adverse criticisms were based; one speaker, at least, still confounding them with "the Clay-with-flints."

W. J. Lewis Abbott.

The Museums' Association.

The note headed "Science at a Picnic," in your issue of August, is so entirely misleading, so far as it regards the Museums' Association, that we ask permission to offer some corrections, especially as the note has evidently been written by some one not present throughout the meetings. The meeting commenced on Tuesday, June 26, with visits to various scientific institutions, lasting four hours, and finishing with the President's address at night, the whole time occupied being five and a half hours. On Wednesday three and a half hours were devoted to papers and discussions, and another three hours to the inspection and discussion of the arrangements and contents of various museums. On Friday another three and a half hours were given to papers and discussions, followed by visits to museums afterwards. And yet the writer of the note speaks of five hours devoted to business, and talks of papers being burked! All the papers in the programme were submitted to the meeting, except one by a member who, although invited two months beforehand to give notice of his paper, sent no intimation of it till the programme was printed, and then appeared with some notes which he wished to descant upon, and was allowed to do so as long as time permitted. Exception may possibly be taken to a day's excursion finding a place in the programme of the meeting; but as curators have few facilities of exchanging ideas one with the other, generally to their mutual advantage, the value of such an arrangement will, we think, be quite evident to most people, and we believe that "shop" was the predominant subject of conversation, even on that day. The Local Committee in Dublin, in fact, showed the truest spirit of hospitality in affording to the Association every opportunity of prosecuting its work in the direction of acquiring knowledge of the excellent museum work that is carried on in that city, and if the writer of the note thinks that no good can accrue from such meetings, except what is derived from the reading and discussion of papers, he will find few people to concur with him in such a narrow and baneful view. It may interest him to know that the volume of proceedings of the Dublin meeting will be the largest that has ever been published by the Association, and the Local Committee are finding the funds for providing illustrations to some of the papers that were read.

H. M. Platnauer,
E. Howarth,
Secretaries Museums' Association.

Public Museum, Sheffield.

[We have much pleasure in printing this letter from the energetic secretaries of the Museums' Association. This body does, as we have often pointed out, such very valuable work, that we should greatly regret to injure it by any misstatements. We do not, however, gather that any of our statements are seriously traversed. We stated, correctly, that the time allotted to papers was five hours; we are now informed that seven hours were actually conceded, but this apparently includes the time spent on the formal business of the Association, which we did not reckon. We stated that "considerable complaint was heard at the way in which both papers and discussion were burkrd"; we are now informed that all the papers in the programme were submitted to the meeting; this is quite true, but how many were cut short? how often was really useful discussion checked? and were the complaints we heard from numerous people present throughout the meeting entirely imaginary? We stated that "the insertion of a day's excursion between the two days of meeting proved vexatious to those who could ill spare their time"; we are informed that those who had time to spare talked "shop"; but no attempt is made to contest our statement. We fully agree that inspection of museums is and was a valuable part of the week's work; but waterfalls are not museums. Our only object was to utter a timely warning, so that the large amount of active and useful work hitherto accomplished by this Association at each of its annual gatherings might not be diminished by any misguided generosity.—Ed.]

Dr. J. M. Clarke writes to us with reference to the review of Hall and Clarke's "Handbook of the Brachiopoda," which appeared in our August number (p. 140), saying that the edition which fell into the hands of our reviewer "is purely incidental and accessory to the regular one. The work is published as pages 134–300 of the Eleventh Annual Report of the State Geologist, for the year 1891 (dated 1892, published 1894), and contains 22 lithographic plates." These plates were not seen by our reviewer.

We have received the following note from Professor F. A. Lucas, of Washington:—I was glad to see a note on Mr. Lydekker's new cetacean in Natural Science (August, p. 85). I have not the least doubt, from my acquaintance with whales, that what Mr. Lydekker saw was a humpback (*Megaptera*) lying at the surface and thrashing his fins about, which is a very characteristic trick of the genus.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan and Co., 29 *Bedford Street, Strand, London, W.C.*

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NOTES AND COMMENTS.

Change of Address.

We beg to draw the attention of our readers to the notice that appears under the above heading on the last page of the present number. It is hoped that the new arrangement, which will come in force with the new year, will have the effect of facilitating our own business and our dealings with the public by concentrating the Editorial, Printing, and Publishing Offices at one address, instead of distributing them among three as has hitherto been the case. It was well at first to place ourselves in the kindly hands of so well-known a firm as Messrs. Macmillan, and we should like to take this opportunity of thanking them for the assistance and courtesy that they have extended to us. But now that we are growing up and begin to feel our legs, we are bold enough to think that we can walk alone. Whether we can succeed or not must of course depend not on ourselves but on the number of hands stretched out to help us. Since the Review will still be conducted by the same editorial staff and on the same lines as heretofore, we appeal confidently to our readers, subscribers, and contributors not to desert us; indeed, we venture to hope that they will now aid us all the more both in purse and person. If only their sympathy be assured, we shall step from the cradle with a light heart.

Marriage Customs of the Octopus.

In a recent number of the *Archives de Zoologie Expérimentale*, Mr. Emile G. Racovitza relates in a most interesting manner the observations that he has made on the habits of *Octopus vulgaris*. Some previous writers, especially Dr. Kollmann, have imagined that the love affairs of the Octopods are carried on in a barbarous, not to say a brutal manner, and that the resistance of the female to the male

occasions a savage combat between the two. Mr. Racovitza, however, assures us that there is nothing more than a courteous flirtation, and that "the male behaves with a certain delicacy towards his companion." Placed at a considerable distance from the female, the male stretches out the third arm on the right, which is known as the hectocotylised arm, and with its extremity he caresses her, eventually passing it into the chamber formed by the mantle. The female contracts spasmodically, probably because the arm touches the gill, but does not attempt to move. The animals remain in this passive condition for an hour or more, but at about half-time the male shifts the end of the hectocotylised arm from the opening of one oviduct to that of the other: this gives rise to fresh contractions on the part of the female, and at the same time the male has been seen to eject a thick cloud of ink. Should the female prove at all refractory, the male stiffens one of the arms of the second pair, and raises it with a warning motion, as a schoolmaster might raise his cane. With the same gesture the Octopus, turning black with anger, threatens any intruders, and in both cases the menace is successful. Finally the male withdraws his arm from the mantle-chamber, caresses the female with it for a few moments, and then places it in the same position as his other arms.

As regards the actual mechanism by which the spermatozoa are transferred from the body of the male to the ova, Mr. Racovitza has also something new to add. It is well known that the spermatozoa of Cephalopoda are ejected in special tubes or cases called spermatophores; and it is generally stated that the male introduces the end of his arm into the mantle-chamber to take a packet of spermatophores, which he then places in the mantle-cavity of the female. This statement, however, is not based on direct observation: "It has sprung," says Mr. Racovitza, "from the simple imagination of a single author, and has been repeated by everybody without having been brought to the test of either facts or logic, just as is so often the case, especially in the natural sciences." The true facts, so far as *Octopus vulgaris* is concerned, appear to be as follow. The spermatozoa are contained in the round head of a tubular spermatophore. The spermatophore is expelled from the opening of the penis, which stretches out, through the mantle-chamber, into the funnel, which then conducts it to the base of the hectocotylised arm. Down this arm there runs a channel or gutter, lined by smooth epithelium, which is thrown into numerous minute folds. Wave-like contractions traversing the walls of the gutter force the spermatophore along this channel to the distal end of the arm, which, as already stated, is placed in the mantle-chamber of the female. The flattened end of the arm then applies the head of the spermatophore to the opening of the oviduct, in which position it comes in contact with the sea-water and bursts, so that the spermatozoa are set free into the canal of the oviduct. The walls of this canal are provided with numerous

and complicated folds, in which the spermatozoa lodge until the downward passage of the eggs allows fertilisation to take place.

Mr. Racovitza has also studied the habits of *Sepiola rondeletti* and *Rossia macrosoma* at the same interesting period of their lives, but the complete account is not yet published. (See *Comptes rendus de l'Acad. d. sci.*, cxviii., p. 72.)

The Habits of a Crab.

In another part of the paper above referred to, Mr. Racovitza has a note on the habits of *Pilumnus hirtellus*. This little crab lives in holes in the rocks at the bottom of the sea off Cape Abeille near Banyuls. In other holes of the same rocks live some small bivalves of the genus *Tellina*. So long as the molluscs remain quiet, with their shells closed, the crab takes no notice of them. But if one of them moves, then its shell scrapes the rock, and the vibration is communicated to the crab in his hole. After listening for a few seconds the crab sallies forth, and proceeds in the direction of the *Tellina*; he then feels for the hole with his claws and, inserting one of them, draws out the mollusc, which he carries back to his own den, cracks open, and eats.

These observations and a few simple experiments show that by means of its otocyst, which is an organ for feeling vibrations rather than for hearing, the crab perceives not only the presence but the whereabouts of the mollusc. This perception is retained in its memory after the vibration has ceased. The actual spot is found not by sight but by touch. The actions of the crab are purely instinctive and show no signs of intelligence; for any object that is used to scratch the surface of the rock will attract and be seized by the crab, even many times in succession, just as much as if it were a toothsome morsel.

"The Toilers of the Sea."

We have devoted some space to these observations of the Roumanian naturalist, not so much because of their intrinsic importance, though that is not slight, as because they exemplify a side of zoological study that deserves more attention and more encouragement than, in this country at all events, it has yet received. It is hardly necessary to point out that there are many structures the true meaning of which will never be apprehended by one who relies on anatomy alone, if indeed they do not escape his observation altogether. Everyone admits, in theory, the necessity for close and long-continued study of animals living in their native haunts; and yet many a zoologist obtains degrees and honours without having seen a single living individual of the animals that he has written about. It is not to be supposed that this state of things exists by the choice of the zoologists. So far as the ornithologists, lepidopterists, and such workers are concerned, the objection cannot, as a rule, be raised. It is the student of marine animals, especially of those that live in

deeper waters, who so often owes all his knowledge to the scalpel and the microtome.

It is partly with the object of remedying this state of things that marine laboratories have been built in many parts of the world, and many of these offer admirable facilities for the prosecution of a veritable biology or study of life in living organisms. But the mere existence of these laboratories is not the only thing required, and this is easily shown by the contrast between our own and other countries. To make the comparison simple, we need only draw attention to two marine laboratories, both on the shores of the English Channel and on exactly the same meridian, but one in England and one in France —namely, the establishments at Plymouth and Roscoff. At Plymouth the coast is more favourable, the fauna is richer, while the building is finer and more richly equipped than is the case at Roscoff. From the Plymouth laboratory there proceeds every year a large quantity of work of much practical value to the English fisheries; so far as these observations are concerned, we compare very favourably with our neighbours. But when we consider the amount of purely biological investigation that is carried on at these laboratories, and the numbers of zoologists that come to them, a calm examination of the facts will show a very large balance on the side of the French. The Roscoff laboratory is thronged every summer, not merely with students, but with investigators from all parts of the world, including England, while the same may be said of the corresponding laboratory at Banyuls in the south of France.

There may be various reasons for this state of things; but there are two at least which are perfectly obvious. The first is the simple question of cost. To work for a month at Plymouth the student must pay £5 for his table, he will be charged extra for all above a small allowance for material and reagents, he will have to take lodgings in the town, and, when he leaves, he will doubtless make some suitable present to the attendants. At Roscoff during the same period, the student is charged nothing for the use of the table, he is permitted as much of the various reagents, even of absolute alcohol, as any reasonable man can want, he can have a bedroom at the laboratory, and, when he leaves, he will only be expected to leave behind him ten francs for the attendant and fifteen francs for the servant that looks after his bedroom.

The second reason is that the Roscoff laboratory is not an independent establishment, but is intimately connected with the Sorbonne and managed by one of its eminent Professors, H. de Lacaze Duthiers. Consequently the pupils of the Sorbonne go there to work before they have passed their Licentiate examination, and naturally continue to work either there or at Banyuls when preparing for their doctorate. With this nucleus it is not so difficult for the authorities to open their doors in the above-described hospitable fashion to all *bonâ fide* investigators, whose presence is as great a

benefit to the students as it is to themselves, and who naturally make many observations of practical as well as scientific importance on the marine fauna of France.

It is not the first time that we have called attention to the desirability of closer co-operation between the educational and scientific establishments of our own country; and this seems to us a field in which a nearer union is not only perfectly feasible, but would prove to the great advantage of all concerned. It has long seemed to us an astonishing thing that our principal Universities do not insist on their biological students spending at least one summer term at a marine laboratory. There can hardly be any other country where the facilities are so great and where so little advantage is taken of them. The enlightened action of the French Government we have already admired; Professor Haeckel takes his students every year to Heligoland; the Americans naturally are up-to-date with their numerous stations and summer excursions for students; even in Japan, as readers of Mr. Bather's account in our February number will remember, the biological students are required to pass at least one term at the laboratory of Misaki.

Perhaps if similar arrangements were made in England it would be possible, and even as things now are we do not see that it would be impossible, for the Marine Biological Association to offer a little more encouragement to students. The man of science is rarely rich. To do scientific work at all often means the loss of much money to him. Surely such an Association should subsidise rather than tax the struggling investigator. Inspection of the balance-sheets of the Association shows that the amount of money which it yearly derives from the fees or purchases of visiting zoologists forms a very small proportion of its income. The slight pecuniary loss that might be entailed by such reforms as we venture to suggest would be more than compensated by the increased number of visitors and the consequent increase in both value and amount of the scientific and practical observations made at the Plymouth laboratory. By all means let more money flow into the coffers of the Association; but—we put the question to the public, to the universities, and to the Government of this country—is it logical to ask people to pay for the privilege of doing useful work for which they themselves receive no pay?

In the China Sea.

In the centre of the China Sea is a great bank about 80 miles long and 30 miles broad, which is known as the Macclesfield Bank. It rises rapidly out of deep water, and lies at an average depth of 40 fathoms. As it was frequently crossed by vessels, the Hydrographic Department of the Admiralty undertook a more detailed survey, a piece of work which has been accomplished by H.M.SS. "Rambler," "Penguin," and "Egeria," in the springs of 1888, 1889,

and 1893 respectively. In the course of these surveys special attention was paid to the biological conditions of the bank, in order to throw light on the general question of reef- and atoll-formation. The scientific portion of the work was fortunately superintended by Mr. P. W. Bassett-Smith, surgeon, R.N., and the three reports that he sent home have now been published together by H.M. Stationery Office, with a preface by the Hydrographer.

"The general result of the whole examination," says Captain Wharton, "may be stated to be that on the whole of the 200 miles forming the periphery of the bank there exists a rim of coral in luxurious growth, and at a remarkably even depth below the surface of from 9 to 14 fathoms, this rim being broken here and there by passages of greater depth, but less than the general depths of from 40 to 48 fathoms which prevail over the whole central body of the bank. On one spot only on the rim was a depth of as little as 6½ fathoms found, and on a patch in the centre of the lagoon, a small spot of 5 fathoms, the shoalest water on the whole bank was found. This evenness of depth is the most striking feature of the chart, and when the great distances are considered, this appears to be a strong argument against any movement of the bottom since the atoll form was assumed. It is at any rate quite evident that from the present time onwards no movement is necessary in order to form in the future a perfect atoll, the simple growth of the coral on the rim sufficing; and that we may have here an instance of a suitable original foundation for an atoll so formed, as pointed out by Mr. Darwin."

Confirmation of the above opinion will also be found in the reports of Mr. Bassett-Smith himself. Thus, he says, "there appears to me no evidence of either subsidence or elevation of this reef other than that brought about by the building up on a submarine mound of both living and dead forms of calcareous secreting organisms from the animal and vegetable kingdom." And again: "Altogether forty-one genera of corals were represented, excluding the alcyonarian and hydroid corals, viz., *Tubipora*, *Millepora*, *Stylaster*, and *Distichopora* giving a total of twenty-nine genera in depths between 25—35 fathoms, and twenty-seven in over 35 fathoms; but out of these several cannot fairly be called reef-building, as *Leptoseris*, *Psammocora*, *Anacropora*, and *Balanophyllia*, with *Cycloseris* and other simple corals. With these facts before us there can be no reason to doubt that the actual increase of solid calcareous rock from these marine organisms requires a much less narrow limit of depth than is usually assigned to them, and helps very materially to explain the great depths of these lagoons, which has been up to the present time one of the most vexed questions with regard to the formation of these atolls, and one of the strongest points in support of Darwin's theory. Together with these growing corals in 30—50 fathoms (as pointed out before) exists an enormous amount of rock formed by calcareous algæ *in situ*, and probably the coating on the surface of the living

tissue protects considerably the rock beneath from the solvent action of the carbon dioxide, which likewise is split up and used by the plants themselves. On the lagoon flat very little algæ is present."

We particularly wish to direct the attention of zoologists to these reports, for they contain a large number of interesting observations, which, from the form in which they are published, are extremely likely to escape notice. The paper appears, in fact, to have been issued in March of this year, though it has only just fallen into our hands. It is illustrated by a chart, two outline sections, and four woodcuts.

Echinoderms of the Macclesfield Bank.

As another result of these important surveys, and especially in consequence of the labours of Messrs. Bassett-Smith and J. J. Walker, a very large number of zoological specimens have found their way to the British Museum. The Echinoderms, which present several features of interest to the specialist, have just been described in a suggestive paper by Professor F. J. Bell. Very noticeable in this collection, as also in the collection of Crustacea, is the large number of young specimens. "In fact," writes Mr. Bell, "I have had forced on me the conviction that Macclesfield Bank is a nursery; with a rim submerged 9 fathoms beneath the surface, any pelagic larvæ that will can enter its boundary; being 76 miles long and 36 miles broad, it affords some opportunity for the larvæ to settle." The importance of a large number of young specimens is very great, for not only does it enable us to study the changes that occur during growth, and thus to determine more satisfactorily the relationships existing between species, but it often leads to a happy reduction in the number of species by the discovery that independent names have been applied to what after all are only immature individuals. Mr. Bell therefore wisely suggests that as full collections as possible should always be made in areas resembling the inside of this reef.

Although Holothurians are common on the shallower reefs not far distant, yet they are almost totally absent from the sand-flat of the lagoon; only two young specimens were dredged, here referred to *Colochirus tuberculosus*.

The Crinoids are represented by twenty-two species, of which by far the most remarkable are the new species *Antedon bassett-smithi* and a fragmentary *Actinometra*. The late Dr. P. H. Carpenter, who devoted many years to a study of the free-swimming Crinoids, elaborated a scheme of classification based on the mode of union of the arm-ossicles, which scheme has hitherto been found to work very well. These two specimens, however, run directly counter to nearly all Carpenter's generalisations on the subject. Mr. Bell, who has himself an excellent right to be heard on these particular points, cannot believe that the specimens are merely abnormalities,

and it is clear that his faith in the method of classification according to fixed joints or "syzygies" has been rudely shaken.

The Starfish, numbering twenty-one species, present nothing so remarkable. The drawing of the plates of a young *Culcita* is, however, of considerable interest; but the questions of homology which it raises could hardly be settled from the evidence of one ray alone, which is all that Mr. Bell has chosen to give.

Among the eighteen Ophiuroids, the most striking is one with branched arms, like an *Astrophyton*, but possessing plates on its disc which closely resemble those of the ordinary Ophiurids. This forms the type of a new genus, known by the ingeniously derived name, *Ophiocrene*. But for the meaning of this name, and for the many questions raised by this specimen, we must refer our readers to the original paper in the *Proceedings* of the Zoological Society for 1894 (pp. 392–413, and pls. xxiii.–xxvii.).

Fresh Forms of Foraminifera.

Quite a crop of papers on these organisms has recently appeared. Mr. F. Chapman continues his studies on the Gault forms in the *Journal of the R. Microscopical Society*, in dealing with the Vaginulinæ. No less than thirteen varieties are described and figured, most of which have been previously recorded by Reuss from Germany and Berthelin from France, but one (*V. priceana*) is new. Mr. E. Halkyard has a general and useful paper, on "Plans of Growth" and form in the Foraminifera (*Trans. Manchester Microsc. Soc.*, 1893), in which he collects together much scattered information, his object being to show the diversity of shape and how one form runs into another among these lowly-organised beings. The paper is illustrated by two plates which considerably assist the text. Walter Howchin continues his work on the Foraminifera of Australia, and contributes two papers. The first is "On the Occurrence of Foraminifera in the Permo-Carboniferous Rocks of Tasmania" (*Austral. Assoc.*, Sept., 1893, Adelaide), with two plates of rock-sections. He has not yet succeeded in isolating specimens, and consequently our knowledge must at present be limited to genera. His second paper is entitled "A Census of the Fossil Foraminifera of Australia," and gives a list of all forms recorded up to 1893. Two hundred and seventy-three species are listed, and these are again tabulated under their stratigraphical position. The paper, which was published in the periodical referred to above, will be of considerable value, but the publication of MS. names is to be deplored, as it can only be injurious to science.

Dr. Franzenau, of Budapest, writes on "Die Foraminiferen des ober-Mediterranen Tegels von Zsupanek" (*Természetrajzi Füzetek*, xvii., 1894), from which he records fifty-four species, three of which seem to be new, though they are but briefly described, and not figured. Mr. Chas. Schlumberger continues his excellent work on the Miliolinæ

by describing and figuring (*Bull. Soc. géol. France*, xxii., 1894, p. 293) a new *Lacazina* (*L. wichmanni*), which he has recovered from fossil deposits on the north-east of New Guinea. In a second paper, "Note sur les Foraminifères des Mers arctiques Russes" (*Bull. Soc. zool. France*, vii., 1894, p. 252), he describes and figures *Triloculina pyriformis*, *Quinqueloculina parvula*, *Sigmoilina herzensteini*, *S. macarovi*, *Reophax flexibilis*, *Lagena serrata*, accompanying his descriptions with those exquisite sections students know so well. The specimens described were obtained from Kola Bay (Kildin Island), and the Sea of Okhotch, and were dredged by Admiral Macarov during one of the recent Government expeditions to the Mourmane peninsula.

Professor Andreae has completed the work begun in 1870 by Von Schlicht and Von Reuss on the Foraminifera of the Septarien-Thon by a paper on these deposits near Frankfort a.M.; he adds notes on the vertical distribution of the Foraminifera therein contained (*Ber. Senck. nat. Ges.*, 1894). Federico Sacco has paid attention to the Miocene Tinoporinæ of Turin (*Bull. Soc. belge Géol.*, vii., 1893), and found it necessary to make three new genera, *Miogypsina*, *Baculogypsina*, and *Taurogypsina*. The first two are founded on previously known forms, but there seems some doubt in the author's mind whether *Taurogypsina* is a Foraminifer or not.

The Life-History of Some Foraminifera.

So little biological work has been attempted on this group that we hail with pleasure Mr. J. J. Lister's contribution to the above subject in the *Proceedings* of the Royal Society (vol. lvi., 1894, pp. 155–160). Mr. Lister has studied the phenomenon of dimorphism, which shows that individuals of a species fall into two groups. In one the central chamber is of considerable size (megasphere), while in the other it is small (microsphere). The two forms differ also, in the Miliolidæ for instance, in the plan on which the chambers are arranged, in the size attained by the adult shell, and in the frequency of occurrence.

The forms known as *Polystomella crispa* and *Orbitolites complanata* were examined, and permit of the following summary and conclusions:—

1. The species are in a great number of cases dimorphic.
2. The two forms differ from each other:
   (*a*) In the size of the central chambers.
   (*b*) In the shape and mode of growth of the chambers succeeding the central sphere.
   (*c*) In the character of their nuclei (size, etc.).
3. The megalospheric form of a species is much more numerous than the microspheric.
4. The megalospheric form has been seen to arise in at least seven genera as a young individual already invested by a shell, produced in

the terminal or peripheral chambers of the parent. While in some cases (*Orbitolites*) the parent of such a megalospheric young was microspheric, in others (*Peneroplis, Orbitolites*) it was megalospheric.

5. Foraminifera, in certain conditions, give rise to active swarm-spores.

The two forms may be safely concluded, says Mr. Lister, to be distinct from their origin, and his observations leave it impossible to regard the difference as sexual. Brandt has recently described a similar nuclear history in *Thalassicola*, one of the Radiolarians, and in this group the individuals of a species fall into two sets, those producing isospores and those producing anisospores, which are regarded as an asexual generation alternating with a sexual; and Brandt's observations present resemblances to the life-history of *Polystomella* as observed by Lister.

So much for the microzoa of the sea. Let us now glance at the macrozoa of the land.

The Extermination of Big Game.

In an interesting article in the October number of the *Fortnightly Review*, Mr. H. A. Bryden, whose book entitled "Gun and Camera in South Africa" is known to all students of African natural history, laments the terribly rapid rate at which the game is being exterminated. He reminds us of the vast herds that roamed over Cape Colony in the days of the earliest Dutch settlers, when lions swarmed so at Cape Town that the Governor thought one night that they "would take the fort by storm"; when antelopes ravaged the crops so that some districts could hardly be cultivated. That a little further inland this condition of affairs lasted till the present century, Mr. Bryden shows by extracts from the journals of the great hunters. Thus Dr. Andrew Smith saw not far from his waggons one hundred and fifty rhinoceroses in one day, and one hundred giraffes almost at the same time. At the present time many of the finest animals are extinct either altogether or locally, and far up the country, frequented only a few decades ago by herds of noble game, Englishmen are now reduced to hunting with foxhounds the jackal and that tiny antelope the "duiker."

Mr. Bryden gives an account of the various agencies that have led to this change and of the war waged against the game by Dutch farmers, native hunters, and the great English sportsmen, Smith, Harris, Oswell, Gordon Cumming, Baldwin, and Selous. He admits that the sportsmen, though often wasteful, have been a comparatively insignificant cause of the extermination, and pays a warm tribute to Selous for his merciful methods of hunting and his economy of life. Mr. Bryden, is, however, quite clear that the great game is doomed to annihilation, and that many species will soon share the fate of the quagga, the bontebok, and Burchell's rhinoceros. He

suggests that a great game park should be established in or near Mashonaland, where 100,000 acres should be fenced in as a preserve. The enforcement of game laws in unsettled districts he admits to be impossible, while the regulations of the British South Africa Company show that it thinks those laws unjust. While prospectors, explorers, and natives are continually killing for food, and while cattle disease sweeps at intervals through a country, destroying every animal of certain species within it, the toll levied by sportsmen is a very unimportant factor in extermination. The sportsmen, too, as Mr. Bryden admits, have done such good work as explorers and pioneers, and have left among the natives so high a respect for British pluck, truth, and fairness, that it is very ungrateful to blame them. It is true that they may have helped in the extermination of some species, but it is also true that, except for their efforts, many species now exterminated by natives and settlers for skins or for the pot would never have been known to science.

Current African Exploration.

After considering the past destruction of the game of the Cape, it is natural to turn to the work of modern hunters in those parts of Africa where great herds of game still survive. With the return of winter the usual shooting excursions into Africa have started, or are preparing to start. Somali-land is, as usual, the favourite district. Sir Henry Tichborne and Mr. Seton-Karr have left for Berbera, whence they march at once across the Haud to Milmil; thence they hope to make excursions southward and westward, and finally shoot slowly back to the coast. Prince Boris, well-known from his daring expedition to Mount Meru, which he was one of the first Europeans to reach, is preparing to attempt a march from Berbera across the Galla country to Lake Rudolph: his trip in the winter of 1892–3 with the Duc d'Orleans across Somali-land into the northern borders of the Galla country, will have served as a most useful training. It is greatly to be hoped that he will reach his goal, for knowledge of this country is the greatest desideratum in African geography. The last effort in this direction has not been very successful, if the native rumours are to be trusted; for, according to one of these, the expedition of Dr. Donaldson Smith, which left Bulhar in July, has been refused permission to cross the Galla frontier. Apparently Dr. Smith either did not feel strong enough to use force, or did not think himself justified in doing so. We must hope that the rumour is false, for it will be very disappointing, both to Dr. Smith, his companion Mr. Gillett, and those at home interested in African exploration, if his generous expenditure of time and money fails to reap the reward it deserves.

Mr. Greenfield, whose arm was so badly mangled by a wounded

lion in Somali-land that his life was for some time in danger, is, we are glad to state, home again, and well on the way to recovery. The latest news from Mr. Scott-Elliott is still that published in our last number. An account of his march from Uganda to Ruwenzori has been published in the *Geographical Journal*, and a letter giving some notes as to the flora of Ruwenzori has been printed in *Nature*. As more is expected from this expedition than from any other in the field, great anxiety will be felt for Mr. Scott-Elliott's safety during his return march to the coast.

Wasted Wealth in India.

The September number of the *Kew Bulletin* contains some interesting extracts from a memorandum on the Resources of British India recently prepared for the Indian Government by Dr. G. Watt. The British India of the memorandum covers an area of 699 million acres, with a population of about 222½ millions. The agricultural products are considered under six heads—Food-crops, such as wheat, rice, pulses, sugar, spices, etc.; Oil-seed; Fibres; Dyeing and tanning materials; Drugs and narcotics, etc., including tea and coffee; and Miscellaneous, such as cutch, lac, indiarubber, and other wild products. *A propos* of the last-named group, Dr. Watt lays considerable stress on the neglect of the wild or semi-wild animals and plants as a source of wealth. India has borrowed far more than she has given, and the improvements of the future, it is suggested, should lie as much as possible in the path of natural selection and evolution of indigenous materials and systems. These can be counted by the hundreds, and India need not look to foreign countries for new crops while she has a long list of unexploited products which are running to waste in every lane and jungle. An extension of the effort to bring these hitherto unknown products (unknown, that is, to European commerce) into a position of definite recognition is more worthy of serious consideration than the attempt to acclimatise the plants of other countries. The people might be encouraged to grow as hedges round their fields, not useless plants with the sole recommendation of rapidity of growth or efficient protection, but bushes valuable as fuel, or as sources of dyes, tans, fibres, and the like. Already, in the Bombay Presidency, miles of roadsides have been planted with the useful and ornamental *Cassia auriculata*; but in Western and Central India thousands of square miles are overrun by scrubby bushes of *Anona squamosa*, serving at present no useful purpose, though the fibre from the bark might be valuable. *Bauhinia Vahlii*, of which the fibre can be bleached and dyed by the same processes as wool, is a common climber in the jungles of the lower hills, and might, at little cost, be cultivated over rocky country at present next to useless.

Fishing for Coal at Calais.

Nearer home there is another source of wealth that has long lain unworked because unknown, namely the beds of coal that are now believed to underlie the south-east of England. We have lately received important information bearing on this subject.

Stimulated by the discovery of coal at Dover, our neighbours across the Channel bethought themselves, some time since, to make a *forage* near Calais. The proximity of the small coal-field in the Boulonnais, and the circumstance that the spot selected for the experiment was practically on the line of strike of known productive Coal-measures, were also factors greatly in favour of the undertaking. The results, although interesting from a scientific standpoint, are not altogether encouraging in the commercial sense. For we learn that at a depth of 325 mètres the boring tools have entered rocks of Devonian age, as proved by the occurrence of fossils.

Mr. Gustave Dollfus, of the Geological Survey of France, who has carefully watched the progress of the work since its commencement, is of opinion, we are informed, that these Devonian rocks are inverted, notwithstanding the fact that they are horizontal. Judging from the disposition of the upper Palæozoic rocks along the axis of the Ardennes from the neighbourhood of Liège, through Namur and Mons to their underground prolongation into the Pas de Calais, it would seem that Mr. Dollfus possesses sound arguments in favour of his hypothesis, though he has not yet put them forward. In the last-mentioned locality, as at Liège, the Coal-measures are found beneath the Devonian. The Palæozoic beds concerned are much faulted, folded, and at places inverted. These phenomena appear to have been caused by the influence of a force which acted in a direction from south to north, while the chief disturbances resulted in the formation of oblique faults. The most important of these is the great Eifelian fault which bounds the main coal-basin on the south. In many places the Lower Devonian of the basin of Dinant, abutting against the ridge of Condroz, has been thrust over the coal beds of the Namur basin, and this together with the inversion of the Middle and Upper Devonian, Carboniferous Limestone, and part of the Coal-measures themselves has much complicated the stratigraphy.

In these conditions it would be perfectly possible to find the Coal-measures at Calais under either inverted or normally-disposed Devonian, though had the boring been situated rather more to the south, the probabilities would, in our opinion, have been greater. When other borings than the one near Dover reach Palæozoic rocks in Kent, we shall not be surprised, therefore, to learn that these latter are Devonian, and the discovery of beds of that age in the London area does not altogether do away with the possibility of the occurrence of Coal-measures in the neighbourhood of the metropolis. It has long been recognised by geologists that the old Palæozoic floor, or "ridge," under London has much in common with the axis of the Ardennes.

"MENTE ET MALLEO."

IT has often been laid down that the importance of any international congress of scientific workers depends, not on the novelty of the facts or ideas brought forward by individual members—for such are really more accessible if published in one of the recognised periodicals—but on the methods inaugurated for facilitating international communication and co-operation, and for assisting the students and investigators of the science in question, wherever on this earth they may be working. Rules of nomenclature, the organisation of research, the publication of bibliographies: such are the subjects that a great world-congress can worthily and usefully occupy itself with. But since members are but men, and since a congress must assemble in some definite place, it often happens that friendly intercourse, widening of sympathies, increase of knowledge, and the dispelling of ill-founded prejudice are among the most valuable results of such a gathering. But these latter results are subsidiary to the main functions of a congress, and they are by no means inevitable; the worth of a congress that summons its members from the ends of the earth has therefore to be estimated not so much by these accessories as by the solid work that is accomplished for the advance of science and to the benefit of those members who, though they pay their subscriptions, may be unable to attend in person. It is by this standard that we must judge the sixth triennial International Congress of Geologists that was held at Zürich a few weeks ago.

One of the most important tasks that this Congress has undertaken is the preparation of a geological map of Europe on the scale of 1 : 1,500,000, and contained in 49 sheets. The objects of this map are to summarise and correlate the facts accumulated by the various surveys, societies, and individuals, and to represent them on a system that shall, if possible, serve as a pattern for the geologists of the world. It was therefore with gratitude that we heard that six sheets of this map were at last ready for publication, and that ten more would be issued within a year's time. As the subscription price of the entire map is only £4, which may be paid in instalments, the first part being 10s., it is to be hoped that many will send their orders to Dietrich Reimer of Berlin, which they should do before the end of December.

Another useful bit of work that will soon be completed and distributed to members both of this and of the previous Congress, is the "Catalogue of Geological Bibliographies." We understand that this compilation has far exceeded the bounds originally determined for it, and it remains to be seen how far it has been improved, or damaged, by this unusual zeal.

The committees started by the present Congress are two. The nomenclature of rocks has been getting into as sad a state as the nomenclature of animals. Consequently, at the suggestion of Michel-Levy, who himself spoke admirably on the subject, a committee has

been appointed to revise and unify the nomenclature. The English members of this committee are Sir A. Geikie, Professor Judd, and Mr. Teall; other countries are likewise represented by their most eminent petrologists. The changes that occur in glaciers are to be the subject of investigation by another committee, which may possibly also organise the study of erratic blocks. Professor F. A. Forel and Dr. L. du Pasquier will be the secretaries of this committee, the expense of which will be borne by Prince Roland Bonaparte. The English representative is Captain Marshall Hall.

Among the papers read at the Congress there were many worthy of the occasion, such as that by Michel-Levy already referred to, Von Zittel's address on Phylogeny, Ontogeny, and Classification, and Suess's address on the Conformation of the Crust of the Globe by Horizontal Forces. We must, however, express our regret that it was thought necessary to split the Congress into four specialised sections, a course that annuls many of the benefits otherwise derivable from such an assembly, and that would not be dreamed of if only some discretion were exercised as to the subjects of papers. While it is very right and proper that such a Congress should discuss the investigation of overthrusts, the classification of Tertiary terranes, and, perhaps, points of unusual interest in the geology of the country where it is meeting, still we fail to see the appropriateness of such comparatively trivial subjects as the Fossil Camel in Roumania, the Geology of Patagonia or Palestine, Nepheline-Syenite of Sweden, Gabbros of Lower Italy, and "grorudites and tingnaites" (the quotation is from *Nature* of Sept. 20, where a more detailed report will be found).

We had intended to say something about the social and administrative details of the Congress, but we are spared a difficult task owing to the fact that what seems to be a singularly outspoken private letter from Mr. Persifor Frazer has somehow got published in the October number of the *American Geologist*. Had the account been intended for the public eye, no doubt it would have been neither so truthful nor so entertaining. It is, however, refreshing, as well as alluring, to see sometimes the naked truth, and in the present instance those who have the interests of the Geological Congress at heart will not regret the absence of the conventional fig-leaf.

The Climbing Rabbit.

We are indebted to the Rev. George Henslow for an extract from an article in the *Hobart Mercury* dealing with the subject of the adaptation of animals to their surroundings, of which we gave a new instance last month (p. 249). The article records some curious facts concerning the development of a new sort of nail in the rabbits of Australia in consequence of the animals' endeavours to climb over the wire netting used to impede their progress in travelling. The farmers have discovered that the rabbits can

burrow under the netting unless it is buried some six or eight inches under the soil. Moreover, they can climb, or evidently intend to do so, after a little training, and to this end they are developing a nail which will enable them to hold on while progress is made upward. This nail development was noticed before this in Queensland, when the bark just out of reach was desirable of attainment, but to effect hand-over-hand nautical climbing shows the rabbit in the act of elevating himself in the scale.

The Gay Science of Newspaper Criticism.

While the provincial Press openly devotes part of its space to an amusement column, some of the London daily papers afford the same relaxation to their readers in the shape of Science Notes, from which we have on former occasions culled choice specimens. The *Daily Chronicle* in particular has distinguished itself by providing a weekly column of mixed information and criticism. Its writer is especially happy in his observations on the scientific Press, and his remarks on a contemporary of our own must have provided a source of innocent merriment to a large body of readers.

Some time ago we suspected the author of these notes of a certain admirable power of divination, which served him in place of reading the memoirs he criticised. There were displayed a striking unfamiliarity with the contents of the memoirs in question, and a disposition to be hard on the "poor English" in which they were written. This last is plainly a matter on which the writer of the notes is an authority, from practice. His observations on the magazine in question for the current month were therefore pleasantly anticipated by the readers of the *Chronicle*, and their reward is great. Among other interesting things, he tells us that in the pages of our contemporary "mountain sickness is intelligently treated by Professor Roy." We are extremely sorry for Professor Roy that he should lose this approbation, but unfortunately the magazine does not contain an article by him on the subject—*although one was advertised*. We read also that "though Mr. Hawkins on continuous-current dynamos and Mr. Moore on the morphological value of the attraction sphere are stiff reading, they will be valued by physicists." It is quite plain that the morphological value of the attraction sphere furnished very stiff reading indeed in a certain quarter, and Mr. Moore, we fear, will have to postpone the delights of appreciation by *physicists*. The *Chronicle's* complaint of the "unnecessary display of technical language" in scientific papers becomes intelligible. Its editor might do worse than present the contributor of "Science Notes" with a dictionary and a treatise on commercial rectitude—or recommend him to a sporting journal, where gifts of insight and prophecy have a value of their own.

I.

On Random Publishing and Rules of Priority.[1]

AMONG invertebrates the species of insects are reckoned by millions. Of crustaceans there are several thousands of species. There are thousands of species also of spiders. To whatever minute subdivision of the animal kingdom zoologists turn their serious attention, the record of species in it presently rises to scores and hundreds. That a large proportion of these are improperly founded, representing only duplicate names, casual variations, differences of age and sex, or errors of the observer, may be readily conceded. But with every allowance of this kind, the irreducible remainder constitute an enormous multitude. For purposes of study and discussion, it is essential that they should all have recognised names and a place in some system of classification. In the rapid advance of knowledge, system is displaced by system in quick succession. The monographic work designed to bring into one view all the known species of a group almost invariably adds many that were previously unknown. Before such a work can be printed and published, the author's fellow students in several parts of the world will have brought out independent contributions to the subject, thus stamping the monograph with the stigma of imperfection from the very moment of its birth.

Before any comprehensive work on zoology can now be produced, the naturalist finds his studies in a manner divorced from nature. There is so much to read that little time is left for observing. He is in a sort of bondage to an antecedent literature which cannot be ignored, and which comprises often a host of sporadic writings. Under existing arrangements, these scattered essays carry equal authority for their definitions and descriptions of genera and species, wherever, however, and in whatever language published. They may be couched in Russian, Polish, or modern Greek, in Arabic or Japanese. There are linguists to whom none of those languages would present any difficulty, and who would say that one language was as good as another and a great deal better; but even a good linguist might be daunted at hearing that some African sage had described the

[1] Read at the British Association, Oxford, 1894.

fauna of his country in his tribal tongue. So sensitive are some of the most eminent Scandinavian naturalists to the inconvenience of employing a language unfamiliar to the majority of scientific readers, that they write their books in English in preference to using their own Dano-Norwegian or Swedish. We may take it as rather a two-edged compliment to ourselves, implying, as it does, that they are more capable of learning to write our language than we are of learning to read theirs. Notwithstanding this, many readers are duly grateful, and would be only too glad to pocket a similar affront if it were kindly offered them by Russian naturalists.

There is, however, a confusion much more embarrassing to the student than the confusion of tongues. He has to face the perplexing fact that not only may the publication of new genera and species lawfully be made in any one of the numerous countries of the world, but that in any one of those countries there is no limit to the media of publication which may lawfully be employed. A very costly book of travels, with or without other faunistic details, may devote a few lines to a new spider, though containing nothing else whatever that concerns an arachnologist. Some small local society, in the course of its peregrinations, discovers a new earthworm. The description is printed in the local *Transactions* rather for the delight and glory of the members than with any expectation or wish that it should become known to the outer world. A new snail is described in a scientific magazine, and at first sight such a report appears to be exactly in its proper place, until inquiry shows that another new snail has recently been described in a rival magazine of equal scientific pretensions. The *Proceedings* of a Marine Biological Station record a new fish, and again one is inclined to say that such *Proceedings* are precisely the proper organ for making known new fishes, and, were there only one such station instead of an indefinite number, that might be true. Again, there are, in our own country, for example, the great London societies devoted to the promotion of Natural History. They are rich and generous. From a new flea to a new rhinoceros, they give the discoverer all the help in their power to introduce his clients to the knowledge of their fellow-creatures.

In short, no one who is capable of endowing the world with a new species need be at a loss for an opportunity of doing so. When, however, the question arises whether the species be really new to science, the varied facilities for publication wear a less amiable aspect. The conscientious zoologist finds himself in the agreeable predicament of having to prove a negative. It is his duty to ascertain that his proposed species has not been previously described. To abbreviate the investigation, he is often tempted to rely on second-hand authorities, the well-known parents of truth and accuracy. But the position should be considered of those who, with rigid honesty, endeavour to consult original memoirs. Suppose, for

instance, that their researches are concerned with some group of crustaceans. In the literature of that class the great American work by Dana is conspicuous. It has the advantge of not being a farrago, a hotch-potch, a Noah's Ark, a confused miscellany of animals of every kind. It is concerned with crustaceans alone. Nevertheless, within that reasonable boundary, it describes and figures so many species of so many different groups, that almost every carcinologist must at some time or other want to have it at his elbow. But, since only one hundred copies of it were issued, it cannot be every man's book, and since the price of it is twenty pounds, it cannot be within the reach of every naturalist's purse. It is needless to enumerate all the other expensive works of more or less similar character. If these are burdensome, though confining themselves to animals of a single class, what must be thought of books which treat promiscuously of all classes of animals, passing airily from a sponge to a parroquet, from a crayfish to a chimpanzee? Still there is this to be said for large and costly productions, that they are generally well known and not easily overlooked. Far otherwise is it with the great cloud of short essays and pamphlets which equally demand attention. Many of these can only be obtained by diligent search in the catalogues of specialist booksellers, or consulted in some library which possesses the magazines, journals, proceedings, or transactions in which they originally appeared. The latter alternative may seem to involve a very small hardship, but those who have tried it very well know that, whether the naturalist leaves his home to go to the books, or the books leave their home to go to the naturalist, there is delay, there is worry, there is expenditure of time or of money, or of both.

In London alone, papers dealing with Crustacea are accepted by the Royal Society, the Linnean, the Zoological, the Entomological, the Microscopical, and perhaps several others. There are societies similar in purpose to these, and as a general rule equally omnivorous, spread in profusion over all the countries of Europe, throughout the United States of America, and elsewhere. When you have finished studying the *Acts* of the Academy of Rovereto, the *Zeitschrift für Zoologie* of Budapest and the Kansas University *Quarterly*, you may pass on to the *Journal* of the Institute of Jamaica, the *Journal* of the Natural History Society of Bombay, the *Journal* of the Asiatic Society of Bengal, the reports of the Wellington Philosophical Society of New Zealand, of the Linnean Society of New South Wales, the *Anales* of the University of Chili and those of the National Museum of Montevideo. Before you have done, you will have qualified for a Fellowship of the Royal Geographical Society, if only you have found out where all the places are whence all the scientific contributions are issued. Were the student capable of reading the different languages in which they are severally published, he would still be seldom able to afford the time and money required to ensure him an

opportunity of perusing them all. The truth is that, in their zeal and devotion, the labourers at natural history do a great deal of work that is not wanted, of work that has been already done, and of work that is premature. It is, perhaps, almost impossible to place any check upon the performances of the incompetent, but were all that is done of vital importance, the want of any order or method in presenting it to the world would still involve a prodigious waste of time.

It is far easier to point out the evils than to suggest practical and feasible remedies. At the risk of being thought rashly presumptuous, I venture to hint at the possibility of a division of labour among the leading scientific societies. Without any restriction upon the pleasant variety of subjects discussed at their respective meetings, they might be willing to enter into an amicable concert in regard to printing and publishing. By way of illustration, one could imagine that under such a system the Entomological Society would only issue papers treating of insects, which is, I believe, in a great measure its existing practice; the Zoological Society would content itself with essays on the rest of the animal world, and the Linnean would deal only with the science of the vegetable kingdom. This particular apportionment of work is not intended for a substantive proposal, but merely for an explanatory suggestion. There would be other societies to consider, other plans to debate, and many inherent difficulties only to be met by compromise. The Microscopical Society, for example, would naturally say that its work essentially lay in all departments, and could not reasonably be confined to one. Though this would be true of its work, it need not perhaps affect its publications. Such a society might take over the invertebrates from the Zoological, transferring to that and to the Linnean those microscopical investigations which concern the vertebrate animals and higher plants. If only the general idea of the division of labour were accepted, it would be derogatory to men of science to suppose them incapable of devising efficient means of reducing the principle to practice. One might hope that, if a beginning were made, either in this country or in any other, the merits of the system would soon be widely appreciated, and concerted action be taken not only among the societies of a single nation, but among those of a continent.

There would still be the unfathomable deluge of miscellaneous zoological writings spread over books of travel and over countless magazines issued by private enterprise. But upon many or all of these the force of good example and of public opinion might be expected gradually to operate. No one need be alarmed that the proposed concentration of subjects would result in too much narrowness of mind, for there will certainly always be a residuum of magazines aiming at a kind of fragmentary omniscience, giving some information on every possible subject, that every possible customer may find some scientific scrap to suit his favourite taste and particular desire of knowledge.

The British Association, many years ago, rendered a great service to science by recommending the adoption of certain rules for zoological nomenclature. It has assuredly an opportunity of rendering a service of like character, but one of even higher importance and wider usefulness, if it will now take the initiative in devising and recommending methods by which research may be simplified in its literary aspect.

It is true that the scientific world is not left entirely in a helpless condition. The *Naturæ Novitates*, published fortnightly by Friedländer in Berlin, are a substantial assistance. They may be compared to Bradshaw's Guides, which give you a map of the country and tell you what lines of railway there are, what trains run upon them, what are the starting-places, and what is the cost of travelling. Works like the "Zoological Record" may be compared to Murray's Handbooks, which tell you both where to go and what there is to be seen when you get there. Such aid deserves the deepest gratitude. Nevertheless, the "Zoological Record" itself requires the student of the Protozoa to buy, not only the account of the works in which he is interested, but also the accounts relating to all other classes of the animal kingdom, just as if Mr. Murray were to insist that the purchaser of his "Handbook to Portugal" should pay also for those referring to Russia, Denmark, Italy, and the rest of the series. The British Association is, of course, privileged to survey the whole domain of science; nor would any of its members wish to see its liberties curtailed. Yet, as an aside, in a stage whisper, and among ourselves, I may venture to deplore the lot of a palæontologist who wishes to possess the original description of three new species of fossil Estheriæ, occupying a couple of pages in our last volume of reports. Bound up with them, he will have the satisfaction of owning 140 pages containing nothing but numerals, 40 of these pages dealing with "The Pellian Equation," a mystery which a zoologist is likely to study with rapt devotion.

In appealing to the Association to consider whether some order and method may not be gradually introduced into the confused concourse of scientific, and especially zoological, publications, I am perhaps proposing a fanciful, a hopeless, an impracticable task. But even if it should prove that nothing definite could be done, I am persuaded that the discussion itself would be advantageous, by calling the attention of scientific writers to the enormity of the evil demanding a remedy, and the expedience of individual self-denial for the general welfare. In the meanwhile, there are two or three subsidiary points on which it would be easy for the Association to express an opinion, and on which, as it seems to me, such an expression of opinion would not only be valuable but timely.

Many naturalists, I believe, now wish that the tenth edition of the "Systema Naturæ" of Linnæus should be accepted as the starting point of valid nomenclature instead of the twelfth edition. Still

more wish that the recommendation in regard to synonyms should now be made a definite rule, to the effect that any name of genus or species which has once become a synonym should thenceforward not be available for any new genus or species. Another point concerns English-speaking people rather than the world at large. Whether our common English pronunciation of Greek or Latin be the best or worst, it would certainly be a great blessing to those who use it if an effective guide to pronouncing could be furnished by a small improvement in printing. It was long ago suggested, and no better suggestion has ever since been made, that long and short vowels in zoological names should be discriminated by the straight line and the upturned crescent placed above them, as in prosody. From the want of these guiding marks scientific conversation becomes often very confusing. Ninety years ago Sydney Smith observed that a young Englishman who made a false quantity in public at the outset of his career seldom got over it. That day has passed. We no longer mind the false quantities; but in discussion we still want to know what the disputants are talking about. There is sometimes no right or wrong in question. As with the name of a well-known genus of trilobites, so far as the derivation is concerned, it may equally well be Calymĕne or Calymēne, but it surely ought to be definitely one or the other. There is a shell called Nautĭlus, a crab called Lithōdes, a shrimp called Hippolȳte, a spider called Schœnobātes. So far as ordinary printing shows, they may just as well be called Nautĭlus, Lithŏdes, Hippolȳte, and Schœnobātes. When a name *can* be mispronounced it almost certainly *will* be; and then there arises a difficulty of understanding one another between those who use the ear-torturing sounds and those who speak correctly. The adoption of the letter "k" instead of "c" in spelling words of Greek origin would be another improvement, which would help to save such a word as Ceraurus (Kera-urus) from being pronounced Seroārus.

Lastly, I desire to suggest that there is one particular department in which a scientific convention might be established with comparative ease. According to existing rules, priority is granted to those who introduce new genera and species only on condition that they name them in conformity with the binomial system, and publish some sort of recognisable description. Wherever the authority resides which has enacted these conditions, the same authority must certainly have the right and power to add to them in the interests of science at large. My suggestion then is this, that, for the valid publication of species, each country, or any group of consenting countries, should have one and only one authoritative journal. Let no new species, however minutely described elsewhere, have any claim to priority until the author has entered its name in the appointed journal, together with a concise account of the characters relied on for its discrimination. The editor of such an organ would be bound to publish at stated intervals all species offered him as new,

unless withdrawn by the consent of the contributors. One can easily imagine that his experience would often enable him to point out with convincing effect that a supposed novelty was not really new, that a proposed name was pre-occupied, or that the proposed form of it was something a little too horribly barbarous.

Were the principle once admitted that new genera and species could not be sprung upon the world from every obscure hole and corner, but must be announced at some appointed and easily accessible centre, it is not unreasonable to hope that naturalists of different countries might consent to combine their forces for the common benefit. It might be found convenient, for example, that all new Mollusca should be published in France or in Germany, all new Mammalia in Italy or in Denmark. As in that case the journals, receiving contributions from all parts of the world, would be a patchwork of many languages, it is probable that the original descriptions would require to be accompanied by a rendering either into the vernacular of the publishing country, or into Latin, or into some sort of scientific Volapük. Such details, however important, it would be premature as yet to consider. It may be that scientific workers will think absolute freedom preferable to the restraints imposed by systematic combination. Yet it seems overwhelmingly obvious that to every student of a special branch of natural history it must be of the highest advantage to know at once where to turn for accredited information on the most recent discoveries in the group which he is studying. It may be at once conceded that the knowledge of new species is only a small fraction of zoological science, but it is almost certain that around the record of new species in a particular group the reports of other investigations in that group will tend to cluster. We are so accustomed to the process of harvesting a stack of wheat from one spacious field, that probably a farmer could hardly think with patience of a different system, by which his reapers would be forced to tramp over half a county, obtaining a bundle of grain in one place and a wisp of straw in another, and from here, there, and everywhere collecting single ears of corn, happy if they chanced to find a fair proportion not green and sour and meagre and mildewed. Why should naturalists continue in a condition of affairs which a rustic would consider almost inconceivably absurd? However great may be the intellectual and physical vigour enjoyed by men of science, and however ample their prospect of life at the outset, they soon have to realise that the work which they would fain do needs more time and more strength than they can possibly hope to bestow upon it. Often, like Alpine travellers, they reach what seemed to be the longed-for summit, only to find that the true mountain-top is still far away and far above them. Their limbs are wearied. The intervening valley is dense with mist. The shadows lengthen. The night is near. They can go no further. The exploration which they, perhaps of all men, were best fitted to accomplish is left unfinished, because, not on

the difficult heights, but in the lowlands and at the outset of the journey, their progress was needlessly hampered and their time pitilessly frittered away. Even those who make no claim to exalted genius, who attempt no heroic adventures, but who with a modest and faithful industry endeavour to do the needful drudgery of science, may ask to be delivered from a state of affairs which more or less compels them to waste their own time and reluctantly to waste the time of others. If it is true in commerce that time is money, it is no less true that it is also one of the chief factors of success in science. Therefore I appeal to this Association to make a small effort for a great purpose, by appointing a committee to consider how the time of scientific workers in general may be saved, through the introduction of method and order into the publication of scientific results.

THOMAS R. R. STEBBING.

II.

Miocene Man in India.[1]

BY Professor Prestwich's explanation of the mode and relative date of the occurrence of the plateau flint implements in Kent,[2] we clearly understand that Man inhabited the region comprising what is now the South-eastern part of England at a very remote period; so far back, indeed, in the earth's history as to require of us the recognition of a vast lapse of time, measured by the duration of slow natural processes, forming, elevating, and removing extensive deposits of marine, estuarine, and fluviatile materials. In the geological chronology, as marked out by fossils and strata belonging to certain stages in the recognised succession of periods and their characteristic formations, that of the "Red Crag" of Essex (formed in a shallow sea in "Pliocene" times, just before the "Glacial Period" began), has been indicated by Professor Prestwich's researches as having been contemporaneous with the condition of things existing in what is now Kent when Men of some primæval kind existed on hill-sides far above where Crowborough and other ridges in the Wealden area now rise to a much lower elevation.

In France flint flakes more or less worked (chipped), said to have been discovered in the Miocene strata, have been adduced as indicative of Man's existence there at that still earlier period; but some cautious geologists and archæologists have declined to accept the evidence.

We have now, however, to notice the finding of artificially prepared flint flakes, of human manufacture, in a Miocene formation of Further India.

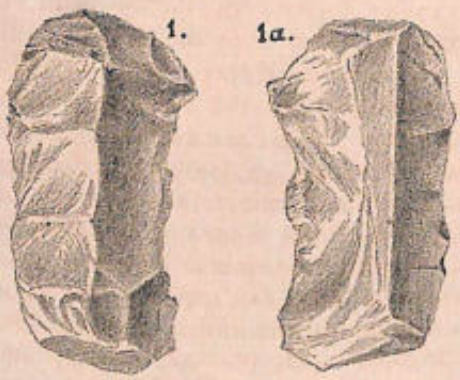
Lately, when mapping the Yenangyoung Oilfield, in Burma, Dr. Noetling was interested in collecting remains of Vertebrate animals, particularly in a ferruginous conglomerate upwards of 10 feet thick, persistent as a dull-red band across the ravines and hills. This rock, lying beneath 4,620 feet of Pliocene strata, contains numerous fossil bones, especially of *Rhinoceros perimensis* and *Hippotherium*

[1] "On the Occurrence of Chipped (?) Flints in the Upper Miocene of Burma." By Dr. Fritz Noetling, F.G.S., Palæontologist, Geological Survey of India. *Records of the Geological Survey of India*, vol. xxvii., part 3, 1894, pp. 101–103, with a plate [marked I.].

[2] See NATURAL SCIENCE, October, 1894, page 269, for special references to Professor Prestwich's several memoirs on this subject.

[*Hipparion*] *antelopinum*. When stooping for a tooth of the latter, Dr. Noetling found some chipped flint flakes, partly embedded in and projecting from the conglomerate; and close to the fossil tooth was one in particular, which he describes and figures (Figs. 1, 1*a*, and 1*b*). Figs. 1 and 1*a* are here reproduced.[2] About a dozen other flint flakes were also found in the rock. Some appear to have been like ordinary flattish "ridge-flakes," and up to about 40 mm. in length; some were smaller and more or less triangular; four of these last are figured (Figs. 2, 3, 4, and 5), each in three aspects.

The chief and largest of the flint flakes, thus fortunately observed and carefully extracted by a trustworthy observer, is elongate-oblong in shape, 45 mm. long by 20 mm. wide; rounded by chipping at one end, obliquely truncate at the other, and ridged on both faces; side-edges thin and sharp, almost parallel. It is sharply ridged on one face (Fig. 1) by the removal of somewhat symmetrical flakes struck off at right angles to the edges and the ridge. The other face (Fig. 1*a*) is bluntly ridged, a narrow portion of the original flake-face remaining



along the middle, from which the sides slope more regularly on one side than the other; one edge having been more irregularly chipped, or, perhaps, subsequently broken, into a ragged and slightly concave outline.

There is no doubt of this being an artificially-dressed flake of flint, actually dug out by an experienced geologist of the Indian Geological Survey, or of the ferruginous conglomerate that contains it and other dressed flakes, belonging to the Yenangyoung Tertiaries of Burma, and to be regarded either as an Upper Miocene bed, that is, of the latest Miocene age, or, at the least, of the earliest Pliocene.

I.—The surface of India has, of course, yielded numerous flakes of agate and flint that have been manipulated by Man, as well as many stone implements of better workmanship. For recorded instances, see Evans's *Stone Implements*, etc., 1872, pp. 79, 103, 115,

[2] Fig. 1*b*, intended for an edge view, is apparently unsatisfactory in its perspective.

214, 249, 259, etc., and the *Manual of the Geology of India*, 1879, vol. i., pp. 440–442.

II.—The gravels and other materials drifted and arranged by rivers in remote times, when the levels of the country differed from those of the present time, and necessarily long before the rivers had settled down in the valley which they now drain, have frequently been found to contain other chipped flakes and rude tools of stone. Some of the latter, from the lateritic deposits in Madras and North Arcot, have been noticed by Foote and others in the *Quart. Journ. Geol. Soc.*, vol. xxiv., 1868, pp. 484, etc.; Evans's *Stone Implements*, etc., 1872, p. 570; *Memoirs Geol. Surv. India*, vol. x., 1873, pp. 43–58; *Manual of the Geology of India*, 1879, pp. 358, 369, and 441. A similar quartzite implement was found in the Narbada (Nerbudda) Valley by Mr. Hacket (*ibid.*, 1889, p. 386, pl. xxi., fig. 1; and in the second edition, 1893, p. 388). Others have been discovered in Assam, Bengal, Orissa, and elsewhere (*Manual*, 1879, p. 441). A "palæolithic celt" was found in the Punjab by Mr. W. Theobald (*Records*, etc., vol. xiii., part 3, 1880, p. 176).

Large cores and flakes from the bed of the River Indus, near Sikkarpoor or Sukkur, in Upper Sind, were found by Lieut. Twemlow 3 or 4 feet below the river-bed, in excavations made for some canal work. These consisted of the cream-coloured flint of the Nummulitic Limestone. Some of the cores were described and figured by Mr. (now Sir) John Evans in the *Geol. Mag.*, vol. iii., 1866, p. 433, pl. xvi.; and a note on the locality was given in the *Geol. Mag.*, vol. iv., 1867, by General Twemlow, who had some flakes also, one of which he gave me, together with a core. One of these cores from near Sukkur, on the Indus, is figured in the *Manual Geol. India*, vol. i., 1879, p. 442, pl. xxi., fig. 3; and flakes of this Nummulitic flint are there described as being common in Sind.

Small chalcedonic cores from the banks of the Mahanuddy River are mentioned in Evans's *Stone Implements*, etc., 1872, p. 21. So also small agate cores, found by Lieut. Irwing in the Nerbudda alluvium, are alluded to in *Geol. Mag.*, vol. iii., 1866, pp. 93 and 283. Chipped stone implements also were found on the affluents of the Kishna, by Foote,[4] in conglomerates on the Malprabha and its affluent the Beni-halla, in the South Mahratta country, where "fine, well-shaped, and mostly large-sized chipped quartzite implements" are very numerous wherever the red lateritic subsoil, which is older than the alluvium, is exposed.

A very definite ridge-flake of agate was found by Mr. Wynne in the ossiferous conglomerate on the Upper Godavery, near Pyton (Paitan) south of Arunzabad. It is noticed in the *Geol. Mag.*, vol. iii., 1866, pp. 93, 94, and 283, and in the *Records Geol. Survey India*, vol. i., part 3, 1868, pp. 61–65, with a plate. It is also mentioned and illustrated

[4] *Memoirs Geol. Survey India*, vol. xii., part i., 1876, p. 241.

in the *Manual Geol. India*, 1879, pp. 389 and 441, pl. xxi., figs. 2, 2*a*. A good general note on the worked flakes and stone implements found in India is given at pp. 440–442 of the *Manual*, 1879, with references up to date.

Of course some of the occurrences of flakes and cores above-mentioned may be merely prehistoric, but others are of Pleistocene and some probably of Pliocene age.

Certainly the many stone implements and chipped flakes found in Southern India at different elevations appear to prove that great changes in the physical geography of the Indian peninsula have taken place since the time when the implement-makers first inhabited the country.[5] These terrestrial movements and consequent alteration of surface-conditions were mostly Post-Tertiary, but some probably took place in the Pliocene or late Tertiary times, when such disturbances appear to have been frequent and energetic.

III.—Dr. Noetling's discovery (see above, p. 346) takes us back to still earlier times, for he has good reason to regard the rock from which he took the specimen that we have described above from his notes and figures as of Miocene age. With a philosophic caution he gives the benefit of any doubt in this case to the early Pliocene (in the text) rather than to the latest Miocene (as in the title of the paper). We note that he throws an unnecessary doubt on the word "chipped" in the title of his paper, though there can be no doubt as to the artificial dressing of the flake. So also, in spite of his other doubt, the "Miocene" of Burma with its *Hippotherium* (*Hipparion*) may well belong to that geological stage, although by "homotaxis" (that is, the mere local succession of certain fossils) it might be supposed to be either older or younger, if its organisms had not been really contemporaneous, but localised by migration at unequal times—that is, at different periods. The *Hipparion*, however, one of the predecessors of the Horse, had powers of progression in and along the great Asiatico-European continent similar to those the Horse is known to have had; and its fossilised remains, therefore, characterise deposits in which they occur in France, Germany, Greece, Northern India, and Burma, not only as homotaxeous, but as being really *contemporaneous*. If the equine animal, so the then existing Man would be really of the same age as the Miocene fossils and the associated manufactured flint flakes of France (?) and India.

Long ago Dr. Hugh Falconer expressed his opinion that the remains of Man would be found in the Tertiary strata of India. The researches in the Sewaliks and elsewhere by Colonel Cautley and himself had caused them to entertain that idea; and in 1865 Dr. Falconer stated that more than twenty-five years previously they had shadowed out this speculation in a communication to the Geological Society of London, and that nothing had occurred to

[5] *Quart. Journ. Geol. Soc.*, vol. xxiv., 1868, p. 484.

invalidate it since.[6] Indeed he held it strengthened, for at page 388 he stated: "Here then was clear evidence, physical and organic, that the present order of things had set in from a very remote period in India. Every condition was suited to the requirements of Man; the lower animals which approach him nearest in physical structure were already numerous; and the wild stocks from which he trains races to bear his yoke in domesticity were established; why then, in the light of a natural enquiry, might not the human race have made its appearance at that time in the same region?"

Greatly would our old friend, this energetic worker and philosophic thinker, have welcomed such evidence of human workmanship in Miocene times, as is shown in Dr. Noetling's discovery. The late Dr. T. Oldham, fully appreciating Dr. Falconer's broad views and acute observations on the remote antiquity of Man in India, entered fully into the subject in the *Record Geol. Survey India*, vol i., 1868, pp. 66–69, and transcribed from his memoirs very important portions bearing upon it.

T. Rupert Jones.

[6] See *Quart. Journ. Geol. Soc.*, vol. xxi., 1865, p. 386.

III.

The Wing of Archæopteryx.[1]

Part I.

ARCHÆOPTERYX, as is well-known, is the oldest bird hitherto discovered, and forms one of the most interesting and remarkable fossils from many points of view. Thus far, only two examples are known, one being in the British Museum, the other in the Natural History Museum of Berlin. Both were obtained from the Solenhofen Slate (Lower Kimmeridgian) of Eichstadt, in Bavaria, though an interval of some years intervened between the discovery of the two fossils. For the present we are concerned with the wing only of the bird, and since this organ has been most wonderfully preserved in the Berlin example, all that I have to say in the first half of my paper will have reference to the second specimen.

Readers of Natural Science will remember that Dr. Hurst contributed to the pages of this Journal for October, 1893, an essay entitled "The Digits in a Bird's Wing: A Study in the Origin and Multiplication of Errors." He chose, as a case in point, the wing of Archæopteryx, and it would seem that his choice was an unfortunate one. The object of the present paper is to try and show that the result of that essay was but to contribute another to the already vast pile of errors Dr. Hurst had set himself to undermine. At the time this paper was read we met, so to speak, on equal terms, inasmuch as neither of us had ever seen the actual specimen. Since then, however, Dr. Hurst has made a pilgrimage to the Berlin Museum, and, therefore, has had the advantage of examining the fossil itself. My own information has been obtained from a study of photographs and the literature of the subject; while my investigations have been carried on in the Department of Comparative Anatomy of the University Museum, Oxford, and were undertaken at the instance of Professor E. Ray Lankester, who desired me to make a model of the wing, to be exhibited in the museum. This has now been done, and I should like here to record my grateful thanks for kind help and advice during the work of restoration.

In devoting the whole of the first part of the paper to a discussion of the wing itself, I trust I shall not seem to be wasting

[1] Read at the British Association, Oxford, 1894.

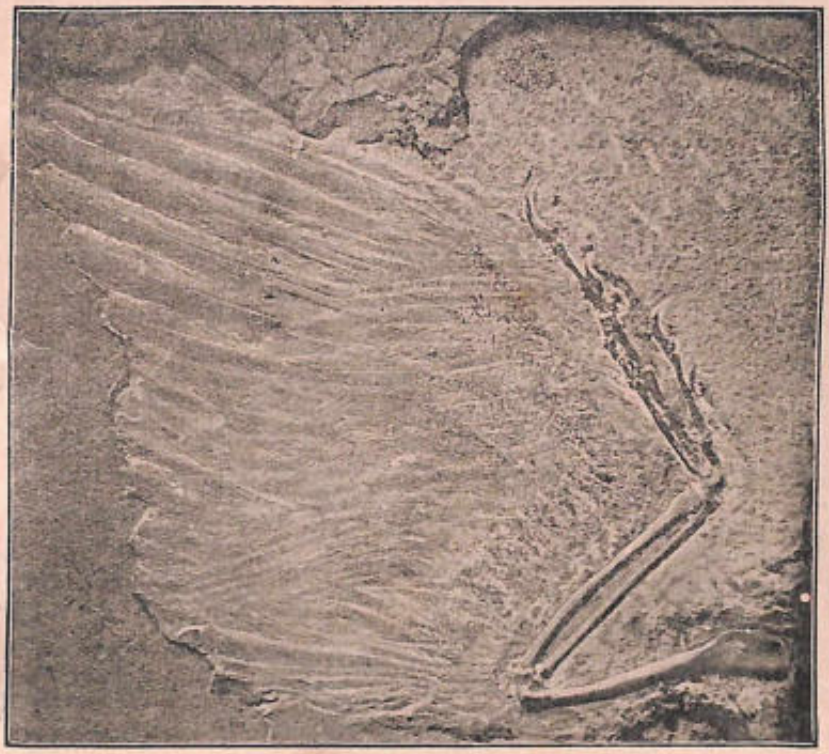


PLATE I.

PHOTOGRAPH OF LEFT WING OF ARCHÆOPTERYX, from the specimen in the Natural History Museum, Berlin.

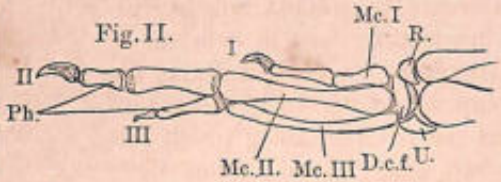
[From article by Dr. C. H. Hurst, NATURAL SCIENCE, October, 1893.]

space; such a course seems to me imperative, if anything like an intelligible idea of what follows is to be gained. Throughout I have, whenever possible, compared the various points of importance with similar points in the wing of living birds, by which means we shall, I trust, grasp more thoroughly than heretofore the wonderful similarity between the wing of yesterday and that of to-day. In the second part, I hope to consider the various restorations which have appeared since the first discovery of the fossil.

The fore-limb of the higher vertebrates is pentadactyle, or five-fingered; thus, then, the manus of the modern bird is deficient in the number of its digits. This deficiency has come about during the specialisation of the organ for the purposes of flight; it became an interesting problem, therefore, to find out the lines along which this reduction has taken place—that is to say, were the two missing digits lost simultaneously or one at a time? and perhaps more important still, at what stage in the development of modern birds did they disappear, and which of the five do the three remaining digits represent? Hitherto, it has been taken for granted that the digits in the wing of existing birds correspond to the pollex, index, and medius, or, in other words, to nos. I., II., III. of the normal pentadactyle manus. This question has lately been raised by Dr. Hurst in the pages of this Journal, and will be discussed at length later.

With the component parts of a bird's wing my readers are, of course, all familiar; but I would just remind them that in the adult bird there are but two free carpal or wrist-bones, and these belong to the first or proximal row, while those of the second or distal row have become anchylosed or fused together into one mass, blending at the same time with the bones of the metacarpus or hand. In the embryo, however, we may have as many as seven distinct carpals, and the metacarpal bones are free. Such a stage as this we assume to represent a sometime adult form, and it is not, I think, beyond the pale of possibility that fossils showing some such arrangement may be discovered.

In how far the oldest known fossil bird, Archæopteryx, contributes towards the chain of gradations which must have existed we unfortunately cannot tell, inasmuch as, though there can be little doubt that the number of digits did not exceed three, and that the metacarpals were not fused together, the carpus is not sufficiently well-preserved to enable us to say anything definite. From the photographs which have been lent me, I fancy I can make out, in one, a large subcrescentic mass, opposed to the base of the metacarpals, but quite separate from them, and representing a fused row of distal carpals, and very imperfectly, a radial and an ulnar carpal, with an intermedium, closely applied to the radiale, between them. In the photograph accompanying this paper, which was taken in a different light, the distal carpal would seem to have fused with the metacarpal bones II. and III., but does not extend to I. Hence, how much is due to



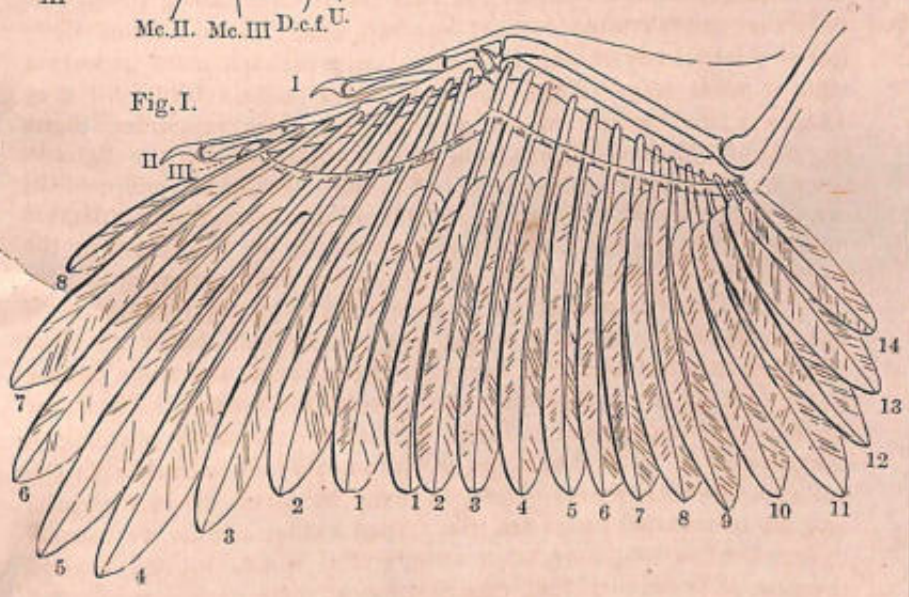


PLATE II.

FIG. I.—Restoration of the wing of Archæopteryx, showing the probable distribution of the remiges or quill feathers.

1–8.—Metacarpo-digital remiges or primaries.

1–14.—Cubital remiges or secondaries.

I., II., III. = Digits I., II., III.

The band running along the bases of the quills represents the tendon described on p. 356.

FIG. II.—Portion of the wing of a half-grown Ostrich (after Parker) showing the component parts of a bird's manus, and illustrating the similarity between the manus of Archæopteryx as seen on the Berlin slab with that of existing birds.

R.—Radiale. U.—Ulnare. Mc. I., II., III.—Metacarpals I., II., III. Ph.—Phalanges.

shadows and how much is real, is a matter upon which Dr. Hurst will be much more qualified to speak than I. Up to the present, all who have described the fossil seem to agree that only one free carpal can be certainly made out, and that this probably corresponds to the radiale.

Whichever of the two possible arrangements of the carpus is correct, the wing of Archæopteryx resembles in a remarkable degree that of existing birds; this fact will be seen at a glance if the reader will turn to Pl. II., Fig. II., where he will find for comparison the figure of the wing of a half-grown ostrich.

The differences, then, I think, between the wing of Archæopteryx and that of existing birds, are rather those of degree than of kind, such as the fusion of the metacarpals, and the reduction in the number of the phalanges of the third digit, and the little platforms of bone thrown out from the posterior margin of the phalanges of the II. digit; these are merely improvements on the original pattern, so to speak. The metacarpal bones of Archæopteryx were evidently quite as stout as those of modern birds; that of the III. digit is even stouter.

In the possession of claws, Archæopteryx agrees with existing birds, in that we might enumerate many species in which claws are present on digits I. and II., and in at least one instance—that of a young ostrich—on III. also. The function of these claws we shall discuss later.

The resemblances between the wing of Archæopteryx and that of modern birds, as I have just said, are greater than the differences; or to be more exact, the bones of the arm, forearm, and hand of Archæopteryx correspond so nearly with those of modern birds, that there seems no reason for suspecting that there existed in Archæopteryx any structures—such as additional digits—other than those seen on the slab.

Judging from the photograph, the wing-feathers of Archæopteryx seem, relatively to the size of the bird, quite as well adapted to the purposes of flight as those of modern birds; and we may infer, therefore, they were arranged and attached to the skeleton in a very similar manner. I doubt not but that, to many of my readers, the precise relations between the skeleton and the supported feathers have hitherto been regarded as details, not necessary, or of any value whatsoever to them in the "struggle for existence," and hence, they have been entirely ignored. Let me briefly sketch the facts. The wing-feathers are divided into (A) remiges or quills, and (B) tectrices or coverts. The remiges are the longest and strongest feathers in the wing, and run along the posterior (postaxial) margin of the forearm and hand. They are divided into (α) primaries (metacarpo-digitals), and (β) secondaries (cubitals). The primaries rarely exceed eleven in number, and never fall below nine, while the secondaries vary considerably. The primaries are distributed along the manus in a definite order; thus, save in rare exceptions,

Fig. I.



Fig. II.



Fig. III.

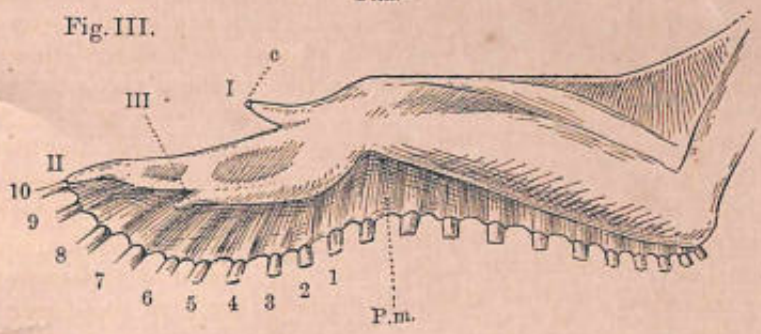


PLATE III.

FIG. I.—The right wing of a nestling *Opisthocomus cristatus*, ventral view, showing how that the second digit (II.) is produced beyond the ala membrana (P.m.), and that the development of remiges 8–10 has been arrested, so as not to interfere with the freedom of the long finger when climbing.

FIG. II.—The right wing of a nestling of the Common Fowl (*Gallus bankiva*), an ally of *Opisthocomus*. Owing to the exchange of an arboreal for a terrestrial habit, the manus is gradually shortening. The pollex only retains the claw. The II. digit projects but slightly beyond the ala membrana; but the development of the most distal remiges is still arrested.

FIG. III.—The right wing of an adult *Opisthocomus cristatus*, ventral view. Contrast with that of the young bird, and the manus is now shorter than the forearm, the "pollex" or "thumb" is considerably reduced, as is also digit II. The claw may be seen on the pollex as a slight wart (c).

1–10.—Metacarpo-digital remiges, or quill feathers. P.m.—Ala membrana or posterior wing membrane. c.—Claw.

the metacarpal bones support 1—6 (metacarpals) the single phalanx of digit III. bears no. 7 (addigital); 8, 9 are attached to phalanx I. of digit II. (middigitals), and the remaining feathers to the antepenultimate phalanx of digit II. (predigital). This is the normal arrangement as we find it to-day. Bands of muscle and tendon bind these feathers together, and to the skeleton, so firmly that a large amount of force is necessary to remove any one of them. The whole of this knitting together is completed by a strong tendon running along the posterior (postaxial) margin of the wing some distance down the quill, from the elbow to the tip of the longest finger, the quills appearing much as though they had been pushed through it, so as to ensure their preserving the right distance apart, and all opening and closing in unison. The quills of the hand are set rather closely together, so much so, that they nearly touch one another at the base, but not quite; thus a series of troughs is formed, and in these lie the bases of the strong, stiff, major coverts which therefore serve as an additional source of strength, binding all into a compact whole.

Turning to the restoration, it will be seen that I have assumed some such arrangement to have obtained in Archæopteryx, as that which has just been described. Indeed, the number and position of the digits, the proportion, texture, and position of the feathers, are so clearly indicated on the slab, that I should rather be said to have copied than "restored" this wing, using that of living birds as a guide to the details. But let the reader turn to the photograph (Pl. I.) and judge for himself. First of all, I would suggest that he should begin by "setting" digit III., which has evidently been dislocated. Then, if the line of the shafts of the quills be carried up toward the skeleton, that of the outermost and shortest will be found to lead to the tip of digit III. running alongside the claw; tracing up the others in like manner, he will find that he has room proximally for some two more feathers, which I have taken the liberty of adding; the fact that these are not indicated on the slab is probably due to the cubital remiges overlapping them.

We have now to consider the question as to whether the actual feathers (or their equivalents from infiltration) are preserved on the slab, or whether what is left is but the impression of the ventral surface; either of these two views is possible. I do not remember to have seen this point raised before, but the general impression among my friends seems to be that, what we have presented to us is the ventral surface. If this is the case, as my colleague Mr. E. S. Goodrich has pointed out to me, not feathers but only their impression is left on the slab.

I should be delighted if it could be proved beyond doubt that this latter view is correct, for we should be more easily able to understand the long, semiplumous covert feathers partly concealing the remiges, inasmuch as the coverts of the under surface are often of precisely this texture in modern birds. Again, this would account for the

fact that no trace of the base of the quills can be seen, as well as explain the facts noticed by Dr. Hurst when examining the fossil. The notes which he has kindly sent me contain the following passage: "Feathers along outer side of ulna not cut away but apparently shifted (through decay of muscles, etc.) so as to lie in same horizon as axis of ulna in greater part, but distally, somewhat lower and distinctly beneath the three visible digits."

Supposing, however, that we are looking at the dorsal surface; then it is not quite easy to see how it is that no trace has been left of the base of the quills resting on the skeleton, for I am told these have not been cleared away to expose the bones. By way of complicating matters, we have to explain the semiplumous feathers before referred to. I think it is evident that these cannot be the homologues of the major coverts previously mentioned. In the first place, chiefly because they not only cross the quills at a considerable angle, but extend beyond them to the tip of the II. digit. They must, then, represent some one or other of the series above the major coverts. Unlike the quills, which would scarcely seem to differ from those of modern birds, these coverts, in their length and texture, depart from the normal habit of dorsal coverts, and resemble rather the ornamental plumes such as decorate the wings of *Cotinga pompadora*.

The crucial test, to my mind, to decide which of the two surfaces we are dealing with, is that of the overlap of the feathers; thus, if we are looking at the dorsal surface of the wing, then the feathers should have a distal overlap, *i.e.*, the free edges of all the feathers will be outwards; if at the ventral, then we ought only to see one broad vane, not two as in the upper surface, and that vane the inner, so that its free edge will be proximal or inwards. Whether or not I have a defective vision remains to be proved, but I feel persuaded that we have the dorsal surface presented to us, and not the ventral. If the reader will note the direction of the light from which the photograph was taken he will find that the distal vane of every feather is lit up, while the proximal vane is in shadow; further, it will be seen that mud has drifted in between some of the feathers, *e.g.*, 5; which, reflecting the light, sharply defines the outer vane of one feather from the proximal immediately under it.

In order, however, to prove this point if possible, I have taken a cast of a sparrow hawk's wing. This I propose to have photographed, illuminating it from the same direction as that of the Berlin photograph, when we shall, I imagine, gain precisely the same effects of light and shade.

Now let us consider the probable function of the claws and power of flight in Archæopteryx.

In fairness to myself, I must beg the reader to follow me a short space off the beaten track, in order that we may discuss what seems a rather important piece of evidence in support of the theory I wish to propound.

Some time since I discovered that in the nestling of the common fowl, out of the ten developing remiges, the growth of the three nearest the tip of the wing was completely arrested, after a certain stage, until comparatively late in life.

Here was a mystery which seemed to admit of no interpretation. What could be the meaning of this extraordinary fact? The key to this riddle was obtained through the kindness of Dr. P. L. Sclater, who recently placed in my hands for investigation a series of nestlings of *Opisthocomus cristatus*, a very primitive and altogether remarkable gallinaceous bird. These were acquired, at the instance of Dr. Sclater, out of a grant from the Royal Society by Mr. Quelch, curator of the British Guiana Museum. For a most interesting account of the habits of these birds I must refer my readers to Mr. Quelch's valuable paper in the *Ibis* for 1890. With one of the many strange facts contained in this essay, however, we are immediately concerned, and that is the wonderful powers of climbing, and, stranger still, of swimming, that the nestling seems to be endowed with.

It would seem that the habits of this bird are arboreal in the strictest sense of the term. The young are, therefore, reared in the trees; but, apparently, instead of remaining in the nest until they are able to fly, they are in the habit, at a very early stage in their life, of climbing out of the nest,[2] it may be to gain a better coign of vantage whence to meet the parents returning with food, or to take refuge in more dense foliage to escape an enemy—sometimes the one and sometimes the other cause impels them to leave their home; in either case it is obvious that the chances of a fall are exceedingly probable. Now, as might have been expected, these youngsters are particularly capable of taking care of themselves, possessing not only enormous feet fitted for grasping—like the parents—but they have powerful auxiliaries in the shape of the beak, which is used much as is that of a parrot in climbing, and wings which are armed with large claws on the first and second digits. On turning to Fig. I., Pl. III., it will be seen that the general form of the wing renders such a mode of locomotion quite probable, and may therefore be said to entirely corrobate Mr. Quelch's statements. The hand is considerably longer than the forearm, the pollex or thumb extends beyond the level of the tip of the III. digit, and is provided with a large claw; the II. digit, with an equally large claw, is produced beyond the fold of skin running along the posterior border of the wing, which encloses the base of the quills. Of these, it will be noticed, only 1—8 have extended any distance beyond the wing-fold just mentioned, so that a long free finger-tip is left. As the bird grows and the feathers develop, the proximal ones grow faster than the distal, so as not to impede the freedom of the hand in

[2] This seems to be always placed over the water, and should the young by any accident fall in they save themselves by swimming; if pursued, they dive!!

climbing; but as soon as the proximal feathers have increased sufficiently to serve to break the force of a fall, should such occur, the remaining distal feathers begin to develop; at the same time, the hand begins to shorten, till, as will be seen in Fig. III., in the adult the hand has come to be *shorter* than the forearm, the claws have disappeared, the thumb no longer extends to the level of the III. digit, nor does the II. project beyond the posterior wing-fold, the bird now being able to move from one place to another by flight instead of by climbing. In the course of development, I imagine the species has managed to shorten the hand and lose the claws as soon after they ceased to be of assistance as possible; but could we discover a yet more primitive form it is probable that we should find that the claws and long hand were retained throughout life.

The common fowl, being a more recently-evolved form which has exchanged its arboreal for a terrestrial habit, is gradually eliminating this phase in the life-history. The hand of the nestling is still longer than the forearm, though not much; but the II. digit is scarcely produced beyond the wing membrane and the claw has been lost though present in the embryo, the pollex has retained the claw, and extends just up to the III. digit. The arrested development of the quills, however, still persists, and apparently for the same period as that in *Opisthocomus*, though it has long since ceased to have any function.

If we are to believe that birds have been derived from an arboreal ancestor, then *Opisthocomus* proves to be a bird of most exceptional interest, for it is probable that the peculiar habits of the nestling may be a survival of an order of things handed down from the very dawn of avian development.

If these speculations have any value, will it seem too great a demand upon the imagination to suggest that the claws in Archæopteryx were of prime importance only during the *nestling* period, and that their presence in the adult may be rather due to the fact that they had not "had time," so to speak, to erase them after they ceased to be functional, as *Opisthocomus* has succeeded in doing to-day? Possibly they may have been used by the adult on rare occasions, but surely it is doubtful.

The view that my readers take with regard to this matter will depend entirely upon their conception of the power of flight of Archæopteryx. Generally speaking, it is probable that it has been considerably underrated. Perhaps undue anxiety to enhance the value of Archæopteryx as connecting link between the Reptiles and Birds has done not a little to warp the views of many of those who have expressed an opinion on the subject. Usually, if we ask for some sort of estimate of the flight of this ancient bird, we are referred to flying lizards and flying squirrels—unsatisfactory comparisons.

Judging from the relative size of the wing as compared with the

body, we might justly have expected something more worthy of the name of flight than this, though possibly it may not have been of a much higher order than that of *Opisthocomus*, which is said never to have been seen to take a more extended journey than that of a few yards at one time. The presence of the claws, too, has done much to injure its reputation as a flyer—a factor, as we have seen, which probably ought not to have been allowed to weigh at all.

W. P. Pycraft.

IV.

Further Notes upon the Organs of Arachnids.

IN an article contributed to NATURAL SCIENCE (vol. iii., p. 441) at the end of last year, and in some previous reviews therein mentioned, I endeavoured to trace the light thrown by recent researches upon the structure and relationships of the spiders and their allies. Several fresh memoirs upon the subject have appeared during the year now passing away, and some account of these also may prove of interest.

A question discussed in several of my previous reviews was whether lung-books or tracheal tubes are to be regarded as the older form of breathing organ among the arachnids. The majority of modern naturalists, following Professor Ray Lankester in considering *Limulus* a marine arachnid of ancient type, believe the lung-books of scorpions and spiders to have been formed by the inpushing or sinking of the gill-bearing limbs of a water-breathing ancestor, and the tracheal tubes found in the higher spiders and other arachnids to have been derived by the simplification of such lung-books. The opposite view is maintained by Mr. H. M. Bernard, who, considering *Limulus* a "specialised crustacean," sees in the tracheæ of arachnids the representatives of the original series of ventral setiparous glands in a hypothetical worm-like ancestor, and believes that lung-books are a new development from the tracheæ. In support of this idea Mr. Bernard has in several recent communications (1, 2, 3) announced the discovery in the Chernetida (false-scorpions) of a series of paired vestigial stigmata on all the abdominal segments behind the fourth (the functional stigmata leading to the tracheal tubes being found on the third and fourth abdominal segments). Unfortunately, however, this observation has been severely called in question by Dr. H. J. Hansen, who, in a discursive paper of considerable interest (4), dismisses the "vestigial stigmata" as nothing but lyriform organs (*see* NAT. SCI., vol. i., p. 525), of which he finds not a pair only, but two pairs on each abdominal segment. Mr. Bernard, in his latest paper on the subject (5), returns undaunted to the charge, and expresses his conviction that the objects under discussion are not lyriform organs. He admits, however, that there are two pairs of them on some segments, and suggests that if one series represent vanished stigmata, the other may be the vestiges of former spinning or cement glands.

But while the facts are still a matter for controversy, the construction of phylogenetic theories can but be speculative.

Another order of arachnids—the Pedipalpida, or scorpion-spiders—is, however, brought forward by Mr. Bernard in this last paper to confirm his opinion. Examining dried specimens of Thelyphonidæ so that the ventral surface of the abdomen reflects the light, he finds, outside the muscle impressions from the fifth to the eighth segment, a paired series of scars, supposed to represent vestigial stigmata, and of areas supposed to represent the flattened vestiges of vanished limbs. Hence he concludes that the ancestor of the Arachnida possessed a pair of limbs and a pair of stigmata on every segment. That a very remote ancestral form of the group possessed a long series of paired limbs, functioning as breathing organs, is quite possible; but the fact that in primitive living terrestrial arachnids (the scorpions), as well as in both living and extinct aquatic arachnids (*Limulus* and the Eurypterida), no breathing organs occur behind the sixth abdominal segment, renders it highly probable that in the immediate ancestors of the class the series had already become so far reduced.

In his researches upon the Chernetida (3), Mr. Bernard has paid much attention to the glands. He confirms Croneberg's statement that the spinning-glands are situated in the cephalothorax, wherein they occupy a large space dorsally, and that their ducts open behind the tips of the movable fingers of the cheliceræ. The peculiar comb-like structures on these fingers, generally known as flagella, are, therefore, probably used in working up the silk. The glands, opening by median papillæ upon the second and third abdominal segments, which were thought by some observers to produce silk, are believed by Mr. Bernard to secrete cement for fastening the eggs together. The coxal glands have openings on the hinder face of the coxæ of the third pair of legs.

Opening on the first abdominal segment, beneath the genital operculum in Chernetids, are a pair of curved conical tubes, with transversely folded walls, known as "ram's-horn" organs. These are said to occur only in the male, and being sometimes found evaginated, were believed by Croneberg and Menge to have a reproductive function, although the former considered them homologous with the tracheal tubes. Mr. Bernard has found these structures in one of his specimens, and observing that they are surrounded with air-chambers, concludes that their function is respiratory. He naturally regards them as homologous with the tracheal tubes, and expresses his belief that they represent the starting-point from which tracheal tubes or tufts and lung-books have been elaborated. He would thus find here another argument for the primitive nature of tracheæ with respect to lung-books.

In a former review in NATURAL SCIENCE (vol. ii., p. 449) I mentioned some well-known facts of comparative anatomy and

palæontology which strongly support the generally-received view that lung-books are the more primitive. A recent investigation (6) by Mr. O. L. Simmons into the development of the breathing organs of spiders supplies valuable embryological evidence upon the same side. This observer finds that in species of *Theridion* and *Agelena* the lung-plates arise upon the hinder surface of abdominal limbs in exact correspondence with the origin of the gill-leaves of *Limulus*, and that the appendage simply sinks into the abdomen without inversion or other complication, exactly in the way lately suggested by Dr. Kingsley, and figured in NATURAL SCIENCE (vol. iii., p. 442). In tracing the development of the tracheæ (which in the higher spiders replace the hinder pair of lung-books), Mr. Simmons finds a stage showing folds indicative of the lung-plates, which vanish later. We have here, then, another link in the strong chain of evidence that among arachnids lung-books preceded tracheal tubes, and that they represent the gill-bearing appendages of aquatic ancestors.

Two interesting cases of virgin reproduction among arachnids have recently been noted. Hitherto, the occurrence of parthenogenesis has hardly been known in the class, though instances among the other two great arthropod classes—the crustaceans and insects—are sufficiently familiar. Now, however, M. Trouessart has (7) brought to light a case among the mites, and Herr Damin (8) one among spiders. The former observer has studied a species of sarcoptid (*Syringobia*) which infests the wing-feathers of the Redshank (*Totanus calidris*). While the bird is on its migration between its northern summer breeding haunts and its southern winter resorts, two kinds of colonies of the mite are found within the quills. The normal colony consists of males and females in their adult and preparatory stages. In the parthenogenetic colony are found specially modified females, incapable of impregnation but producing fertile eggs; very rarely, a few abortive males are present. During the bird's sojourn in the south the mites live freely upon the barbs of the feathers; when the time for migration draws near, three or four in the nymph stage of development enter the quill. According to M. Trouessart, if both sexes be represented among these, a normal colony will result, but if all be females they will become capable of virgin reproduction—a truly remarkable adaptation to their confined mode of life during the period of migration.

The parthenogenetic spider observed by Herr Damin was a female of *Filistata testacea*, a species common in the south of Europe. Taken while still immature, this spider underwent two moults, and then, never having been in company with a male, spun a cocoon and laid eggs, which in due time hatched, producing healthy young. Some time after this, strangely enough, another casting of the skin took place. In connection with his observation, Herr Damin notices that while females of *Filistata* are very common, the male is exceedingly scarce.

The memoir of Dr. Hansen (4), already mentioned, adds considerably to our knowledge of sense-organs in arachnids, supplementing in many respects M. Gaubert's researches, already reviewed in NATURAL SCIENCE (vol. i., p. 525). Lyriform organs were not observed by M. Gaubert in scorpions or in Solifugida. Dr. Hansen has found them on most of the joints of all the walking legs, and on two joints of the palps, in both *Scorpio* and *Buthus;* and also on the middle finger of the cheliceræ of various genera of solifugids. He has noted them, too, in various regions of arachnids of other orders where they had not been seen by M. Gaubert. In a series of ridges and furrows upon the inner face of each chelicera in solfugids, Dr. Hansen believes he has discovered a peculiar stridulating organ. He describes and figures many interesting varieties of tactile hairs and other sense-organs. A specially interesting discovery is that of two air-stigmata, one at the proximal and one at the distal end of each of the eight tibiæ of the legs of *Phalangium* and some harvestmen of other genera. The existence of such stigmata on limbs is claimed by Dr. Hansen as unique among arthropods, and seems a special adaptation to the excessive length of leg characteristic of the Phalangiidæ. In the other families of the order, whose legs are relatively shorter, these stigmata appear to be absent.

In his publications on arachnid morphology, Mr. Bernard mentions an extensive research upon the Solifugida which he has in progress. A study of the animals of this order, with their imperfectly fused anterior segments and other archaic characters, cannot fail to throw additional light upon the points now under dispute. Mr. Bernard has already issued several preliminary notes, the most interesting of which (9) announces the discovery of lateral eyes in these animals. Hitherto the two central eyes only had been observed. Two lateral eyes have now been found by Mr. Bernard on either side of the cephalothorax; owing to the great development of the cheliceræ, they seem to have travelled from the dorsal to the lateral surface. They appear to be degenerating, though in some cases a ganglion and retinal cells can be observed.

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GEO. H. CARPENTER.

V.

Anlagen.

WHEN one is studying the actual development of any organism, one sees, in microscopic sections, the beginnings of organs before these have assumed definite shape and form. In some cases, as in the case of the developing reproductive organs of many animals, these beginnings come into visibility first as three or four definite cells, clearly marked out from the cell-mass in which they lie, by difference in character which is directly visible, or by difference in character apparent only by difference of reaction to certain stains. In other cases, perhaps in the majority of cases, the beginning of an organ or structure, so far as visibility to us goes, is not two or three separate cells, but a thickening of tissue resulting from the more active proliferation or multiplication of cells in some definite area. For instance, in many vertebrates the nervous system, as it first becomes visible under the microscope, is a broad band or plate distinguishable under the microscope by the relative opacity it causes along the dorsal surface of the embryo. At first the whole young embryo of an animal or plant is, in the majority of cases, a mass of cells indistinguishable from each other in appearance. At various points in this mass the various organs come up into visibility, successively or simultaneously, in definitely localised regions of the common mass of germinal cells.

For these first visible appearances of organs German writers use the word "Anlage," borrowing it from its familiar use as the word denoting the actual concrete beginning of some operation. Thus *die Anlage eines Gartens zu machen* is not to draw out the plan of a garden on paper, but to make the footpaths and flower-beds.

Starting from the objective and visible fact, that organs appear as visible proliferations or buds of cells in definite regions, biologists in many cases have carried the idea back to the invisible and theoretical. Some have supposed that the hereditary mass of the fertilised egg-cell itself is a mosaic of definitely-placed though invisible primitive beginnings of organs, and that the visible beginnings of these organs, in the embryonic mass that results from the division of the egg-cell, are due to the presence in such areas of the

invisible beginnings handed on from the egg-cell. For those who, like Weismann, take an extremely preformationist view of development, the imagined number of definite but invisible beginnings present in the egg-cell is very great, and these beginnings themselves are arranged in ordered hierarchies. But even Oscar Hertwig and many of the supporters of more epigenetic theories of development allude to the presence of definite physical beginnings in the structure of the egg-cell. Now the word used by Germans for these imagined particles of the hereditary mass is also "Anlage."

There is wanted, then, an English word to express these two meanings, to include both the visible beginnings of organs and the beginnings of these as imagined before they become visible. Everyone who has had to translate, into English, German writings dealing with embryology or with biological theory has met with the difficulty of translating "Anlage" at a very early stage of his work; and even in English writing which is not based on German originals the difficulty of expressing the idea has to be faced. In the search for an English bearer of the idea one must remember first that the idea is a concrete fact, not an abstract conception: what we want to express is a definite budding or proliferation of cells on the one hand; on the other, a definite particle of nuclear matter that in suitable conditions is able to control or cause the formation of the definite bud or proliferation of cells. English writers have made many different attempts to get hold of a suitable word. The first and most obvious word is "rudiment." But, as Professor Edward Mark has already pointed out in a preface to his translation of Oscar Hertwig's "Text-Book of Embryology," rudiment is quite unsuitable, because, both in English and German, precise writers have used it in the sense of an organ or structure which has been at one time more complicated that it is at present. But a reason that seems to be still more against its use as a rendering of the ideas connected with "Anlage" is that we use rudiment most commonly as a term denoting structures in adult organisms, whereas "Anlage" is invariably, or almost invariably, connected with embryonic structures. Such words as "origin," "beginning," and so forth, fail to express the concreteness of "Anlage," the fact that it refers almost invariably to objects that are either actually visible, or that have a corporeal existence although invisible to us. Occasionally, but very rarely, Weismann uses the word in a sense that has led Professor W. N. Parker to render it by "predisposition"; but Weismann's use of it in this sense is associated with a tendency of his, especially noticable in "The Germ-Plasm," to glide rather easily from the abstract to the concrete; in at least some of the cases where I have compared the translation with the original, it seems to me that his meaning would have been more clearly, if less invitingly, expressed by using a concrete English term for a word that in German is abstract only in a metaphorical sense.

"Basis" and "foundation" also are used by some writers; but these are the renderings in English of the German word "Grundlage," and express the structure upon which a superstructure is "reared," not the first stage of the superstructure itself. The word "forecast" has been suggested by several writers, among others by Mr. E. A. Minchin. It would have been a word very suitable in the cruder days of the theory of preformation; in these days it seems to imply too close a connection in form between the "Anlage" and the organ arising from the "Anlage." Professor Mark, who has discussed the difficulties of the matter, employs the word "fundament"; this is a neologism that few would have occasion to resent. I have myself employed the word "incipium," but that offends Latinity, and fundament, incipium, and primary constitutent alike fail to convey the definite idea that "Anlage" is a growing structure in a growing organism, or, when it is used of nuclear particles, a structure of which a chief property is to grow or cause growth. The purpose of the present note is to suggest as an English equivalent of "Anlage" a word that would be intelligible to all English and German and French biologists, and that would be understood easily by any educated reader. The word I propose is "blast," a bud or point of growth. The existing meanings of "blast" could cause no confusion, and the word would indicate equally well the visible beginnings of organs in the form of cells or cell-groups, and the hypothetical precursors of these as parts of the hereditary mass. The obvious existing precedent for the use of the word is the common acceptation of the terms "epiblast," "mesoblast," and "hypoblast" as indicating the three great groups of cells that are formed in the development of all the higher animals.

It may be objected to the term "blast" that it would be inconvenient, as the epiblast, hypoblast, and mesoblast contain the "Anlagen" or "blasts" of many organs and structures. But in this hypothetical objection there is contained what seems to me a possible advantage in the recognition of the term "blast" in the sense I suggest. The days are gone by for embryologists to see in the layers of cells known as the epiblast, hypoblast, and mesoblast simple structures of a homogeneous and equivalent nature throughout the animal groups, and there are many who would hesitate before the suggestion of homology between these groups, except in very closely-allied animals. In the case of the mesoblast, it has been recognised almost since the inception of the term that the group of cells is a composite group, containing derivatives from many sources, and containing different sets of derivatives in different animals. For a layer or group of cells that, in the vertebrates, includes a set of primitive structures, "Anlagen" or "blasts" so varied as the primitive segments, the epithelium of the cœlom, the sexual cells, the epithelium of the sexual ducts and kidneys, and the beginnings of the connective tissues, blood-vessels, and blood, the term "mesoblasts,"

"middle blasts," would be more logical and not less intelligible than the single term "mesoblast," with its misleading suggestion of homogeneity. In the case of the other layers, the compositeness of the groups of cells is less obvious, and equally there is less reason to suppose that there could arise any confusion were they to retain the simple designations "epiblast" and "hypoblast," while the word "blast" was used by itself in the sense I now suggest for it.

P. CHALMERS MITCHELL.

VI.

A Portable Zoological Station in Bohemia.[1]

SINCE the establishment of the commission for investigating the Natural History of Bohemia in 1864, it has ever been one of its chief objects to study the life of the lakes, ponds, and streams. So early as 1871 Dr. Anton Fritsch began to publish some of the results of his work in this direction in the *Archiv* of the commission; and since that date much valuable research has been accomplished both by Dr. Fritsch and other authors. For nearly twenty years, however, the task could only be undertaken in a very unsystematic and spasmodic manner. There was no provision for making observations on fresh material on the spot. Dissections and microscopical work could not well be done in the open air, and it was impossible always to find a convenient and quiet cottage near the various pieces of water where the naturalist could temporarily take up his abode. Moreover, it was difficult to extend the period of collecting at any one spot, and to examine the fauna and flora under varying conditions. It therefore appeared to Dr. Fritsch, who was foremost in directing the undertaking, that a small movable laboratory might be constructed at very little cost to obviate all difficulties; and by the summer of 1888 a convenient portable hut was eventually obtained. Thus was inaugurated on a small scale the systematic study of the fauna and flora of the European lakes, which has since been carried on more exhaustively elsewhere, and which led to the establishment of the well-known station on the Lake of Plön (Holstein), opened by Dr. Zacharias in 1892.

The Dutch had already constructed a small portable station for the study of the life on the coast of Holland, and Dr. Fritsch arranged his building on much the same plan. A view of the exterior is given in Fig. 1, and of the interior in Fig. 2. The structure is of wood, consists of about 80 parts, and has a total weight of 1,000 kilogrammes. It can be erected in two hours and a half, pulled down in one hour and a half. The floor space is 12 square metres, and on

[1] Untersuchungen über die Fauna der Gewässer Böhmens.—IV. Die Thierwelt des Unterpocernitzer und Gatterschlager Teiches, als Resultat der Arbeiten an der übertragbaren Zoologischen Station. By Professor Dr. Anton Fritsch and Dr. V. Vavra. *Archiv. d. naturw. Landesdurchforschung von Böhmen*, vol. ix., no. 2, 1893 (1894). [This work is well illustrated, and we are indebted to the authors for the loan of the four photographs here reproduced.]

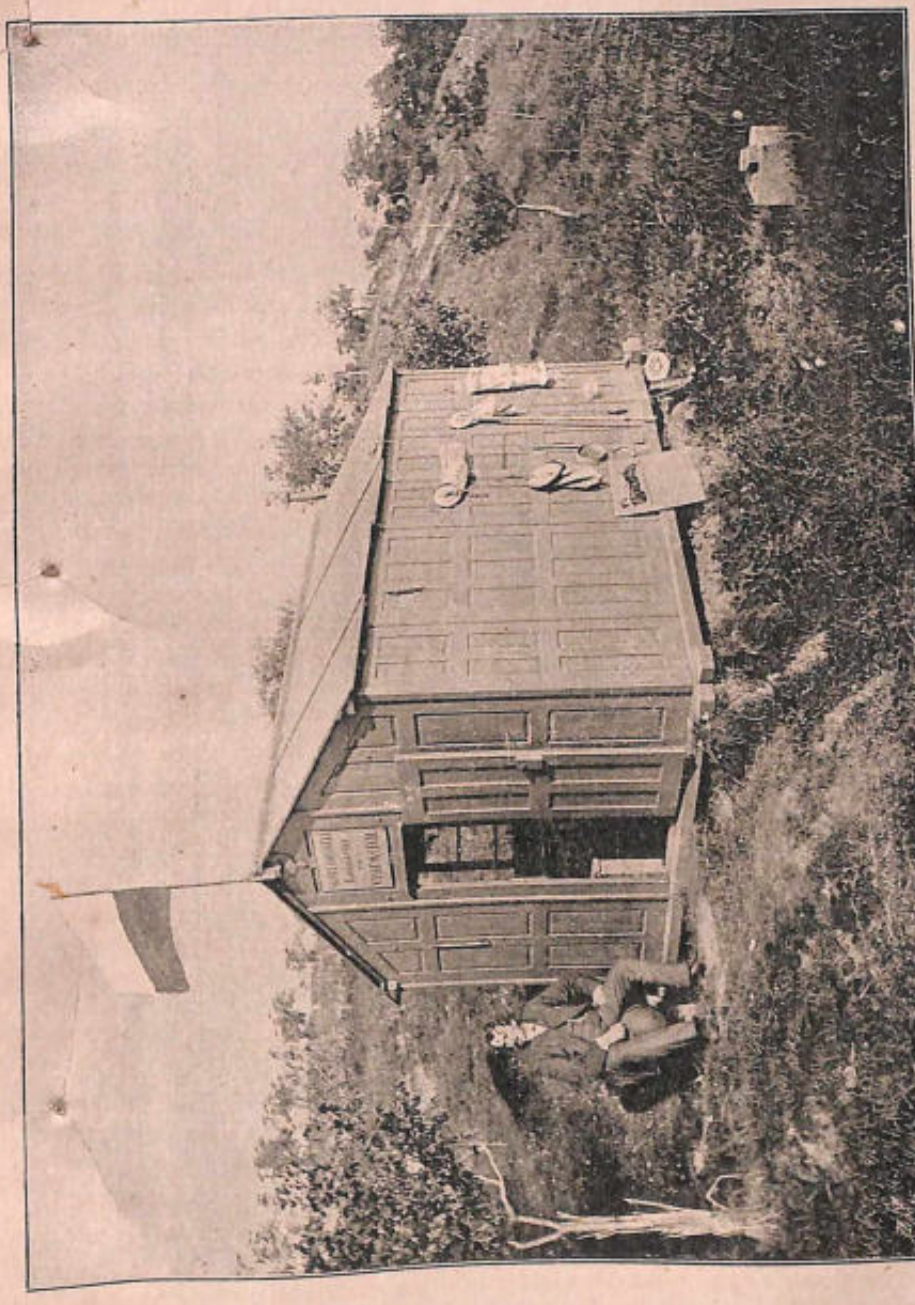


FIG. 1.—The portable Zoological Station of the Commission for investigating the Natural History of Bohemia.

the northern side there are two windows, of which the shutters fall inwards and form two large work-tables (Fig. 2). At night, or whenever required, these shutters can be turned upwards and the apartment readily converted into a sleeping room. The walls are conveniently fitted with shelves and hooks, and there is a small stove for warming.

The fishing apparatus is shown in the third photograph (Fig. 3). In this figure, no. 1 represents the net for surface-captures, made of thick silk, with two rings of 20 cm. diameter (one brass, the other cane), and one ring of 10 cm. diameter fixed to the neck of the bottle. No. 2 is a net on a bamboo rod for shore fishing and for depths of



FIG. 2.—View of the Interior of the Bohemian portable Zoological Station.

one metre. No. 3 represents a deep net with a simple arrangement for closing at any given moment. It consists of two parts 30 cm. in length, with an inner conical part leading to the bottle. No. 4 is a net for dredging mud. Nos. 5 to 8 are respectively a measure, thermometer, and two metal sieves for washing mud. No. 9 is a weight used in measuring depths, and no. 10 is a holder for glasses. Nos. 11 to 13 will be recognised as an aquarium and water-cans, while no. 14 is a plan of a lake. A common small hand-net is also used, and there is a light boat for the service of the station, but these are not shown in the illustrations.

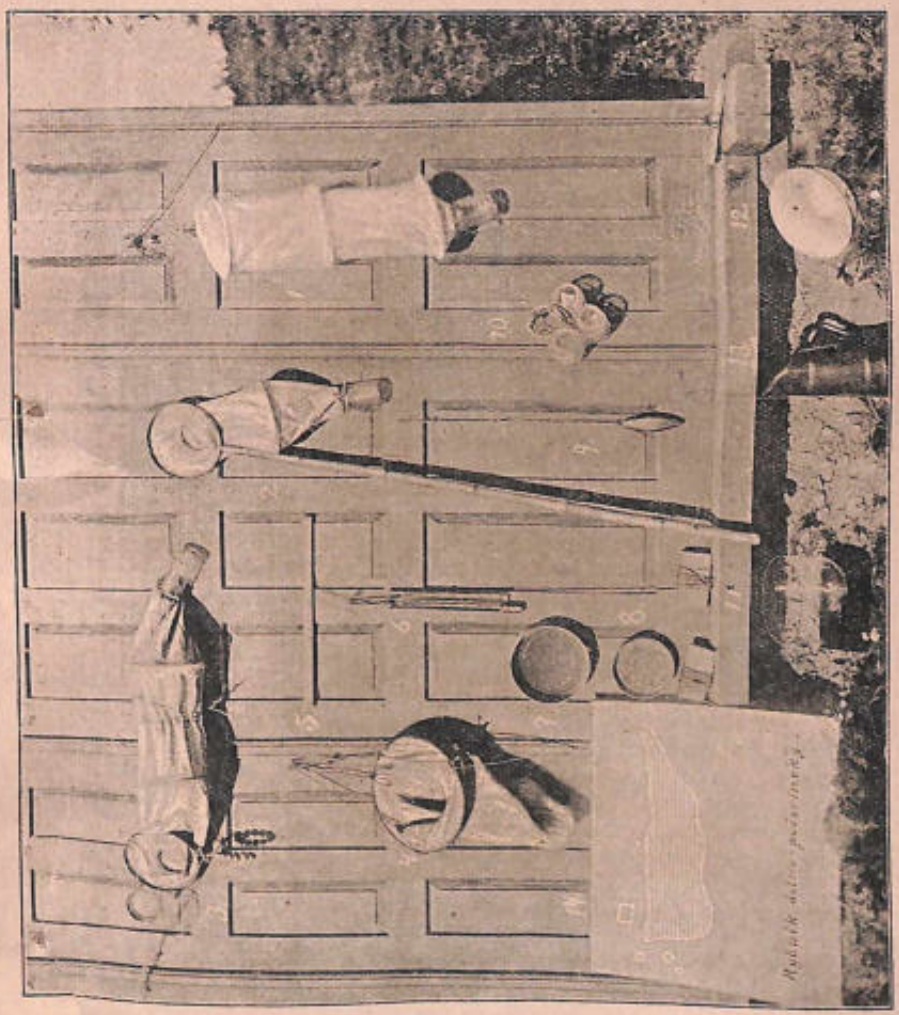


FIG. 3.—Apparatus for investigating the Freshwater Fauna of Bohemia.

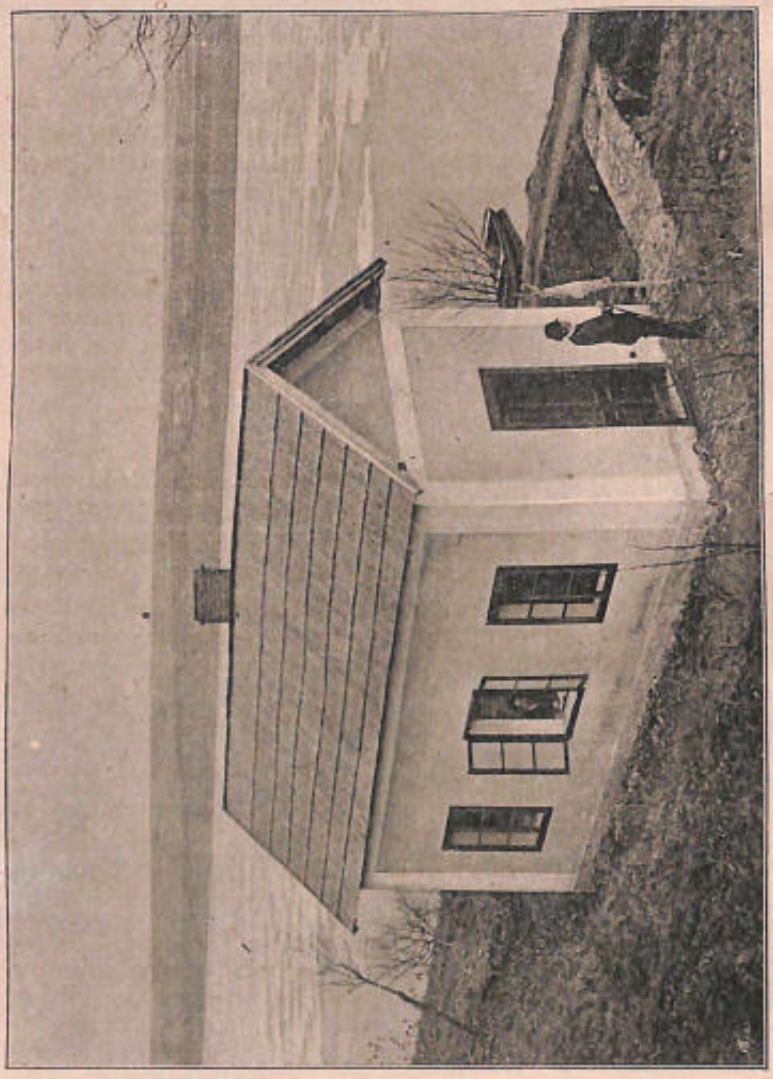


FIG. 4.—The permanent Zoological Station established by Baron Bela Dercsenyi on Lake Unterpocernitz, Bohemia

Thus equipped, Dr. Fritsch's portable Zoological Station has done good service; and when it appeared necessary to remove it to the south of Bohemia in 1892, the incomplete researches on the Unterpocernitz Lake were regarded as so promising that the local landowner, Baron Bela Dercsenyl, was induced to erect a permanent station on its shore on a somewhat enlarged scale. A photograph of the building due to the baron's generosity is reproduced in Fig. 4. It was handed over to the Bohemian Commission in July, 1892, and a large part of the report on which this article is based is occupied with the results of work accomplished here. In the permanent station there is a small second chamber which can be used for meals and sleeping accommodation; but the laboratory itself is arranged much as in the portable hut already described.

Progress in researches of the nature undertaken in these stations is necessarily slow, and the time has scarcely arrived to utilise the results in general deductions. Drs. Fritsch and Vavra, in the report now before us, are content to present a well-illustrated synopsis of the fauna and flora of the Unterpocernitz and Gatterschlag Lakes, merely alluding to the wider problems which remain to be solved. In the systematic list there are notes on the varieties of each species, its time of appearance, and normal circumstances of growth; and many of the illustrations are original. We congratulate the naturalists of Bohemia on their enterprise, and heartily wish them the success they deserve. We only regret that we cannot yet chronicle any similar work in our own more highly-favoured land. But one can hardly hope for any extension of our energies to freshwater, while the study of marine biology remains in the unpromising state to which we allude in our "Notes and Comments" for this month.

SOME NEW BOOKS.

The Agency of External Influences in Development.

Aeussere Einfluesse als Entwicklungsreize. Von August Weismann, Professor in Freiburg i. Br. Jena: Verlag von Gustav Fischer, 1894.

The Effect of External Influences on Development. The Romanes Lecture, 1894. By August Weismann, M.D., Ph.D., D.C.L., Professor in the University of Freiburg in Breisgau, with annotations by the Author. London: Henry Frowde, Amen Corner, E.C. Oxford: Clarendon Press Depository, 1894. Price 2s.

We have before us Dr. Weismann's lecture, published by the Clarendon Press, and purchased by ourselves, and the German edition sent us with the usual courtesy of the German publisher. The preface in the two editions is slightly different. Both contain a kindly and appreciative reference to the late Professor Romanes. The German preface calls attention to an additional note added at the end of the German text, and dealing with some of the matter contained in Hertwig's "Preformation or Epigenesis," an account of which has recently been published in Natural Science.

The German text is the original from which the translation was made by Mr. Gregg Wilson. It certainly serves to show that Professor Weismann is not so oblivious of the agency of external influences in development as Oscar Hertwig implies in his recent criticism. It deals, not with the whole field suggested by the title, but specially with the agency of certain external influences that call into activity varied constituent parts in the process of development. It is, then, in no measure a withdrawal from the preformationist position. It is an argument to show that many external influences that have been supposed to affect the process of development as direct causes in the epigenetic sense, that in fact have been supposed directly to stamp certain characters upon organisms, stamp these characters not directly but by calling into activity latent but existing constituents of the germ-plasm. As was explained in his book, Weismann believes that in the germ-plasm that gives rise to the organism there are present many sets of parts, each of them capable of producing the whole organism. In the actual development, when the germ-plasm is disintegrated into the separate parts or determinants which control the production of the separate variable parts of the animal, for each separate independently variable part there are many determinants present, and hence a struggle ensues between these as to which of them shall acquire the mastery and guide the growth of the organ in question. When the environment seems to have an important direct effect, Weismann believes that it is, so to speak, merely interposing in the struggle between the determinants, egging on some of them that otherwise

would have been vanquished or might have remained dormant, and preventing others from acquiring the control that otherwise would have fallen to their lot.

He expounds and enlarges Roux's theory of intra-selection. If an individual development were predetermined in every detail, it could no more produce an organism fit for life than, as Roux has aptly put it, "would a commander be victorious, who, instead of giving general instructions to his chief officers as to the placing and movement of their troops, should in advance issue detailed orders for the conduct of everyone down to the lieutenants, or even to each private soldier. The influences which encounter organisms during their development are never exactly similar, and to adapt themselves to these, the organisms must have a certain amount of freedom." The necessary freedom is given by intra-selection, for that "effects the special adaptation of the tissues to special conditions of development in each individual."

Taking the special case of colonial insects, in which there are males, females, and neuters, he elaborates an argument to show that the direct effects of poor nutrition are not sufficient to account for the differences between neuters and females, but that "the poor nourishment acts as the stimulus for the latent primary constituents for the workers in the germ-plasm, not only for those in the ovary, but also for those of all characters by which the worker is distinguished from the queen."

The origin of these materials on which intra-selection and the stimulus of external agencies act, Weismann refers to variations in the germ-plasm.

The Development of the Idea of Evolution.

From the Greeks to Darwin: An Outline of the Development of the Evolution Idea. By Henry Fairfield Osborn, Sc.D. 8vo. Pp. 260. New York and London: Macmillan & Co., 1894. Price 9s. nett.

The Columbia University of New York has decided to issue a series of small volumes on Biology, under the editorship of Dr. Osborn, the Da Costa Professor of Biology. It is not to be a library of elementary text-books in the ordinary sense of the term, but to consist chiefly of works dealing with certain fundamental problems which will concern students who have passed beyond preliminaries. With this aim it is therefore appropriate that the first volume of the series should relate to the history of the philosophy of the subject. We are glad now to welcome such a work from the pen of Professor Osborn, who has industriously compiled a most interesting and useful treatise. It is based partly upon a course of lectures delivered at Princeton in 1890, partly upon another course delivered last year in New York.

Professor Osborn's treatment of the subject, though largely following in the lines of previous authors, differs from preceding attempts in exhibiting more continuity. He shows more clearly than has hitherto been done that the growth of the idea of evolution has been continuous from the efforts of the earliest Greeks to the views of the present day. He carefully follows the broad idea of Evolution as a natural law, and traces back the birth and development of each of its parts, while constantly keeping in mind the changing environment of knowledge and prejudice. As a treatise for the advanced student before entering upon special reading and

research, it would indeed be difficult to imagine one more fairly balanced; and the Professor has been singularly successful in emphasising the salient points.

Evolution, as a natural explanation of the origin of the higher forms of life, succeeded the old mythology in Greece, and first developed from the teachings of Thales and Anaximander (B.C. 611–547) into those of Aristotle. This great philosopher had a general conception of the origin of higher species by descent from lower species, and he even stated the theory of the Survival of the Fittest, though rejecting it as an explanation of the evolution of adaptive structures. He also believed that there was no fortuity in Evolution, but that the succession of forms of life was due to the action of an internal perfecting principle originally implanted by the Divine Intelligence. In short, the ancient Greeks, with Aristotle as their most brilliant genius, had already discussed the problem of life from three points of view — had enquired whether Intelligent Design is constantly operating in Nature, whether Nature is under the operation of natural causes originally implanted by this mysterious Design, or whether it is governed by natural causes exclusively due to the laws of chance from the very beginning.

During the long Middle Ages the theory of Evolution made no advance, and even in the end retrogressed. Subsequently the rigid conceptions and definitions of species developed in the rapid rise of systematic Zoology and Botany were grafted upon the Mosaic account of the Creation, establishing a Special Creation theory for the origin of each species. Later still, when it was discovered from Palæontology that species of different kinds had succeeded each other in time, the "Special Creation" theory was again remodelled to cover a succession of creations extending down almost to the present day. Thus an ecclesiastical dogma developed into a pseudo-scientific theory full of inconsistencies, but stoutly maintained at the time by leading zoologists and botanists.

In the seventeenth and eighteenth centuries three classes of writers contributed more or less directly to the foundation of the modern study of Evolution. First, there were the industrious naturalists collecting facts; secondly, the speculators who still held many of the false Greek notions; and thirdly, the great Natural Philosophers, of whom Bacon, Descartes, and Leibnitz are examples. Of these it is curious to note that the third class did most towards facilitating modern progress. They clearly perceived that the point to which observations should be directed was not the past but the present mutability of species, and further, that this mutability was simply the variation of individuals on an extended scale. In short, these philosophers recognised the gradations of type, the facts of variation, and the bearing of these facts on the production of new species; they even noted the analogy between the artificial selection practised by man in producing new forms of plants and domestic animals and the production of new forms in Nature. At the same time, it must be added that the Evolution of Life was only made a very subsidiary element in the System of Philosophy they established.

Coming to later times, Professor Osborn deals very fully and in general fairness with the respective claims of modern evolutionists. His opinion of Treviranus is less high than that commonly held, and he attempts to assign a very definite position to Erasmus Darwin. The concluding section of the book, however, contains more familiar matter than the preceding chapters, and it is unnecessary here to

reproduce an outline of the history. We have merely tried to give a general idea of the scope of the volume, which will be welcomed by all who have not the time or opportunity to refer to the original works on which it is based. We need only add that those, too, who desire to study the subject more deeply will find the bibliography at the end a useful guide.

The Great Ice Age.

THE GREAT ICE AGE, AND ITS RELATION TO THE ANTIQUITY OF MAN. By James Geikie, LL.D., D.C.L., F.R.S., etc. Third edition. Largely rewritten. London: Edward Stanford, 1894. Price 25s.

THIS is a book which, in my view, ought not to have been written. When Mr. James Geikie, in 1873, published the first edition of his "Great Ice Age," it was welcomed by every geologist as a laborious, ingenious, and able argument in favour of a position which was widely held. It contained abundant proofs that its conclusions were based on a wide induction, and whether they were right or wrong, whether they were ephemeral or likely to live, it made no difference to sensible men, who know that most theories are tentative and that it is always a supreme advantage to have even an extravagant theory worked out by an able man.

When, after twenty years, Mr. Geikie brings out a new edition of his work, largely rewritten, we expect from him, not merely in his capacity as an experienced geologist, but as the occupant of an influential chair in the University of Edinburgh, and responsible for teaching many young men, that he shall have carefully read and discussed the various objections which have been urged against his facts and his conclusions, and that he shall either answer them or frankly say that fresh light has modified old views. Such a work would have been of great advantage to us all and would have enhanced Mr. Geikie's reputation where, I suppose, he chiefly wishes it to be enhanced, namely, among the younger geologists whose day of triumph is not yesterday or to-day, but to-morrow.

Instead of this, what have we? In the first place, a parade of authorities which is really most misleading. Names there are in abundance, some with reputations and some without, but they are virtually all on one side. Of the men quoted and the men whose views are alone considered, some were long ago committed to the same views, and others belong to an official school having an official creed to maintain. What use, therefore, is such a book to anybody? Who is likely to buy it, or read it, or to learn anything from it, except the unwary amateur, who will not realise that almost every position maintained by Mr. Geikie has been controverted, and that a large part of his arguments are nearly as obsolete as the geological science of De Saussure and De Luc?

Mr. Geikie declines to traverse the unwelcome literature where the history of the glacial theory is written. The consequence is that very many so-called discoveries chronicled by Mr. Geikie were made by men long ago, whose work ought to have been recognised.

There is also a great deal of modification of old views, and of climbing down. How much, only those will realise who take the pains to carefully compare the two editions; but instead of this being frankly and openly confessed, it is disguised under a specious form of rhetoric.

Thus Mr. Croll, who was the *Deus ex machinâ* of the first two editions, the inspired prophet who had solved everything, and against whom it was almost treason to argue, is treated very differently in

the present one. The chapter on the molecular theory of glacial motion is cancelled. This is peculiarly gratifying to me. For years I have been trying to champion Forbes' theory of glacial motion, and have written a great deal on the subject. I have always deemed it to be the pivot of any possible glacial theory. When Mr. Geikie brought out his previous edition he brushed Forbes' theory aside as impossible, and devoted many pages to a theory invented by Croll, which has been the amazement and amusement of many of us, namely, the molecular theory of ice motion. This is now thrown over without a word. Forbes' theory is completely accepted, and one of the old masters justified. So far, so good. And yet Croll was right. He wanted ice to do some wonderful things which a viscous body will not do. Forbes' ice—the ice we all know and can experiment upon—was no use to him. He had, therefore, to invent a special ice of his own. His scholar, Mr. Geikie, wants to do Croll's work with Forbes' tools. Assuredly, to change my metaphor, the old wine cannot exist long in the new bottles. Let us proceed, however. The chapters on cosmical changes of climate are now condensed and remitted to a short concluding chapter, with an ominous change in its heading—it is now entitled "Cause of the Glacial Period"—and to foot-notes full of ambiguous surrender. We are even told that if Mr. Croll's views happen to turn out to be wrong, still Mr. Geikie's geological conclusions will remain. All this is very curious—curious rather as a study of human nature than of science.

Let me next refer to one or two critical instances of Mr. Geikie's method of ignoring his opponents and critics. He devotes two considerable chapters to the vexed question of the origin of rock basins and mountain lakes. In this he repeats the kind of arguments which Ramsay long ago published, and he largely quotes from Mr. Wallace, who is a distinguished zoologist, but whose reputation in geology has still to be made. Will it be believed that not a word of any kind is said about the arguments on the other side, urged by the Duke of Argyll and Mr. Bonney, both ex-presidents of the Geological Society of London, nor of those of Mr. Spencer, who is in the very first rank of American geologists, which arguments have simply pulverised the whole theory of glacier erosion of such lakes? It is literally one of the most impertinent incidents in all my experience of literature, that Mr. Bonney's elaborate memoirs on this subject, directly aimed at Mr. Wallace and Mr. Geikie himself, should be completely ignored —Mr. Bonney, who has probably spent as many months in exploring the handiwork of ice in Switzerland and Norway as Mr. Geikie has spent days, and who has inductively analysed this particular problem with great sagacity and experience on the spot, where the lessons can be really learnt, and not by a process of deduction from impossible mechanical data.

In regard to the astronomical theories of an ice age as urged by Croll and Sir Robert Ball, we are treated to a *réchauffé* of old arguments which have been riddled through and through, not merely by geologists, but by mathematicians and astronomers like Mr. G. Darwin, Mr. Meach, in America, and, lastly, by Mr. Culvewell, who at the British Association at Oxford assuredly gave a final *quietus* to the whole subject in an analysis of its conditions, whose force was acknowledged by Sir R. Ball himself. Not a word is said of all this, but the ingenuous youth of Scotland is led to believe that the position is still intact, and that there is still something to be said for astronomical cycles causing climatic revolutions in the earth.

On the question of antarctic ice, Mr. Geikie, following Croll and ignoring all that has been written by Lord Kelvin and others, actually postulates a depth of 12 miles of ice as a probability. As if ice under such a pressure would not crush and liquefy and flow away, and as if the accumulated evidence of all the explorers available did not go to show that the antarctic ice has, in fact, a very moderate depth.

Next, take the question of Greenland and its contour, upon which a good deal depends. We are again treated to the story of Greenland being a mass of islands, most of them low, which are supposed to be covered with a stupendous mound of snow and ice. All the work of the Danish exploring expeditions in mapping out the Nunatakker, and going to show that the great island is in reality a high plateau, is ignored in favour of Mr. Geikie's old view. This is now supported by an *obiter dictum* of Mr. Nansen, who is not a geologist, and whose *dictum* is as conclusive as that of any other traveller who infers the depth of a deposit from the appearance of its surface. As a matter of fact, it is as easy to know what depth a snow mass is from walking over it as it is to know the depth of the ocean from sailing over it, or the contents of a pie from an inspection of its crust. Nor, again, in what is said about the glaciation of Greenland, is any reference made to the proofs recently afforded by Danish botanists that the flora of Greenland, instead of pointing to its recent emergence from much more glaciated conditions, points exactly the other way.

Again, take the question of a ground-moraine, the invention of an ingenious Frenchman, who had probably never seen a glacier, and the main buttress of many geologists who have never seen glaciers either. This particular postulate is used with the greatest confidence and assurance, as if it had ever been proved or even admitted. Mr. Bonney has, in this instance again, shown what a fantastic postulate it is as usually quoted. But it does not need Mr. Bonney's assurance. Anyone who has carefully examined glaciers at work in Switzerland and Norway can judge for himself as to this monstrous invention. Where is anything like the supposed ground-moraine being formed by ice now? How is it possible to explain its formation by any mechanical theory? Of course not. *Glacial streams* sometimes form a layer of rounded boulders, and sometimes, very locally, small beds of stratified sand; but how can we suppose that ice, whose qualities are pretty well-known, can, under the enormous pressure of 5,000 or 6,000 feet, actually move under its foot masses of 500 or 600 feet deep of soft beds (in many cases of sorted beds)? The same ice which, when it has moved scores of miles from its original home, is supposed to have such erosive power that it can scoop out great lakes, and such momentum that it can travel up-hill for hundreds of yards! yet this ice is not only to thus carry *under its foot* these beds, but to do so without breaking the shells sometimes present in it, and without kneading the assorted beds into a heterogeneous mass of boulders, clay, and sand. Assuredly the ice referred to is Saturnian ice and not Mundane. As assuredly the head of the Geological Survey of Scotland ought either to substantiate his postulate by some rather more reasonable arguments or leave it to the followers of Swift and of Rabelais.

Again, as to subglacial water. All kinds of things are postulated of it. For instance, while water running in rivers runs according to gravity, and follows valleys and watercourses in a rational way, subglacial waters are supposed to have been quite indifferent to the contour of the country, and to have gaily travelled over hill and dale,

sorting the clays from the sands, and arranging the eskers and drumlins in places where it is impossible to understand how any water could flow at all, and to have flowed, not in the form of streams but sheets of water. The position would be laughed out of court at once if applied to subaërial streams and rivers, but when they are buried under ice anything seems possible.

Lastly, the old Norwegian ice-sheet is trotted out, and nothing said of the arguments and facts collected by Pettersen and Kjerulf in Norway, and by Mattieu Williams and Bonney and others in England, showing that the whole thing is a dream.

I have merely quoted a few samples of the kind of geology which may be found in this book. They are only samples. Every chapter, and almost every paragraph, contains similar præ-Baconian logic, and if I had room I might go on for ever.

One word in conclusion, which is personal and necessary. It would, in fact, be impossible for me to sign this article without mentioning it. In referring to the works ignored by Mr. Geikie, I have preferred to refer to those of better men than myself; but I am bound to say that I cannot quite ignore myself. I have written two fat volumes and a great many papers on the subject chiefly in the *Geological Magazine*, and others, in *Nature*, both of them publications of high reputation. In them I have examined, with courtesy and with such pains as I was capable of using, nearly every position maintained by Mr. Geikie, and have had the advantage of the support of most responsible critics who have at great length reviewed my books in the best reviews, and of many distinguished private critics both physicists and geologists. I had a right, therefore, to expect that some attempt would have been made to meet my objections. It is no use in these days supposing that the democratic wheels of Science are going to spare the self-sufficient, and the self-engrossed, and the self-opinionated. Every man whose work is to live must face his critics with frankness and with pains, if the criticism is courteous and rational; and those who will not do so, but go on repeating old wives' tales that have been exploded long ago, will find that the tide has mercilessly swept away the sand-heaps they have built. With these unsubstantial buildings may also go, if we have no care, reputations that are based on something better than the truth of certain theories, namely, long and devoted labours like Mr. Geikie's in the field.

HENRY H. HOWORTH.

A RARE BOOK ON AMERICAN ZOOLOGY.

A REPRINT OF THE NORTH AMERICAN ZOOLOGY. By George Ord. Being an exact reproduction of the part originally compiled by Mr. Ord for Johnson and Warner, and first published by them in their Second American Edition of Guthrie's "Geography," in 1815. Taken from Mr. Ord's private, annotated copy. To which is added an appendix on the more important scientific and historic questions involved. By Samuel H. Rhoads. Published by the Editor, Haddonfield, New Jersey, 1890. Pp. x., title page, pp. 290-361, and appendix pp. 1-52, and "index to whole volume," pp. 53-90. With a portrait of Ord. Price $3.

IT is perhaps hardly an exaggeration to say that no person on this side of the Atlantic has ever seen Ord's "Zoology." The book is as well-known by name among mammalogists as Linnæus's "Systema Naturæ," on account of the endless controversies relative to the nomenclature of the grizzly bear. In all discussions on the relation-

ships and nomenclature of that animal, Ord's "Zoology" has been quoted. Baird cited a copy of the work in 1857 as "exceedingly rare," and Coues refers to an imperfect volume as "the only copy I ever handled." The history of the "Second American Edition" of Guthrie's "Geography," in which publication Ord's memoir appeared, is involved in obscurity. Both Baird and Coues used the copy in the possession of the Philadelphia Academy of Sciences, but that copy has mysteriously disappeared. The only other recorded copy has been unearthed by the diligence of Mr. Rhoads, who learned that Ord had presented a quantity of books and papers to the Philadelphia College of Physicians. On enquiring at the College, it was not found in the library, but information was forthcoming that Dr. J. Solis Cohen had purchased from the College a number of books that were not considered to come within the scope of the library. Dr. Cohen made a search among his books, and, to the joy of the enquirer, found not only a perfect copy of the work, but Ord's own copy containing marginal pencil notes on the zoological portion. Zoologists owe, therefore, a debt of gratitude to Mr. Rhoads for his painstaking search, and to Dr. Cohen for permitting the reprinting of so rare and interesting a treatise.

Owing to the worn and imperfect condition of the type, a photographic reprint was impracticable, but judging from the evident care and pains taken by Mr. Rhoads we feel no hesitation in accepting this as a faithful reprint of the "Zoology." Ord's name does not appear, and the editor of the "Grammar" says, "the modesty of its author forbids a personal acknowledgment, which the editor would have the highest satisfaction in making." But that Ord was the author there is absolutely no doubt, and to him belongs the credit of priority of nomenclature for numerous American animals.

The "Zoology" commences on p. 290 of the "Grammar," with a brief note to say that the author has arranged the orders and genera according to Turton's edition of Linnæus. Pp. 291, 292 are occupied with a list* of the animals, of which both the common and binomial designations are given, and it is on p. 291 that the name *Ursus horribilis* (Grizzly Bear) occurs. Pp. 293–313 are devoted to notes and observations on the mammalia given in the list, but no scientific descriptions are attempted. The list* of American birds occupies pp. 315–319, and the next thirty-six pages are devoted to notes and observations similar to those given on the mammals. A list* of amphibia occurs on p. 357, and after a few notes on these, the fish and remaining orders of animals are dismissed in about a page of text.

In the appendix which Mr. Rhoads has added to the work, he has been assisted by Dr. Coues, Mr. Witmer Stone, and Professor Cope, and the result appears to be an excellent and careful digest of the nomenclature of the animals quoted by Ord and a valuable addition to American zoological literature.

The new names proposed by Ord are here given; those printed in italics apparently hold good:—Vespertilio americanus; V. rubra; Viverra albus; Mustela hudsonius; *Ursus horribilis* (stands if specifically distinct); Mus *tuza*; M. *cinereus*; M. canadensis; M. *pennsylvanica*; Arctomys *ludoviciana*; A. *columbianus*; Sciurus *pennsylvanica*; S. hiemalis; S. carolinensis; S. labradorius; Antilope *americanus*; Ovis montanus; Vultur columbianus; Falco cæsius; Picus

* These lists (the only part of scientific value) did not occur in any other editions.

montanus; Phasianus *columbianus*; Tetrao *fusca*; Sterna *philadelphia*; *Larus delawarensis*; Anas *columbianus*. The reptiles given in "Amphibia" under new names are, Professor Cope says, "either synonyms or unidentifiable." Mr. Rhoads has published in the *American Naturalist*, June, 1894, pp. 523–526, a summary of changes in nomenclature proposed, and to this the critical reader is referred.

A few errata are carefully inserted on p. 52, and an index of ovving 4,000 entries enables ready reference to any names used either in Ord's work or Rhoads' appendix. C. D. S.

Deep-Sea Deposits.

Carte des Sédiments de Mer Profonde, avec notice explicative. By John Murray and A.-F. Renard. 8vo. Pp. 45 and map 43 by 25 cm. Bruxelles: Société Belge de Librairie (Société Anonyme), Oscar Schepens, Directeur, 16 rue Treurenberg. 1894. Price 2 fr. 50.

Those who are fortunate enough to possess the first number of Natural Science will find therein a long review of the "Challenger" Report on Deep-Sea Deposits. The pamphlet now before us is practically an epitome of the contents of that volume by its authors, and is published so as to bring within reach of all the main facts and results detailed in the Report.

The text appended to the present map, which appeared originally in the *Bulletin de la Société belge de géologie*, vol. viii., 1893, consists of a concise summary of the various types of sediment distinguished by the authors, exclusive of the micrographical and chemical details and theoretical considerations provided in the bulkier Report. It presents all the facts on which are based the construction of a submarine geological and mineralogical map.

The descriptions commence with the nature of the deposit (red-clay, blue-mud, globigerina ooze, radiolarian ooze, etc.), and follow with a general determination of the macroscopic characters of the deposit. The sediments were treated with hydrochloric acid, and both the soluble and organic contents are given. The residues, after washing, were sorted into three groups, which are given under the headings of Siliceous organisms, Minerals (particular minerals and rock fragments), and Amorphous matter.

The pamphlet concludes with a chapter on the geographical and bathymetrical distribution of deep-sea deposits. The map, which is clearly coloured, shows at a glance the depths of the oceanic areas in *brasses* (100 brasses = 183 mètres), the lines of depth being drawn at 100, 500, 1,000, 2,000, 3,000, 4,000, and deeper than 4,000 brasses; and the deposits are coloured and defined as muds and coral sand, globigerina ooze, diatomaceous ooze, radiolarian ooze, pteropod ooze, red clay, and blue muds.

The information about deep-sea deposits thus presented in so convenient a form cannot fail to be of the greatest service.

The Fisheries of the Mediterranean.

Les Pêcheries et les Poissons de la Méditerranée (Provence). By Paul Gourret. 16mo. Pp. 360 with 109 figures. Paris: J. B. Baillière & Sons, 1894. Price 4 fr.

This little work, by the sub-director of the Zoological Station at Marseilles, forms a volume of Messrs. Baillière's Library of Useful Knowledge. It is an interesting treatise on the fishes and fisheries of

the Mediterranean, well written and profusely illustrated, and likely to be of value, not only to the specialist, but also to the general reader desiring some authentic information on the subject of which it treats. The first part deals with the various fishing stations, giving an account of the configuration of the coast, the nature of the sea-bottom, the depth of the water, and the winds and currents. The second part mvies exclusively to appliances, is especially well illustrated, and occupies by far the greater portion of the book. The third and fourth parts explain the influence of surrounding conditions on the distribution and varying abundance of the different fishes of economic value, and detail the precautions necessary for the successful protection of the fishing industry in Southern France. The final chapter gives an admirable list of the fish-fauna, including the scientific and popular names of each species, together with its habitat, abundance or rarity, and modes of capture.

The Flora of Nebraska.

Flora of Nebraska. Edited by the Members of the Botanical Seminar of the University. Introduction; and Parts 1 and 2. Pp. 128, plates 36. Published by the Seminar, Lincoln, Nebraska, U.S.A. Price $1 per part.

The botanical Seminar of the University has undertaken the publication of a complete "Flora of Nebraska," to be issued in twenty-five parts by subscription at the rate of one dollar per part. Each part will be the work of a specialist, and will be in the nature of a monograph of the group under consideration. The parts first to appear will deal with groups of which the literature is so diffuse as to be inaccessible to all save those who have constant access to the botanical library of the University. These will also be the more profusely illustrated. This is a great point in the new publication, and results in the disposal of three-fifths of the whole work on the cellular plants, representing fifteen parts, while nine are devoted to the few vascular cryptogams and the flowering plants. Number twenty-five will contain a catalogue or check-list, and also a list of host-plants of fungi. The aim of the Seminar is most commendable, and we wish them success in their project; the "lower plants" generally receive but poor treatment in a flora, and their study in consequence is neglected. Parts 1 and 2, which have been issued together, augur well for the usefulness of the work; part 1 (Protophyta to Phycophyta) is by Alton Saunders; part 2 (Coleochætaceæ to Characeæ) by Albert F. Woods. Professor Bessey supplies an introduction to the whole. *A propos* of the introduction, the plan is adopted of replacing the familiar term *order* by *family*, while the former represents the *tribe* of Hooker and Bentham. For instance, the polypetalous dicotyledons are subdivided into the orders Thalamifloræ, Disciflorae, and Calyciflorae, the old cohorts becoming suborders, while the old natural orders are families. It would be a great advantage if systematists could agree on the terms to be applied to the various subdivisions; the want of a uniform code is very confusing to the student, who can never be quite clear what is meant to be conveyed by the chance mention of a given term.

The Geological Survey of the United Kingdom.[1]

The Report of the Director-General of the Geological Survey has recently been issued for the year ending December 31, 1893. This

[1] See Report of the Science and Art Department for 1893, published by H.M. Stationery Office.

shows an excellent record of work done, and due credit is given to the admirable weather which permitted a survey of 200 square miles in excess of that surveyed in the previous year.

Nine new sheets of the one-inch map of England and Wales were issued during the year in two editions (Solid and Drift), while forty-nine MS. coloured copies of the six-inch maps have been completed and deposited in the office for public reference.

In the record of field work we find that Mr. Fox-Strangways has revised the mapping of the Charnwood Forest area, and determined the overlap of the New Red Marl. The contact of the eruptive rocks with the altered strata has also been observed at Badden Wood and Garenden, while much detail has been ascertained of the topography of the land that sank beneath the Keuper Sea. Mr. Lamplugh has been engaged in studying the "Skiddaw Slates" of the Isle of Man, but has as yet not obtained any clear idea of the succession. He has obtained a few obscure organisms from the slates, but nothing of any palæontological value. The red sandstones to the north of Peel have, however, yielded more tangible evidence, and the corals found, though specifically indeterminable, seem to point to Upper Silurian or Devonian age.

In the Devonian area, the presence of *Entomides* near Tor Point establishes the presence of Upper Devonian rocks to the west of Plymouth Sound, while the Plymouth district has been brought into connection with those of Newton Abbott and Torquay.

Messrs. Strahan and Gibson have completed sheet 249 of the one-inch survey of the South Wales Coal-field, and sheets 232 and 263 are in progress. By tracing the outcrops of certain coal seams, Mr. Strahan has ascertained approximately what part of the Pennant Rock forms the surface of the ground of the anticlinal ridge which separates the Caerphilly and Llancaiach basins, information of considerable commercial importance.

The Permian and Trias areas are mainly under the observation of Mr. De Rance, who, among other matters, has made a detailed examination of the sections exposed in cutting the Manchester Ship Canal.

Some few new localities for Corallian fossils in the great Oxford Clay series of Huntingdonshire and Bedfordshire have been found by Mr. Cameron; while Mr. H. B. Woodward and Mr. Strahan have examined and photographed the Jurassic beds exposed in the Dorsetshire cliffs between Swanage and Weymouth for the purpose of preparing a detailed section.

The subdivisions of the Chalk have been studied by Messrs. Hawkins and Jukes-Browne, and much interesting information has been obtained on these points. The Chalk Rock of Dorset varies much lithologically, and at Winterbourne Abbas two beds of hard brecciated rock were found to be separated by five or six feet of softer glauconitic chalk containing large grains of quartz. The brecciated material contains small fragments of fine-grained green sand, which, as Barrois has noted, correspond with varieties of Upper Greensand, indicating possibly that the Upper Greensand was undergoing erosion not far from this place during the formation of the Chalk Rock.

In the Tertiary deposits the true position of the Ramsdell Clay has been cleared up, and Mr. Blake shows that one part belongs to the London Clay and the other to the Bagshots. Mr. Clement Reid continues his work on the Eocene deposits of

Hampshire, and some interesting notes are promised by the Director-General in his next Report.

The study of the Drift series has made steady progress, some 2,711 miles of boundary having been traced, mainly in East Cheshire, South Wales, and the Isle of Man. Mr. De Rance has noted the character and position of some 300 boulders, from two to twelve feet in length, in East Cheshire, and the information obtained has been supplied to the British Association Committee on Erratic Blocks.

In the Petrographical laboratory the main point of interest is the determination by Mr. Teall of the mineral Riebeckite as a normal constituent of the granophyre of Meall Dearg.

In Scotland, progress has been made in the mapping of Sutherland and Skye, Caithness, Aberdeenshire, and Banff, while important work has been accomplished in Kincardineshire and Forfar, Arran and Cantyre. Well-marked fossils have been determined in the shaly Cambrian series overlying the quartzites of the N.W. Highlands, which have allowed the positive fixation of the true Cambrian age of these beds. From the locality of Meal Gubhais, near Loch Maree, *Acrothele cadusia* and fragments of *Olenellus* rewarded the diligent search of Messrs. Horne, Gunn, and Clough. A collector, Mr. Maconochie, was accordingly directed to search the spot and a large series of specimens was obtained, and is now being worked out by Mr. Horne.

Mr. H. B. Woodward has completed his survey of the Jurassic rocks, with their associated Tertiary Igneous rocks, of Raasay, and a detailed section from below the Lias to the Great Oolite has now been obtained. It is interesting to note that the quartzite summits of the region north of Strathcarron retain their ice-worn character still so fresh that their polished surfaces, plunging steeply down the mountain sides, can hardly be walked upon. When wet, these glistening sheets of rock shine like polished marble, and show the fine striation impressed on them by the ice-sheet. Each larger valley among these mountains has its lateral and terminal moraines marking the positions of the later glaciers.

In Ireland the revision of the Metamorphic areas has been steadily progressing. Mr. Kilroe finds no break between the Croagh Patrick quartzite and the Llandovery strata, and it is therefore difficult to resist the conclusion that it must be of Upper Silurian age. But on this point the Director-General advises caution until the ground to the south has been exhaustively examined. In Palæontology, Professor Sollas has determined the existence of a new fossil (*Pucksia machenryi*) in a narrow band of slate, which had previously yielded *Oldhamia* and some spherical bodies resembling Radiolaria.

The Report concludes with some notes on improvements and accessions to the Museum of Economic Geology, and a return of work done by the individual surveyors attached to the Geological Survey.

THE half-yearly number of *Timehri* (the journal of the Royal Agricultural and Commercial Society of British Guiana), just received (vol. viii., pt. i.), contains several interesting and readable articles. The first, on Guiana Orchids, is by the editor, Mr. James Rodway, whose articles on the Guiana forest are known to readers of NATURAL SCIENCE. Valuable hints are given on cultivation in the tropics, for orchid houses seem as necessary in Georgetown as in a European

climate. Only in the wind-protected, saturated atmosphere of the forest are most of the species at home. A paper by Mr. Llewellyn Jones, entitled "A Few Popular Facts About Diffusion," will interest sugar growers; as will also "Some Enemies of our Canefields," and "Cost of Sugar Production in British Guiana."

THE annual report of the Missouri Botanical Garden, which is very nicely got up, and is illustrated with 41 plates, contains reports for the past year, anniversary publications, and scientific papers. The anniversary publications are the fourth annual flower sermon, to defray which there is a bequest of $200, and the proceedings at the fourth annual banquet of the trustees ($969·55). Would not an abstract of the sermon have expressed all that was necessary? Of the scientific papers several have been issued in advance as "reprints," namely, "A Study of the Venation of the Species of Salix" (Glatfelter); "Bibliography of the Tannoids" (Bay); "Sugar Maples and Maples in Winter" (Trelease); "North American Species of *Gayophytum* and *Boisduvalia*" (Trelease). The others include Phenological Notes for 1892 and 1893, and a note on the emergence of *Pronuba* from the Yucca Capsules, by J. C. Whitten, and a list of plants collected in South-eastern Missouri. Among the plates, which are a marked feature of the book, are several very nice views in the gardens.

WE have received a useful little pamphlet of twenty-four pages entitled "Brief Notes on the Physical and Chemical Properties of Soils," by R. Warington, F.R.S. (London: Chapman & Hall, 1894. Price 1s.). The subject is so slightly treated in most English text-books that it is intended to supply a temporary want, enabling teachers of classes in agriculture to treat the properties of soils more exhaustively than usual.

WE have received nos. 1—4 of *Nederlandsch Koloniaal Centraalblad*, a publication devoted to the scientific literature of the Netherland Indies. Dr. Boerlage edits Botany; Dr. Max Weber, Zoology; Dr. Wichmann, Geology; F. Blumentritt, the Philippines; Dr. Van den Burg, Medicine; H. F. C. ten Kate, Anthropology; while C. M. Pleyte Wzn is the general editor. The publication, if kept up-to-date, cannot fail to be of the highest importance to dwellers in Malaya. The journal is published by Brill, of Leiden, and costs 2 fl. 50 per annum.

WE have also received "Rainmaking and Sunshine," by John Collinson (London: Swan Sonnenschein, 1894. Price 3s. 6d.). The work scarcely falls within the domain of NATURAL SCIENCE, and we are impressed with the appropriateness of the author's motto:—"Man's knowledge is as a rivulet; his ignorance as the sea."

MESSRS. DULAU & Co. have issued part xxxv. of their useful Catalogue of Zoological and Palæontological Works. More than fifty pages are occupied with a list of works relating to Anthropology and Ethnography.

OBITUARY.

WILLIAM TOPLEY, F.R.S.

BORN 1841. DIED 1894.

THE sad news of the death of William Topley has come as a surprise to his many friends. He had attended the Zurich meeting of the International Congress, and, full of interest and energy, he had proceeded to Algiers. There he was seized with illness, and on returning to his home at Croydon, it was found that gastritis had developed, a disease from which he never recovered. Mr. Topley, who was born at Greenwich, received his scientific training at the Royal School of Mines. In 1862 he was appointed an Assistant-Geologist on the Geological Survey of Great Britain, and six years later was promoted to the rank of geologist. Commencing field-work in the Wealden area, he became specially interested in the Cretaceous and Neocomian strata and in the problems connected with the sculpture of the scenery. His important memoir on the Geology of the Weald, and the paper (written jointly with Dr. Le Neve Foster) on the Medway Gravels and Denudation of the Weald, were results of this early work. In the meanwhile, and before the Wealden memoir was written, Mr. Topley was transferred to the far north of England, to take part in the survey of the coal-field of Durham and Northumberland. Later on, in 1880, he was appointed to superintend the publication of maps and memoirs at the Geological Survey office in Jermyn Street, and on the retirement of Mr. Best, in 1893, Mr. Topley took entire charge of the office.

Always full of work, and seemingly untiring in energy, yet he never published very much. The effects of denudation on anticlines and synclines, and the intrusive character of the Whin Sill, were topics he discussed from his experience in the north. He took an active part in the publication of the *Geological Record*, fulfilling for a time the arduous position of editor. He served for many years as one of the secretaries of Section C. at the British Association, and prepared for that body reports on Gold, on Coast Erosion, and on National Geological Surveys. He was one of the secretaries of the

International Geological Congress in 1888, when the meeting was held in London. Again, whether serving on the councils of the Geological Society or of the Geologists' Association, his services were in frequent request. He filled the office of President to the Association in 1885–86, and on many an occasion he acted as guide during the excursions of the members. Among these societies, as on the Geological Survey, his loss will for long be keenly felt, for he was ever ready with help and information. In the economic applications of geology, Mr. Topley was especially interested. The subject of the Channel Tunnel, the sub-Wealden Boring, and the underground distribution of Palæozoic rocks in the south-east of England engaged his attention. So also in questions of water-supply and of the occurrence of petroleum he was a recognised authority; and with regard to the water-supply of the metropolis he gave important evidence which was published by the late Royal Commission appointed to deal with the question.

Mr. Topley was Examiner in Geology to the Durham University and to the Science and Art Department. He was elected a Fellow of the Royal Society in 1888.

GUSTAVE HONORÉ COTTEAU.

BORN 1828. DIED AUGUST 10, 1894.

WE deeply regret to have to record the death of the oldest and most respected member of the School of Systematic Echinologists—Gustave Honoré Cotteau. Mr. Cotteau was a judge in the Department of l'Yonne. In the intervals of leisure of a long and busy life, he was able to study the fossil Sea Urchins, and to describe probably more new species than any of his contemporaries. Fossil Echinoids from all parts of the world were sent to him at Auxerre to be determined and described. He took up the work of Desor when the author of the great "Synopsis des Echinides fossiles" passed away. For over 30 years he has been the greatest authority on his group. His work has been characterised by the most exemplary care; his diagnoses are models of close and detailed description. Some of the younger palæontologists may have thought he was inclined to attach more value than was necessary to trifling variations, but his determinations were always accepted with the most implicit faith owing to the care he bestowed upon his specimens and the love he put into his work. The list of his memoirs and papers is a very lengthy one, and among them it is difficult to pick out any for special mention. They are all so good. He was a Member of the French Institut, a Foreign Member of the Geological Society, and an honorary member of numerous societies who honoured themselves by honouring him.

THE death is also announced of DR. PAUL ALBRECHT, lately of Hamburg, formerly of Brussels. He made numerous small contributions to Comparative Anatomy, and a few — such as his discovery of the pro-atlas in the vertebrate skeleton — are of importance.

BOTANISTS, among others, will regret the death of General ROBSON BENSON, who did some excellent work in connection with the Botanic Gardens at Rangoon and Madras.

PROFESSOR PRINGSHEIM of Berlin was a botanist of eminence, who has also recently passed away.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

MR. C. L. GRIESBACH has been appointed the successor to the Directorship of the Geological Survey of India, in the room of Dr. William King, retired.

MR. CHARLES L. EDWARDS, lately of the University of Texas, has been elected Professor of Biology in the University of Cincinnati, Ohio.

THE fine collection of fossil Echinoids that belonged to the late Gustave Cotteau has been presented to the Ecole des Mines, Paris.

ACCORDING to the *American Naturalist*, the American Museum of Natural History has organised an expedition, under the direction of Professor Rudolph Weber, to make collections in, and a scientific exploration of, the Island of Sumatra.

MR. W. HOLMAN HUNT has been appointed the Romanes Lecturer for 1895. Though Mr. Holman Hunt is best known in the regions of pure art, his presidential address to the Sunday Society, quoted in NATURAL SCIENCE, vol. v., p. 15, showed him to have a true sympathy with science.

DR. JAMES E. TALMAGE has been appointed to a newly-founded Professorship of Geology in the University of Utah. The chair has been endowed with a sum of $60,000 by the Salt Lake Literary and Scientific Association, and the Deseret Museum, belonging to this Association, has been placed at the disposal of the Un.versity.

AT the annual conversazione of the Chester Society of Natural Science on October 8, the Kingsley Medal was awarded to the President, Dr. Henry Dobie, whose first contribution to science was published so long ago as 1849, and related to the minute structure of voluntary muscular fibre. In 1850 and 1851, Dr. Dobie published some observations on the cilia in the sponge *Grantia*, and he has subsequently made contributions to our knowledge of the rotifers.

THE cases for the Barrande Collection of Lower Palæozoic Fossils in the Royal Bohemian Museum, Prague, are now fixed, and more than 20,000 specimens are already mounted for exhibition. Dr. Antoń Fritsch, the director, estimates that the arrangement of the entire collection will occupy nearly a year. The Barrande Fund is at present being devoted to the publication of a memoir on Graptolites, by Dr. Perner.

THE Museum of La Plata will shortly issue the second part of Mr. Lydekker's memoir on the Fossil Vertebrata of the Argentine Republic. It will be illustrated by over sixty folio plates, and deal chiefly with the Edentate Mammals. Mr. Lydekker returned to England last month.

We are pleased to learn that the Essex Field Club's petition with respect to the formation of an Epping Forest Museum has been granted by the Conservators, and active work will be commenced as soon as certain legal formalities have been complied with. Meanwhile, contributions of suitable specimens, and offers of loans of same, both for the Forest Museum and for the Central Museum at Chelmsford, will be gratefully received by the hon. secretaries, Messrs. W. and B. G. Cole, Buckhurst Hill, Essex.

The Norwich Castle Museum was opened by the Duke and Duchess of York on October 23. Much progress had already been made in the arrangement of the collections, and the general public were freely admitted three days afterwards. The *Norwich Mercury* celebrated the event by issuing an admirably illustrated supplement on October 20, descriptive of the transformation of the old prison into a museum. An earlier description of the new institution, by Dr. Henry Woodward, appeared in Natural Science for November, 1892.

Mr. Sauvinet, an assistant at the Natural History Museum of Paris, left on October 5 for Mecheria, in the Province of Oran, Algeria, to obtain antelopes and mouflons for the Jardin des Plantes. He will probably also obtain for the Museum some animals of the Sahara.

The same Museum has recently received from Mr. A. Boucard the valuable collection of birds that formed the basis of that gentleman's "Catalogus Avium," published in London in 1876. The collection consists of 25,000 specimens, representing over 7,000 species.

The different Alpine Clubs are doing good work in the study of the former extension of the glaciers in their districts. One of the latest of the reports is issued by the Italian Club, and contains a detailed survey by Professor Sacco, of Turin, of the old moraines in the northern Apennines. The lower margin of these moraines seldom descends below 700 metres, though in one valley in the Maritime Alps the glaciation reaches to within 500 metres of the sea level. In a few instances moraines have been recorded at lower levels in northern Italy; but Professor Sacco points out that moraine material washed down by rain or rivers has sometimes been taken to be true glacier deposit. It is often one of the most difficult questions for a geologist to decide, whether a moraine is truly in the position in which it was left by the ice, or has been washed down and reconstructed, perhaps at a level a thousand feet lower.

The "Return of the Income and Expenditure of the British Museum" for the year ending March 31, 1894, has just been issued. Among the acquisitions calling for special notice are the Pascoe and the Stainton collections of insects. The former of these contains about 49,000 specimens, of which no less than 3,000 are types. The Stainton collection consists of Lepidoptera, and is accompanied by a valuable series of original drawings of the larvæ of Micro-lepidoptera. Eighteen thousand specimens of Coleoptera have been received from Messrs. F. D. Godman and Osbert Salvin, being a further instalment of the rich collections acquired by the editors of the *Biologia Centrali-Americana*, which they are generous enough to place in the public collections. Messrs. Godman and Salvin have also enriched the Bird-room by handing over the remainder of their collections of bird-skins, consisting of 11,704 specimens, as well as 8,000 skins of birds from Mexico and 1,267 eggs of American species.

A series of 510 specimens of Cornish minerals have been presented to the Museum by Mr. J. C. Williams, M.P., many of which were obtained from mines now closed, and consequently inaccessible. The original drawings of most of the late Sir Richard Owen's discoveries have, through the kindness of the family, also found a resting place in the Museum.

A healthy record of work done towards the cataloguing of the vast collections is reported, no less than five volumes being listed. These comprise two on Birds, one on Snakes, one on Lepidoptera, and one on Corals.

From the special report devoted to the Department of Zoology, we learn that the whole of the series of Birds' eggs, some 47,000, has been arranged by Mr. Seebohm, and is now accessible. Six hundred and eighty type-specimens of land and fresh-water shells from the Morelet collection have been purchased and incorporated with the general series, and the arrangement of the Scorpions is nearly finished. The interesting groups of British Birds have been increased by three, viz., Greater Black-backed Gull, Dotterel, and Greenfinch, and a large series of models illustrating the life-history of insects has been placed in the insect gallery. No less than 130,185 specimens were received during the year, a number never before reached in the history of the Museum.

Turning to the Department of Geology, the chief acquisitions are the skull of *Megaladapis* from Madagascar (*see* NATURAL SCIENCE, vol. iv., p. 243), and the exquisite and perfect skeleton of a young Plesiosaur from the Oxford Clay of Fletton, near Peterborough. This, perhaps the most beautiful example of *Plesiosaurus* known, has been articulated so as to permit of the detachment of any single bone, and has been placed in a special case in the Reptilian Gallery.

In the Department of Mineralogy, the principal addition to the collection is that of the Williams Minerals above mentioned. The fine series of Meteorites has been increased by sixteen specimens, of which as many as ten are new to the collection. A series of 369 rock specimens, and microscopical sections of the same from the Allport collection, have also been secured.

No less than 17,677 specimens of plants have been incorporated into the collections of the Botanical Department, but the principal additions in Botany were the Deby Diatoms, comprising about 30,000 named slides, and the Jenner Herbarium of Algæ, containing 6,000 specimens. From the above sketch, the extraordinary growth of the Museum collections must be apparent to the least observant, and we hope soon to hear of extensions to the buildings, else the old Bloomsbury trouble must come back again.

As we go to press, we are informed by a correspondent that Mr. E. J. Bles, the Director of the Marine Biological Station at Plymouth, and Dr. G. H. Fowler, the Honorary Secretary of the Marine Biological Association of the United Kingdom, have tendered their resignations.

THE Rolleston Memorial Prize, for original work in biology accomplished by graduates of Oxford or Cambridge, has been adjudged to Messrs. M. S. Pembrey, of Christ Church, and E. S. Goodrich, of Merton College, both of Oxford, whose merit was considered equal.

CORRESPONDENCE.

The Bird's Foot.

I fear, from the courteous criticism of Mr. Mitchell in the September number of Natural Science, that I did not express myself clearly in my note on "The Bird's Foot," but if he will take the trouble to look over the article again, I think he will find that my intention was not to controvert Mr. Finn's proposition, but rather to maintain it. My conclusion in regard to the arrangement of the tendons was simply that the three-toed birds must have lost their fourth toe before its tendon became differentiated from the others, and this, it seems to me, is quite different from saying that birds "do not show evidence of the modification of their foot from a specialised four-toed foot."

If the deep flexors of a four-toed foot were entirely free from one another—as in the Passeres—and that bird were to lose the inner toe from disuse, would not the tendon of the flexor longus hallucis become aborted also for lack of something to pull upon? I think it would, but unfortunately we have no passerine or hummer minus a hind toe to settle the question. As to the distinctness of the muscles of the flexor perforans digitorum and flexor longus hallucis, if deductive reasoning is worth anything, we are justified in saying that these muscles are completely differentiated before the tendons, since all the more generalised birds are synpelmous while the flexor muscles are quite distinct.

In *Pterocles* and *Chauna*—there should, by the way, have been a query after the words *Palamedea* also, because I was not acquainted with its foot—it is to be noted that while the slip to the first digit is lacking, the flexor longus hallucis still meets with resistance from its vincular connection with the other tendons, and is thereby probably prevented from becoming abortive. Is there not a reason, based on habit, why *Palamedea* still retains the slip to the first digit? If I remember aright *Palamedea* is a bird of the forest region, and *Chauna* more a resident of the pampas, and does not *Palamedea* sometimes perch?

Turning now to the question of flight, while I cheerfully subscribe to the proposition that birds which fly well *to start with* will gain increased power of flight by ability to rest on the surface of the water, I protest that this does not furnish evidence that flying birds are ever derived from those which splash along the surface. In this connection it would be unfair not to speak of the habits of young ducks, suggestively named flappers by English writers on field sports, and not to mention the little Hoactzin which swims and dives well, although it does not, so far as I am aware, pass through any splashing stage. On the other hand, the difficulty experienced by the majority of the ducks in rising from the water in a calm, or down wind, is a most powerful argument against flight having been *acquired* in this manner. Right here let me say that Mr. Finn's suggestion in the foot-note on p. 211 seems extremely good. I am quite ready to believe that flight originated by jumping *down*, but not by jumping *up*. It should be remembered that the pluvialine relatives of the Gull fly remarkably well, and that the Gull is but an indifferent swimmer, gathering its food along the shore or at the *surface* of the water, not beneath it. The broad-winged Pelicans, too, are surface swimmers, not very good ones either, and sit high upon the water like the Caravels of old, instead of ploughing through it after the manner of a torpedo boat, as do their relatives the Cormorants and Snake Birds. In

fact, I am quite willing to rest my case on the habits of the various members of the order Steganopodes; the better they swim, the worse they fly, and *vice versâ*. As for the Frigate Bird, so far as habits go, he can scarcely be deemed aquatic at all, but rather "a creature of the upper air," "never alighting on land nor resting upon the water."

There are still many points of interest in the tendons of the bird's foot, and "one of these days" I hope to take up the subject again.

FREDERIC A. LUCAS.

IF in my note in the September number I did not rightly apprehend Mr. Lucas's position, the misapprehension is the less to be regretted as it has called forth another interesting note from him. It seems that we are in agreement on the immediate point, and differ only on a point on which zoologists may well expect divergence. An argument involving the terms "effect of disuse," "rudiment," and "phylogeny," cannot be entered on in the affix to a letter, and I am content for the present to accept Mr. Lucas's re-statement of his position. P. C. M.

DISPERSAL OF THE NUTLETS IN A LABIATE.

A PROPOS of Miss Pertz's interesting paper on the "Dispersal of Nutlets in certain Labiates," a member of the British Association asked, at the time the paper was read, what was the advantage to the plant of the reflexed hairs in the calyx of *Teucrium Scorodonia*; and he drew on the blackboard a section of the calyx, showing the hairs well reflexed. No answer was offered at the time. A solution at once occurred to me, that the hairs were of use in preventing the nutlets from dropping out, as soon as mature, at the foot of the plant; an impediment, such as a ring of hairs, would retain the ripened nutlets in the calyx till a strong enough wind arose to overcome the resistance, and flirt the nutlets out to a distance from the parent plants. The puzzle was, why should the hairs, which are rather stiff when mature, be *reflexed*? a position likely to defeat the supposed object.

After examining several plants this autumn of *Teucrium Scorodonia*, I do not find that the hairs in the calyx are reflexed in the living plant. When the corolla has fallen, the hairs, which hitherto have been pressed erect between the corolla and calyx tubes, begin to descend, moving very gradually through an arc of 90 degrees, till they finally assume a horizontal position; their curved, flexible tips meeting and interlacing just above the maturing nutlets. When these are matured, the ring of hairs is just so rigid that a violent shaking of the plant is required to eject them; and it is obvious that the force which is sufficient to do this will be enough to throw them to some distance. It thus appears that the obstruction offered by the spreading hairs in the calyx actually assists in the wider dispersal of the nutlets; while the slight upward curve of the four teeth forming the lower lip of the calyx would, perhaps, have a similar tendency, giving an upward direction to the flight of the nutlets, when shot out by any violent motion. A free course is allowed them, so far as the upper lip of the calyx is concerned; for in this species it is remarkably bent back, almost at right angles to the calyx-tube.

E. F. LINTON.

THE MUSEUMS' ASSOCIATION.

As one of the executive of the Dublin local committee, I confess I read with indignation the note "Science at a Picnic" in the August number of NATURAL SCIENCE. Such a heading, savouring of the up-to-date style of a branch of London journalism, sounds inviting. The correcting reply from the Association Secretaries in this month's number (p. 319) shows how inaccurate and misleading the statements made under the above title were, and one would have been content to say no more had not you, as Editor, practically repeated (p. 320) the earlier statements, with now accompanying and apparently justifying explanations. The meeting lasted *four* days, not three, as stated. The President's address, though printed in the August number as the most important contribution to the meeting, is not admitted as part of the Association proceedings. The only day of the four not as full as it could well be of

actual museum work was Thursday, when an excursion was arranged, not simply to a waterfall (p. 320), but to the mansion and estates of one of the most enlightened of Irish noblemen, Lord Powerscourt, F.R.S., whose art collection, viewed with great interest by the art curators present, is, I am told, one of the finest. Two papers were shortened, one was my own, taken last, and shortened at my own request, to make way for the other "burked" (?) paper, which was, though contributed by an able member of the Association, of an extremely discursive nature, and was at the end of half-an-hour "no forrarder." Discussion on the papers was not seriously curtailed. Once or twice it was invited, and was not forthcoming.

As an Englishman engaged in science work here, I wish to protest against the countenance your note gives to the too prevalent opinion that Irishmen are not capable of sustained and earnest work.

Dublin. T. JOHNSON.

[WE can assure Professor Johnson that, neither in our original note nor in our subsequent defence, did we intend to cast any aspersions on the Irish character, or to slight the labours of the Dublin local committee. Our views as to the mode of conducting a meeting of this kind may not meet with universal approval; there is room for argument. Our statements, however, were based partly on the official programme of the Association, which we knew was not absolutely adhered to, and partly on information received from members of the Association, present throughout the meeting, whose good faith we have no reason to doubt. We wish so well to the Museums' Association that we should regret indeed to arouse the indignation of its members against us; but under the circumstances we can hardly say more than that we *hope* we were wrong.—ED.]

THE GLACIAL PERIOD AND PRIMITIVE MAN.

ON examining the evidence for the immense antiquity often attributed to Man, it is apparent that there is a great difference between the pre-Glacial and the post-Glacial. The post-Glacial evidence is of a varied character, comprising not only stone weapons but other implements, rude drawings, charred and split bones, etc. On the other hand, the pre-Glacial evidence entirely consists of rudely-worked flints. In fact, the deniers of human antiquity, when confronted with the post-Glacial evidence, cannot deny its human origin, so take refuge in denying its antiquity; while, when opposed to the pre-Glacial, they admit the antiquity, but deny the human character.

Now it can hardly fail to be remarked that during the immense period for which Man is often supposed to have existed before the Glacial Period, the progress of the species was very small—in fact, compared with its subsequent advance, almost nil; while, during the far shorter time that has elapsed since, progress has been continuous and rapid.

Accordingly, I venture to suggest the following hypothesis. Man, that is the intelligent and progressive animal known as Man, dates only from the Glacial Period. Before this there was no true man, but only a very intelligent anthropoid, surpassing those of the present day, being able to use properly-shaped stones as weapons, and when such stones in their natural state were inconveniently formed, to break off fragments, and so reduce them to a convenient size and shape. Probably it also knew the use of fire. Furthermore, it resembled Man, physically, much more closely than any existing species does. Nevertheless, it was in all essential respects only an ape.

The severity of the climate at the commencement of the Glacial Period caused a great mortality among this species; only individuals survived who were able to adapt themselves in some degree to the changed conditions. Under this pressure the species changed rapidly. The development was, of course, mainly in intelligence and adaptivity; especially intelligence in dealing with fire, now becoming a vital necessity. Strength and activity, being of secondary importance, were no longer kept up by Natural Selection, and so retrograded, either through the accumulated

effect of disuse, or economy of growth; or perhaps the possessors of extreme strength and activity were more rash, combative, and foolhardy, and so more liable to be killed. There was also a change in certain physical characteristics, consequent on the enforced change in habits. Thus the change from ape to man was, so to speak, sudden, the transformation being effected probably in a few thousand years.

The question of the unity or plurality of the origin of Man was at one time a good deal debated. I am not sure that, upon the hypothesis of development, the words have precisely the same meaning as upon the older hypothesis of a special creation; for we now regard the whole race as undergoing a gradual transformation, and do not regard the new species as descended from a single ancestral pair. Nevertheless, the words can be given a meaning; either (*a*) did the evolution take place in one spot and at one time, or in different places and at different times? or (*b*) was the evolution from a single "species," or from more than one?

In the latter sense, I am inclined to believe in the unity of origin. It seems, however, just possible that, while one race arose as above suggested, another may have arisen in the tropics, say in Equatorial Africa, where there was no Glacial Epoch. In consequence, it was deficient in intelligence and essentially unprogressive. The existing races are the result of the fusion in varying proportions of these two.

I think it more probable, however, that the whole race was, in a sense, imprisoned by the Glacial Epoch, making locomotion difficult and dangerous. When the first interglacial period set in, it was liberated, and in consequence spread down to the tropics, which were thus peopled with Man, but a very primitive form of Man. The tropical regions underwent no further glaciation, and consequently their inhabitants were not compelled to improve; the portion of the same race who stayed at home underwent another glaciation, and were in consequence further developed. When mild conditions again came round, this more highly-developed race spread towards the tropics, driving their predecessors before them, and to a certain extent amalgamating with them. In this way, successive waves—each more highly developed than the former—spread out from the glaciated regions towards the equator on the occasion of each interglacial period. So we might expect to find that the lowest type has been driven into the equatorial forests. Here it would hold its ground, being nearer to the anthropoid ancestor, and therefore better suited to the environment than the succeeding races.

It may be objected that a race which had passed through a glacial climate, and to a certain extent acclimatised itself, would probably find even the interglacial climate too warm, and would, therefore, be more likely to spread towards the pole than the equator. I fancy that the first spreadings were more probably towards the pole. It was the approach of the second glaciation that drove a portion toward the equator. Those who had spread toward the pole were probably killed out by the extreme severity of the climate when glaciation again supervened.

To guard against possible misconception, I may remark that I do not attribute the transformation *merely* to the severe conditions; the intellectual transformation was caused principally by the varying conditions, necessitating a certain power of adaptation. Had the severe conditions prevailed unchanged until now, after the first transformation the organism would have adapted itself to the new surroundings, and, further power of adaptation being unnecessary, would have disappeared. The Esquimaux show what unvarying severity of climate can effect; and had the cold of the Glacial Period continued unchanged, the human race would probably be lower than the Esquimaux. It seems to me that, even at the present day, we can see that in races which have long (2,000 years or more) inhabited a region with unchanging climate, progressiveness and the power of assimilating new ideas die out.

J. R. Holt.

The Erect Posture of Man.

I have several times seen it stated that the erect attitude of man constitutes a very serious difficulty in the way of those who believe that only

Natural Selection has been the cause of his development. The matter is not mended by showing that the erect position is better than the crawling, because it is necessary for Natural Selection, not only to show that the final result is better than the initial, but that every step on the road carried an advantage with it. The problem is certainly difficult, but I do not think it is insuperable, and I have a suggestion to offer which may, perhaps, lead to a partial explanation.

The idea was suggested to me by watching mungooses in India; when these animals wish to look around them, they sit up like a dog begging, bringing the head as high as possible. Now, if we suppose that in the commencement of the Glacial Epoch, the anthropoid was liable to attack, either from carnivora, as famished wolves, or from his own species, evidently extreme circumspection would be requisite; a single moment of inattention might prove fatal. Evidently, the result would be that those individuals who were able to carry the head aloft persistently would have an advantage over those who could only raise it up now and then.

This may account for the head being carried high, and the back being straight, but by itself it will not account for the disuse of the fore-limbs in locomotion; it would be more likely to lead to excessive lengthening of the arms, so that while the back was straight, the hands could be rested on the ground.

I have postulated that this anthropoid had sufficient intelligence to use stones as weapons; now, stooping to pick up a stone would necessitate being off guard for a moment, during which a sudden attack might be made; moreover, if the ground was frozen hard, detaching a stone would take time, and necessitate a prolonged period of helplessness, during which an attack would be disastrous; evidently it would be desirable for the animal to carry a suitable stone *with* it, by throwing which the adversary might be, if not killed, momentarily disabled, and time gained to pick up another stone. It would be difficult or nearly impossible to use the arm belonging to the hand carrying the stone in locomotion, besides which it might be desirable to have it free in case of sudden attack. So the normal progression would probably be nearly erect on the hind limbs, with occasional assistance from the arm not used in carrying the stone.

In fighting, or in hurling missiles, extreme firmness on the legs would be desirable; the slightest stumble might prove fatal. Now, those individuals who dispensed with the assistance of the arm as much as possible, and used the legs only, would come to have stronger legs and be steadier on them than those who made much use of the arm in locomotion; they would consequently have an advantage in the struggle for existence.

If we admit the possible transmission of acquired characters, this steadiness on the legs might be transmitted; but it is not necessary to assume this. My argument is that those individuals who had a congenital dislike for using the arm in locomotion, or preference for the unaided use of the legs, would become steadier, and so have an advantage over their fellows; and it is this partiality for using the legs only which I suppose to be transmitted.

The erect posture, and the habit of using only the legs in locomotion, being acquired, there would no longer be any advantage in having the arms long enough to touch the ground. Now, in hurling missiles, too long an arm would be a decided disadvantage, as it would make the animal liable to over-balance himself. Too short an arm would be a disadvantage, obviously, too. The arm would, therefore, shorten to some extent, until the best length for throwing stones had been attained. The advantages of a firm grasp, either in throwing or in using weapons, would also lead to modifications in the form of the hand.

I have laid considerable stress on the advantage of carrying a stone, so as to be at once ready for attack. Remembering that this missile must have frequently failed to hit its mark, it may be supposed that I have laid too much stress on a very slight advantage. But I suppose the struggle for existence to be terribly severe. Food was scarce, and therefore the anthropoid had to venture far afield to seek it, or starve. Trees were scarce, so he could not seek safety in climbing. Probably the change from a frugivorous to an omnivorous diet was contemporaneous. After having killed his assailant, he must often have been too terrified and exhausted to

search for fruits, and have eaten the dead body. Judging from our present animals, the change from a vegetable to an animal diet must have been very easy. Most of our frugivora, as squirrels, etc., will eat insects, and not unfrequently prefer them.

I have supposed the change to have taken place in the Glacial Period, but obviously this is not essential. The essential points are, (1) fierce struggle for existence, (2) liability to sudden attack, (3) recourse to trees cut off.

Most probably this hypothesis will prove untenable on criticism, but I think I have shown that the erect posture is not an insuperable stumbling-block, that at least it is possible to *imagine* ways in which it might have come about.

6 Harrington Street, Dublin. J. R. HOLT.

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NOTES AND COMMENTS.

The Scots Fishery Board.

In our September number we were able to announce that Mr. John Murray, of the "Challenger," was very strongly supported by leading scientific men for the post of Chairman of the Scots Fishery Board, in succession to the late Mr. Peter Esslemont. The appointment is made by the Crown, and no doubt the recommendation of the Scots Secretary is the important factor in determining the decision. Mr. Murray seemed peculiarly marked out for the post, from his special attainments in connection with marine biology, and from his long and capable service under Government in connection with the "Challenger" expedition and the "Challenger" reports. Most of our readers are now aware that Mr. Angus Sutherland, the Liberal Member of Parliament for Sutherlandshire, has been appointed chairman, and has in consequence vacated what may be regarded as a safe Government seat.

As this seems a post which, to the great advantage of the State, might be filled by a man of special distinction in science, it is worth while going into the history and nature of the Board. Like so many excellent Scots institutions, it dates back in origin to a time long preceding the date at which Scotland began to enjoy the advantage of union with England. The first White Herring Fishery Board was founded in the 14th century, no doubt with a view to secure some benefit to the Crown from sea-fishery. The Board of British White Herring Fishery, the parent of the present Board, was established by 48 Geo. III., c. 110, and was dissolved in 1882 by the 45 and 46 Vict., c. 78, which established the present Fishery Board and is the basis of its action. The Act provided that the Board should consist of the sheriffs of three sheriffdoms, to be selected by the Crown, and six other members to be appointed by the Crown. The Crown nominates one member to be chairman, who has a salary of £800 per annum, and appoints the secretary,

who has a salary of £300–£400. The Secretary of State of the Home Department appoints the inspector of salmon fisheries, who has a salary of £600.

The Board have the appointment of all the other offices, such as the general inspector of sea fisheries, with salary of £300–£350, an assistant, with salary of £250–£280, and a number of minor appointments. £1,800 is given to the Board to spend in scientific investigation: they opened at Dunbar about twelve months ago their first fish-hatching establishment.

The Board have the general superintendence of the salmon fisheries of Scotland, and take such measures for their improvement as the funds under their administration allow. They are the only authorised authority for branding herrings, and from this source they derive an annual income of about £8,000. They look after the piers and quays under the authority of 5 Geo. IV., c. 64. Their expenditure under all heads amounts to about £28,000 a year. At the beginning of this year the only scientific member of the Board was Professor McIntosh, of St. Andrew's.

Here, then, is a great permanent department with most important duties and with a jurisdiction extending over all Scotland and the Northumberland coast. For the administrative and executive duties, the permanent staff and a board of intelligent persons selected for any reason whatever should suffice. But nothing is clearer than that the main object of expenditure should be the maintenance and improvement of Fisheries, now that the income is no mere wresting of taxes for the Crown. The regulation and repair of piers and quays, and commercial necessities like the branding of standard qualities, are matters within the intelligence of most. The times and seasons for protection, the regulation of trawling, the provision of bait, and above all the extension of fish-hatching stations are matters that can be solved only by trained scientific experts. Sheriffs, in Scotland, are necessarily learned men; Members of Parliament, no doubt, have special abilities and have the opportunity of being in touch with the practical needs of their fisher-constituents. But it is urgent and imperative that in this country, as in France and America and Russia, departments dealing with problems soluble only by science should be under the responsible control of scientific men.

This is no question of Mr. Angus Sutherland and Mr. John Murray: it depends upon no weighing of their personal capacities. The choice of individuals is best left in the hands of a responsible department, and many considerations unknown even to omniscient editors may select or reject an individual. The choice of the kind of individual should not be left to Government.

SCIENCE AND THE STATE.

FOR the selection, by Government, of the wrong kind of individuals for posts that should imply scientific knowledge in the holders,

we blame not Government, but men of science. In all matters touching State-craft the habit of these is to hold aloof superiorly, leaving mere politics to the common man, or to rush belated into a controversy, delaying compromise by new discharge of venom. The savant in his laboratory, ignorant of the bombardment of his city, is a figure that tickles the fancy, but should offend the judgment. The irruption into the Irish controversy of a great scientific man, since unhappily dead, and his easy pre-eminence among irreconcilable and turbulent partisans, was food for laughter and tears. Scientific men have no right to wait till controversies strike down to their elemental passions. They must take their part as citizens in daily conduct of the State. Otherwise, when the pinch comes, they will be unschooled in the diametrical opposition between political and scientific knowledge as bases of action. In scientific action the expedient depends upon the recognition of what may be certain among facts; in political action the expedient depends upon the recognition that no facts are certain.

Thus scientific men have succeeded in gaining a reputation, especially among politicians, for acting in the conduct of affairs either negligently or with virulent self-confidence; whereas the fact is that what error of this kind lies in them is no more frequent than the corresponding error of politicians in matters of science. The defects of the qualities of science may be want of prudence and conciliation: the defects of the qualities of politicians are want of decision and certainty.

Decision and certainty are absolutely called for in matters like the maintenance of fisheries. There is no room for compromise with nature: knowledge of nature is not opinion, but definite and inevitable fact, and again we must insist that scientific men must be appointed to posts involving scientific training and knowledge in the conduct of the duties.

But scientific men are to blame in another fashion. Professions like the Law, the Church, or Medicine are close corporations or trade unions, which take vigorous action to support their members. Partly for the advantage of their members, partly for the advantage of Law, or Medicine, or Religion, they secure that every post shall be held by a suitable member of their own bodies: that the untrained and inexpert, whatever their general ability may be, shall not be appointed.

With us there is nothing corresponding. Here is a field for the British Association: to form a committee that shall be an advisory and consultative board on all matters affecting the interests of Science, and the interests of scientific men. It must be a committee elected by a body large enough and representative enough to convey the imperative judgment of Science, and strong enough to secure that its advice shall be acted upon. Our English Government is no autocrat that can disregard considerations presented to it by weighty authorities. If retired naval and military officers or mere

members of a party occupy posts that should be held by scientific men, not Government but scientific men are to blame.

GEOLOGY IN QUEENSLAND.

WE do not know whether it is that geologists are more united, or that geology, from its obvious connection with the Deluge, has had the public ear for a larger period than other branches of science that should come within the cognisance of the State, but the recognition of geology by the State has resulted in more active work in geology, both in England and in the colonies, than in any other branch of science.

We have received the *Annual Progress Report* of the Geological Survey of Queensland for the year 1893, by Mr. R. L. Jack, the Government Geologist. The work of the year appears to have been much interrupted by the disastrous floods, which took place at Brisbane during the month of February, and which might have irretrievably injured the collections had not prompt measures been taken by those on the spot, and had not the officers out in the field been summoned home by telegraph.

Since that time of trouble the offices and the collections of the Survey have been removed from their temporary home in Elizabeth Street to a more centrally placed building, recently occupied by the Registrar for Titles' Department. Here the collections have been rearranged and exhibited by the two energetic and able Assistant Geologists, Messrs. W. H. Rands and A. G. Maitland. The result of this moving, and the greater available space for exhibition, appears to have been to increase the popularity and therefore the usefulness of the Museum. Even at the somewhat inconvenient premises in Elizabeth Street, the collections were daily visited by about twenty-one people; the attendance has now greatly increased.

The exhibits in the Museum are arranged under the four heads of Maps, Minerals, Rocks, and Fossils. The productions of the colony itself naturally occupy a prominent place, and, subordinate to a broad scientific classification, they are arranged in accordance with their geographical position, beginning in each case with the northernmost representative. The rocks of Queensland are now completely exhibited for the first time. The arrangement of the fossils naturally follows that adopted in Messrs. Etheridge and Jack's excellent work on "The Geology and Palæontology of Queensland and New Guinea."

In a rising and imperfectly surveyed country such as Queensland the officers of a Geological Survey have naturally little time to spare for what we at home dignify by the title of pure science. They are bound to consider the immediate need of the community that pays them, and to devote their attention to the practical and commercial side of geology. In fulfilling this duty, the Geological Survey of Queensland, though miserably small in numbers, and though suffer-

ing from the financial whirlwind that lately swept over Australia, acquits itself right well. The most important work of this nature has been the survey of the well-known goldfield at Charters Towers. A geological map of the field, showing the surface outcrops, was issued early in the present year; since then the underground structure has been carefully investigated by all the available strength of the Survey, and a fresh edition of the map, embodying this work, will shortly be published. It is safe to say that this will now be the best surveyed goldfield in Australia, and that the detailed maps will be of enormous scientific and practical value.

In addition to actual surveying and collecting, the officers of such a Survey are continually being asked for information by prospectors, miners, agriculturists, and others. And in our colonies, it must be remembered, the officers of the State have to behave far more as the servants of the Public than some of them think it necessary to do at home. The Geological Survey of Queensland has recently received many enquiries as to water supply, especially as to the probabilities of finding it through artesian borings. Information is carefully collected and again retailed, which, we doubt not, greatly benefits the enquirer. But, as all who have had anything to do with water supply know well enough, it is an exceedingly difficult matter to advise upon, in the absence of very full and precise information as to the structure of the surrounding country. The circumstances may all point to the likelihood of an artesian well being successful, but an unperceived fault may disarrange the strata and prove the ruin of the whole attempt. Consequently, before a survey can be of that practical service to the community which is hoped and demanded, it is necessary that the whole country be adequately mapped in as great detail as possible. This, of course, is not a task to be undertaken in a hurry or by rule of thumb; it demands slow collection and collation of facts and prolonged scientific study. Many of the steps necessary for the successful accomplishment of the task are not such as appeal to the uninstructed public, which only regards immediate results. We refer especially to the collection and study of fossils, and the correlation of strata by their means. All this, however, must be done, and men and money must be supplied. The experience of all countries has shown that the investment is one that eventually pays a large interest, and we sincerely trust that, for the sake of the rich and beautiful colony of Queensland, every facility will speedily be given to its Geological Survey to continue the work so worthily begun by Mr. R. L. Jack and his devoted assistants.

The U.S. Geological Survey.

The September-October number of the *Journal of Geology*, just to hand, contains some observations by Professor R. D. Salisbury, *à propos* of the work of the United States Geological Survey, which are very

germane to our remarks on the Queensland Survey in the preceding paragraph.

It appears that additional appropriation has been made, providing for the gauging of the water supply of the United States, and for the investigation of the artesian water areas. This was by no means before it was required; for the information constantly demanded from the Survey, concerning the water resources of the country, is far more than it is at present in a position to give. Under Major Powell's administration an excellent beginning was made, but it was only a beginning, and by no means sufficient knowledge was obtained to enable the Survey to solve the various problems that are continually presented for solution. The mere fact that so many applications are made to the Survey shows that the American public is at last awaking to the fact that a knowledge of geology may save them many thousands of dollars.

The hydrographic work of a Survey must be based not only on geology and topography, but on meteorological records. The variation in rainfall on the one hand, and in barometric pressure on the other, are potent factors in determining the annual or seasonal yield of any given district. Meteorological information may no doubt be obtained from the already constituted Weather Bureau of the U.S. Government; but, as was pointed out in our review of W. B. Clarke's "Climatology of Maryland" (NATURAL SCIENCE, vol. v., p. 148), the climate of a country is itself largely dependent on its topography and geology. Thus the whole work falls most naturally into the charge of the Geological Survey. We learn from the *Journal of Geology* that "the organisation of the department of hydrographic work under the present limited appropriation is under the charge of Mr. F. H. Newell, who is assisted by Mr. Arthur Davis. Both of these gentlemen are trained topographers, and have had long experience in such hydrographic work as is contemplated by the Survey. At present they are largely engaged upon the special study of the water supply of the great arid and semi-arid regions of the interior, employing local assistance wherever parties are found who are interested in the work. A large amount of this assistance is voluntary, so that they are able to obtain much more extensive results than would otherwise be possible with the resources at command. The railroads, especially, are giving much assistance by having their bridge-tenders read the river gauges that have been set up under the direction of Mr. Newell."

Mr. Salisbury also informs his readers that the Director of the U.S. Geological Survey proposes to establish a laboratory for the study of materials used for road-making. This, it is believed, will give a great impulse to the improvement of highways throughout the land.

Hitherto much money has been wasted owing to the employment of inappropriate road-metal, which produced roads dusty in summer and muddy in winter. "Inferior materials have sometimes been used, when, in the immediate vicinity, there were other materials which,

alone or in combination, would have produced a solid road-bed." "A large part of the country, including the greater portion of the Southern States, and some portions of the Mississippi Basin, has been thought to be essentially destitute of materials suitable for the construction of good roads. The inquiries that have been made by geologists have shown that in many places within these regions there are hidden deposits of gravel and other sorts of rock, which, when properly used, might give excellent highways, and that around the margin of this great area, often within the limits of convenient railway distribution, there are abundant supplies of rock well fitted for such use. It only remains to discover the supply of such stone as is cheapest and best for the use of each region. . . . . For more detailed information, it is proposed that the various road commissioners send to the Survey samples of such rocks and gravels in their immediate vicinities as are believed to be valuable for road construction."

A laboratory in connection with the Massachusetts Road Commission has already been established at Harvard University. It is, however, not thought advisable that each State should establish a laboratory, for this would lead to much duplication of work, and after a time little work would remain to be done. Moreover, "the results obtained by divers observers and methods would lack the unity which give [*sic*] a national value." An officer of the Survey will therefore be detailed to take charge of the investigation of road-metal in the Harvard laboratory, and the establishment of a national laboratory will be brought to the attention of Congress, and a request made for a suitable appropriation.

As we firmly believe in the truth of the remark once made by a wise man, that the status of a nation's civilisation may be estimated by its facilities of communication within its own borders, so we cordially approve of these ideas of the new Director of the U.S. Geological Survey, and hope that in his attack on Congress he will meet with the success that his efforts deserve.

A Geological Record.

Although, in its relation to the State, geology is active and vigorous, yet it exhibits at present symptoms of neglect of a matter vital to the progress of scientific work. Rumours are rife that records of geological literature will soon be as dead as the Dodo, by reason of the difficulty of getting supporters in sufficient numbers to make such ventures pay. Talkers there are in plenty, and agitators for Records of new kind and arrangement; but few there be sufficiently interested in such work to put their hand into their pocket and keep a Record going. Dagincourt's "Annuaire" has made a good fight, has filled a great gap, but it is not supported as it should be. Blake's "Annals," too, dies from the non-support of those who clamour for Records. But during all the births and deaths of Records as individuals, there has yet been one that has

weathered the storm, and that solely because it has been supported by the very life-blood of our own Geological Society. Commenced many years ago as a quarterly list of books received, under the auspices of Thomas Rupert Jones, then secretary to the Society, it has gone steadily on, recording year by year the contents of every serial and the name of every book and map that has been received into the library. This year it may still be found by the curious in the November number of the *Quarterly Journal of the Geological Society* —ignored by nine-tenths of geologists, greatly prized by the remaining few. For of all lists published, these are the only ones where you may find a complete record of the geological papers in any particular serial for something like twenty years. It is not too much to say that this is the most valuable publication that the Geological Society gives to its members in the course of the year. The "Additions to the Library," for so it is called, has, we believe, been compiled by the present librarian, Mr. William Rupert Jones, for many years, though we have never observed his name attached to it. The list just published, which covers the period July, 1893—June, 1894, consisting of 125 pages, exceeds its predecessor in length by 45 pages, and is a remarkable example of careful and painstaking recording. We trust the Geological Society, whatever it may do in the matter of records, will keep this portion of their publications intact, for it serves a purpose which no other record has ever served, in giving a continuous account of the contents of various serials which is of considerable utility and value.

A Universal Zoological Record.

In this vital matter of recording, a most important step in advance comes from America, and Minneapolis has practically begun what should have been done by London, Naples, or Berlin many years ago. This means that we have received from Mr. Clarke Barrows, of the University of Minnesota, certain printed catalogue slips on cards of the well-known size supplied by the Library Bureau of Boston for card-catalogues. The arrangement of one of these slips is here reproduced:—

Wilson, Edmund B.

1892 July — The Cell-Lineage of Nereis. A Contribution to the Cytogeny of the Annelid Body. 7 plates, 8 wd.cts. & 1 cytogenetic table.

Journal of Morphology, Vol. VI, Nr. 3, pp. 361-460 (480).

Abstract in—

O [punched if required.]

These cards are samples of what is intended eventually to be a complete card-catalogue of zoological literature arranged under authors' names. The beginnings of the undertaking are, however, small. It is proposed to begin the catalogue with the current volumes of the periodicals now available at the University of Minnesota, which only number forty-two, whereas the actual number of periodicals containing zoological papers is not less than a thousand. But arrangements will soon be made to get the titles of all other important zoological writings, so that the catalogue will be complete for all zoological literature not recorded in Carus and Engelmann's Bibliotheca Zoologica up to the end of 1861.

The great feature of the proposal, however, is not the completeness of the catalogue, but the fact that these cards will be issued to all who choose to subscribe for them, at a rate not exceeding a penny a slip, thus furnishing them with a continuous and up-to-date reference to all zoological literature. The advantage of this plan over all other systems is that the list is always kept in alphabetical order; or slips can be selected and arranged in any other order that the purchaser desires. Let us make for a moment the very desirable supposition that the promoters of the *Zoological Record* will subscribe for a set of slips. Then the editor could at once roughly sort out the large majority of these slips and hand them over to the recorders of the different groups. The remaining doubtful slips could be gone through by the editor or any competent zoologist with the aid of the original publications, and could thus be sorted according to the group or groups with which they dealt; duplicates could of course be purchased when required. By this simple means each recorder would be furnished at the start with his list of titles, and whether the list were complete or not it would at all events be known that it was complete for certain periodicals. Each recorder would thus have to refer only to the papers in his list, and would be spared the drudgery of wading through piles of profitless publications. Librarians also will be glad to be spared the trouble that authors' copies are continually giving them, since their subscription will get for them not merely the reference, but a slip all ready to insert in their card-catalogue. In short, this enterprise may do much to remedy the sad state of things described in the vigorous article by Mr. Stebbing contributed to our last number; and we venture to think it superior to the proposals that Mr. Stebbing has put forward there.

There is nothing very new in this plan. Similar suggestions were made in the correspondence columns of *Nature* during August and September, 1892, while on the continent Dr. H. H. Field seems to spend most of his time in advocating the establishment of a central zoological bureau to perform this very work.[1] The only thing that is new, and that is really astonishing, is that someone has at last made a beginning. We may for various reasons regret that

[1] See *Mém. Soc. Zool. France*, vii., p. 259; 1894.

it has been left to Minneapolis to show us the way, and we could wish that the headquarters of the movement had been in a city where there were a few more zoological publications than are to be found in the library of Minnesota University. But we must rejoice that the admirable methods of library cataloguing which have been brought to such perfection by the Americans are at length to be applied to the relief of the overweighted zoologist, and we must hope and strive that sufficient pecuniary support be forthcoming. If the undertaking be once fairly started, and if it acquire those proportions that are needed to make it really valuable, then we shall soon be wondering how on earth we ever managed to get along without its assistance. Those, and we trust they are many, who desire to aid this good work, should communicate with Mr. Clarke Barrows or Professor H. F. Nachtrieb at the University of Minnesota, Minneapolis, U.S.A.

THE LINNEAN SOCIETY'S CATALOGUE.

THERE is not a wide difference between Records of Literature and Catalogues of Libraries, and while we are on this subject we may well consider the strange case of the Linnean Society. For many years past, successive Councils of that learned body have struggled with the apparently hopeless task of bringing out a list of their books that shall supplant the old catalogue of 1866. Such a work should take certainly not more than one year to accomplish, but although there have been many rumours of its incubation, nothing has as yet appeared. We say "appeared" advisedly, because we have ourselves seen an abortive pamphlet which purported to be an attempt at a catalogue of the serials, but, so far as is known, that has never been distributed or sold by the Society. Surely years enough have passed since the publication of the last catalogue to make it worth the while of the Council to publish another, for the library contains an interesting series of books quite apart from the precious volumes which at one time formed part of the library of Linnæus himself, and many of which are almost if not quite unique in this country.

It may be urged in excuse that the officials of such a Society have enough to do to carry on its regular business, but there are many men who would be perfectly competent to make a catalogue, and who would be glad of such congenial occupation. It is curious how apathetic ordinary members of societies are: they are content with the pride of membership and with the idling of an occasional evening; so long as tea and coffee are served they do not care one jot what happens with regard to the general management. But surely there must be many members of the Linnean who are sufficiently alive to the importance of knowing what the resources of their library are, and it is surprising that they do not insist on a catalogue being published.

Possibly by the next anniversary meeting (May 24, 1895), some Fellow may be goaded into asking for what may be an explanation

but can be no excuse. In the meantime, it would be quite possible to make a catalogue. The members of such a Society as the Linnean, with a historic past, should regard themselves as trustees for the hopes of the original founders, and they should remember that it is their imperative duty to maintain and improve the body corporate.

Science and Medicine.

Sir James Paget and Dr. Lauder Brunton.

The reproach of empiricism is being removed from medicine at a rate which would have startled the medical mind of only twenty years ago. It is true, indeed, that no amount of scientific knowledge can replace clinical experience, but it is to the combination of the two that we must look if we are in search of the ideal practitioner. Every scientific man will welcome the tone of the inaugural address delivered by Sir James Paget before the Abernethian Society, at St. Bartholomew's Hospital, on October 11. That a man cannot be at once a scientific man and a good practitioner was characterised by Sir James as sheer nonsense, and he proceeded to show in how many ways the science of medicine could be advanced, even by a busy man in general practice, if he were possessed of a truly scientific mind. He emphasised also the importance of the collection of facts, verified again and again till they were absolutely certain. In all that he said we most heartily concur. Few applied sciences are so complex as that of medicine, and in none is there need of more close and accurate observation. This power is to be attained only by scientific training, and it is to the superior education in science now afforded to the medical student that much of our recent advance is due, while still more may be looked for in the future. No better maxim can be taken as a guide in science than the sentence of John Hunter's quoted by Sir James Paget: "Do not think: try: be patient: be accurate."

If further commentary on Sir James Paget's address be needed, it may be found in Dr. Lauder Brunton's Harveian Oration, delivered a few days later before the Royal College of Physicians. The one man above all others from whose discoveries modern medicine may be said to date is undoubtedly Harvey: and Harvey was a practical physician no less than a brilliant investigator. This year's Harveian Orator is himself an example of the same combination, and in bestowing the Moxon medal for clinical research upon Sir William Jenner, the College of Physicians has shown its recognition of the high value to be set on the results of scientific method in the hands of a busy practical man.

There is, however, another side of the matter. Every practitioner may be and should be a scientific man, but it is not by the labours of the practitioner alone that modern medicine has advanced. Of this fact Dr. Brunton's Harveian Oration affords many instances. He devoted the major part of his time to a consideration of the

modern developments of Harvey's work, and here it is to be observed that the pure physiologist is to the front. The specialisation demanded by modern science renders this imperative, and all that can be asked of the practitioner is that he shall keep himself abreast of the discoveries of the physiologist and learn to apply them in the treatment of disease. It is the intelligent application of the discoveries of pure science which forms the field in which clinical medicine and surgery have made and will make their most rapid advances, and it is in this field that a scientific training and habit of mind will be of most value to the practitioner.

ANTITOXINS.

THE latest application of pure science to practical medicine consists in so-called "serum therapeutics." Although diphtheria antitoxin is still on trial as a means of practical treatment, there can be no doubt that its discovery is an immense advance in medicine. Chemical theories of immunity now bid fair to oust from the field the theories based on the doctrine of phagocytosis. "Für böse Gifte, Gegengift," sings Brangäve, in 'Tristan und Isolde,' and it is a good motto for the modern pathologist. There is already strong evidence that the 'Gegengift,' manufactured in the animal body as the result of a non-fatal attack of tetanus or diphtheria, can be used with effect in the treatment of the same diseases in man; but some time must elapse before good statistical evidence is forthcoming as to the effects of the diphtheria antitoxin. And for this reason. It is a usual custom in this country to regard as diphtheria all cases of membranous sore throat, nor can it be doubted that it is a wise practice to treat them as such. But statistics are in this way largely invalidated. Conclusive proof of the diphtheritic nature of a sore throat can be obtained only by the recognition of the characteristic diphtheria bacillus, and it is, perhaps, chiefly on this account that the foreign statistics of diphtheria mortality appear so much higher than ours, since only cases in which this evidence is present are admitted into the category of true diphtheria. When this source of fallacy is removed in this country by a systematic bacteriological examination of all cases of suspected diphtheria, we shall be in a position to estimate at its true value this latest development of science in the treatment of a hitherto intractable disease.

AN ELECTRICAL THEORY OF VISION.

AN interesting communication, brought before the British Association, and as yet unnoticed in our columns, was Professor O. J. Lodge's suggested electrical theory of vision. This was preceded and introduced by experiments illustrating Clerk Maxwell's theory of light. The necessary electrical radiations were obtained from a sphere charged from an induction coil; for their detection an arrangement of loosely-packed particles was employed. These, under the influence of the surgings of the electrical radiations, attracted one

another, and so became more closely packed. When closely packed they allowed a current to pass through more readily. The arrangement is termed a "coherer," on account of the capacity of the constituent particles to become more closely packed. After coherence the original looseness and consequent resistance may be restored by tapping. Professor Lodge's "coherer" consisted of iron borings loosely placed in a glass tube. The glass tube was placed in a large copper "hat" to protect it from electrical influences other than the radiations. The radiator was cased so that its radiations were directed to the coherer, and the coherer was read by a reflecting galvanometer with which it was placed in circuit.

With these appliances there were shown :—The influence of the radiations on the conducting power of the coherer; the polarisation of the radiations by gratings of parallel copper wires, the vibrations in the direction of the length of the wires being arrested, the others passing on; the reflection of the radiations from bright metal surfaces, from wood, and from the human body; the transparency of glass to these radiations, and the refraction of the radiations by means of a prism of paraffin: all indicating the analogy of these radiations to those of light.

From such experiments came Professor Lodge's theory of vision, which, however, he put forward as the suggestion of a theory rather than its promulgation. Briefly put, his suggestion is that light may excite transverse vibrations in the rods and cones of the retina, that these vibrations may alter the resistance of some part of the eye and so cause the recording of the light stimulus, and that the stimulus corresponding to the process of tapping for restoring the coherer to its original state would be the influence of darkness. Between the stimulus of light and that of darkness there would be persistence of vision. These ideas were illustrated by means of the coherer, the restorative tapping being effected automatically through vanes revolving by clockwork.

In the subsequent discussion, Lord Rayleigh insisted on the necessity of the three-fold nature of colour-vision being taken into account in any theory of vision; Professors Armstrong and Fitzgerald naturally laid stress upon the physico-chemical side of vision and the resemblance of the retina to a sensitised plate; while Professors Rutherford and Schaefer discussed the anatomical structure of the eye as a coherer. But Professor Burdon Sanderson made what, in our view, was at once the most obvious and the most valuable criticism, when he pointed out that, while Professor Lodge's suggestion was worthy of consideration as a theory of the action of light upon the retina, it was not a theory of vision at all.

Mind and Body.

The point raised by Dr. Burdon Sanderson in connection with Professor Lodge's suggested theory of vision raises the perennial difficulty

of mind and body. In the question of vision this difficulty is strikingly obvious. We can see that light has a direct effect upon the protoplasm of many of the simplest animals and plants. Many single-celled forms of life, imprisoned in a vessel, darkened, for instance, by being covered with black paper except at one spot, will crowd towards the window of light; others will shun the light and assemble in the darkest regions. In many of them, again, when they are observed under the microscope by a beam of light reflected through them by the mirror of the microscope, the direct effect of light as a stimulus may be seen by their sudden movements when the mirror is flashed on them or turned off. From conditions so simple up to the complicated eye of the mammalia an infinite number of grades of complication in the arrangement of cells, specialised in the direction of sensitiveness to light, may be observed, while, again, there are similar series of grades of elaboration of accessory structures. Thus there are structures that seem to be mechanisms directly optical, like convex transparent lenses and corneas to focus the beams; curtains of pigment, perhaps corresponding to the velvet sheet of the photographer; pigments that are directly affected by the chemical action of the light, like the sensitised film of a photographic plate.

Some of these arrangements are fairly well understood; about the function and mechanism of others theories are numerous and divergent. But were they all understood, not vision, but the mechanism of vision, would be understood. The appreciation by the brain or by the mind of the sensational stimuli afforded it by the optical apparatus involves many things that are not optical. It involves memories of previous visions, memories of touch, memories of movements, and several other factors. The analysis of what we know as sight, and the correlation of the component elements with the results of chemical, physical, and physiological investigation of the eye, is one of the obscurest tasks of the psychologist.

REALISM IN ART.

As an obvious example of the difficulties attending the phenomena of vision, consider the dispute in art connected with what is called realism. We may dismiss, first of all, idealised compositions, pictures in which the imagination of the artist frankly transforms his impressions of Nature. Take three types of paintings or drawings, each of which are set forward by some and accepted by many as adequate representations of what may actually be seen. To the first of the three types belong the landscapes of Mr. Waller Paton, scenic arrangements like Mr. Frith's celebrated "Derby Day," or the later works of Sir John Millais. Round such as these one may notice a group of admirers crying, "See, it is Nature itself! Look at the sheep on the distant hill with their owner's initial in red paint on their fleeces, while on the grass-blades in the foreground behold here

a beetle, there a caterpillar crawling." To the second type belong the striking productions of the modern impressionists; to many these appear chaotic daubs; an increasing number of people believe that the impressionists alone have the secret of producing by their canvases the same impressions that Nature produces. The third type, which is that most astonishing to the Western mind, consists of the strange Art of China and Japan, an Art so incongruous with our ideals that, at the first seeing, we hold it a fantastic perversion of the real. Yet these yellow artists would say "lying prone on the ground we draw things as we see them, and the lines and marks of our patient brushes show our brothers what we have seen."

Leaving aside all questions of art and technique, it is plain that the same things appear in different ways to different men, and to different races of men. Some of these differences may depend upon difference in structure of the optical apparatus, and so may have a physical explanation; many of them seem more likely to depend upon differences in mind or brain, and are matters for the psychologist.

Physiological Apparatus for Plants.

Workers in plant-physiology will find a description of several new and useful pieces of apparatus in a recent number of the "Minnesota Botanical Studies" (Bulletin no. 9, part iv.), a publication of the Geological and Natural History Survey of Minnesota. The first, devised by A. P. Anderson, is a balance with a self-registering mechanism for recording the rate of transpiration and its periodicity (if any) during a certain time. It consists of a balance, one arm of the beam of which is lowered by the increase in weight of a calcium chloride absorber. The lowering of the arm closes a circuit, and an electro-magnetic mechanism releases a weight which falls into the scale-pan attached to the other arm. The scale is thus balanced automatically after an increase equal to the weight used has taken place. At the instant the weight is released it is recorded on the registering cylinder of a recorder. The whole is enclosed in a case, for protection from falling moisture.

A new electrical auxanometer and a continuous recorder are described by W. D. Frost. The former was devised for such delicate measurements as of growth in thickness of stems and fruits, but is equally efficient in measuring growth in length, while its extreme lightness and delicacy make it especially useful in measuring the growth of small plants. The frame-work of it is made of aluminium, and the whole instrument weighs fifteen grams. For measuring growth in thickness it may be attached to the plant, increase in size of which unwinds a thread from a pulley, while the turning of the latter, by means of a ratchet-wheel, alternately opens and closes an electric current. The recorder and the auxanometer may be

separated any distance, and the former may also be used with other pieces of apparatus where a continuous record is desired.

The Discoverer of Dionæa.

In the *Journal of Botany* for November the editor gives some account of William Young, an American adventurer of the last century, who first brought to England, in 1768, living specimens of *Dionæa*, the carnivorous plant commonly known as Venus' fly-trap.

Young seems to have been a pushing, uneducated man, who, by dint of impudent self-assertion, got himself appointed Botanist to their Majesties, in 1764, much to the disgust and surprise of honest folk, like John Bartram and others, who knew him at home in Philadelphia. Apparently his only other claim to distinction rests in a MS. volume containing about three hundred rude figures of plants, and a corresponding volume of specimens, both now in the Botanical Department of the British Museum.

Change of Address.

We beg again to draw the attention of our readers to the notice that appears under the above heading on the last page of the present number. It is hoped that the new arrangement, which will come in force with the new year, will have the effect of facilitating our own business and our dealings with the public by concentrating the Editorial, Printing, and Publishing Offices at one address, instead of distributing them among three as has hitherto been the case. It was well at first to place ourselves in the kindly hands of so well-known a firm as Messrs. Macmillan, and we should like to take this opportunity of thanking them for the assistance and courtesy that they have extended to us. But now that we are growing up and begin to feel our legs, we are bold enough to think that we can walk alone. Whether we can succeed or not must of course depend not on ourselves but on the number of hands stretched out to help us. Since the Review will still be conducted by the same editorial staff and on the same lines as heretofore, we appeal confidently to our readers, subscribers, and contributors not to desert us; indeed, we venture to hope that they will now aid us all the more both in purse and person. If only their sympathy be assured, we shall step from the cradle with a light heart.

I.

The Homes and Migrations of the earliest known Forms of Animal Life, as indicated by Recent Researches.[1]

THERE is no portion of the geological record more important to the biologist than that which contains the history of the earliest known forms of animal life, for whether he believes in the theory of evolution or not, he is there made to see that there is a remarkable similarity in the forms which constitute the earliest faunas wherever they have been discovered. In speaking generally of these faunas we do not, of course, mean that similar marine animals occurred in each area at the same exact period in the world's history; on the contrary, if we admit that the animals migrated then as now when the conditions were favourable or otherwise, such would be impossible; but what is meant is that they occupy in the geological succession relatively the same general position. For instance, where the faunas, which we may here call A, B, and C, are found in conformable strata, the order of their appearance is the same in each area. If the sections have been correctly interpreted, C has in no case been found to precede A, nor B to succeed C. Year by year we have been made acquainted with fresh discoveries among the older rocks, but whether the evidence comes from Australia, China, India, Europe or America the fact remains that the order of the appearance of certain forms of animal life in the succession does not vary from that which had previously been made out in other areas. In the most favoured areas the rocks can only be expected to contain a very imperfect history of the animal life which existed at the time when they were deposited; still, sufficient evidence has been obtained to make it easy to realise what were the dominant forms prevailing at the time. For instance, it is well known that the earliest faunas are everywhere characterised by having Trilobites as their dominant organisms, and further that each fauna is distinguished by well-recognised genera with a comparatively limited vertical range. As the earliest faunas of which we have any knowledge contain well-developed forms belonging to widely separated groups, it is clear that we are here far from the beginning of life on the globe. How is it then that, with all

[1] Read at the meeting of the British Association, Oxford, 1894.

the careful researches which have been made of late years, no well-recognised forms have been discovered in earlier rocks than the Cambrian ?[2] Certainly, only a few sediments have yet been discovered of earlier date than the Cambrian which could have been deposited under conditions suitable to the existence of animal life; but some have been found, and yet up to the present they have only yielded very doubtful traces of organisms. The great physical changes which affected a very large surface of the globe before the Cambrian rocks were deposited possibly removed or obliterated much important evidence; but on the other hand it is more than probable that sediments containing many earlier faunas than those at present known to us are now submerged in the great oceanic basins.

It is generally admitted by geologists that the ocean basins have been great depressions of the earth's surface from a very early period in the world's history, but this does not of course mean that the same amount of land has been under water from that time to this. We know, on the contrary, by geological evidence that, owing to repeated physical changes, the area covered by water has varied much in extent at different times from the Cambrian period to the present; and it seems but reasonable to suppose that it did so also in Pre-Cambrian times. These changes, however, have been mainly along the borders of the oceans; hence the strata which have been brought to the surface are all comparatively shallow-water deposits. From what has been stated it would be anticipated that the earliest known faunas would be likely to have a fairly wide distribution over the globe, and that they would contain such forms only as could live near shores or in comparatively shallow water and were not easily affected by changes of temperature.

If the earliest known faunas lived along the borders of the ocean basins, it is but natural to suppose that pre-existing faunas, from which they were descended, inhabited similar localities; and we are justified in supposing that these basins when first tenanted by animal life were very much more limited in extent than they were at the commencement of Cambrian time. The lowest Cambrian rocks, being everywhere deposited near shore or in comparatively shallow water, tell us clearly that the depression at that time was still going on, and that the waters were spreading gradually over fresh land-areas. Most of the evidence obtained of late years bearing on these questions has been accumulated through researches in Europe and North America; therefore I propose to confine my further remarks to those portions of the globe.

As the surfaces of the Pre-Cambrian continents of Europe and America were very irregular when the lowest Cambrian deposits

[2] The paper by Professor W. K. Brooks, of which an account was given in the October number of NATURAL SCIENCE (vol. v., p. 242), deals with this question in a very interesting and highly suggestive manner, and seems to me to explain some of the difficulties which meet the palæontologist in his inquiries.

were thrown down, it is only to be expected that the Cambrian rocks should vary considerably in importance in different areas, and therefore that the faunas also in some areas should be but poorly represented. Those parts which would be first encroached upon, if the depression was very gradual, as we have every reason to think it was, would contain the best record of the succession of life; and all recent researches have tended to show that the Cambrian faunas in the areas bordering the North Atlantic basin are the richest in the number of genera and species, and therefore contain the most perfect record of the succession of life in the early rocks hitherto obtained.

In a paper communicated to the Geological Society in 1875,[3] which referred mainly to the European areas, I stated that "the first fauna of which we have any knowledge occurs in the beds furthest to the west, and there in earlier beds than any which occur in the regions more to the east; and though the forms which make up this fauna belong to inferior classes, yet they are not the lowest types in those classes, but often show evidences of considerable progression in development. On this account I have often expressed the opinion that we were far from the beginning of this type of life even in the earliest Cambrian faunas, and that the forms had already undergone many changes previous to this period. It is easy now to see how these changes could have taken place, and moreover how it was that new forms so frequently appeared at certain stages highly developed and with no previous evidence in the rocks as to the changes they had undergone. The home of the earliest forms of life seems to have been somewhere towards the south-west, and possibly not far from the equator; and it is from here that the various forms seem to have migrated to the areas in which they were subsequently entombed. The migrations seem to have taken place towards the North-American continent very much about the same time as towards the European; and the sea-encroachments along that continent seem to have been in a direction from south-east to north-west, so that the lines indicating the two depressions would meet in mid-Atlantic. This accounts for the great similarity in the two faunas, and for the general resemblance offered by the order of succession of these early rocks in the two continents. The higher lands in America would be to the west and north-west, and the higher lands in Europe to the east and north-east; so that the last lands submerged would approach each other and occupy the same region of the globe."

When the above remarks were written, forms belonging to eight classes had been discovered by us in the Lower Cambrian (now called Lower and Middle Cambrian) in Wales; and in a paper in the *Quarterly Journal of the Geological Society* (vol. xxviii., p. 173, 1872) I stated that: "These same groups are also more or less present and tend to characterise these early deposits wherever

[3] *Quart. Journ. Geol. Soc.*, vol. xxxi., p. 552.

found; but no country has up to the present time produced a more varied fauna or a greater richness in types than England. Scandinavia has a larger number of species, but not so many groups." The classes at that time represented in the Cambrian rocks of South Wales were the following, viz., Spongida, Echinodermata, Annelida, Brachiopoda, Gasteropoda, Pteropoda, Crustacea, Trilobita. In the table, in the paper above referred to, seven classes only are mentioned; but since then it has been shown that the genus *Stenotheca*, which I had previously discovered in the Menevian rocks at St. David's, should be classed with the Gasteropoda.

Mr. C. D. Walcott, Director of the United States Geological Survey, in his very important memoirs, "The Fauna of the Lower Cambrian or Olenellus Zone,"[4] and "Correlation Papers, Cambrian,"[5] makes careful comparisons of the rocks on both sides of the Atlantic, which contain the Cambrian faunas, and says that "in lithologic, stratigraphic, and palæontologic characters, the Cambrian series on the opposite sides of the Atlantic are such that there is no hesitancy in considering them as belonging to one geologic group and as part of one geologic basin."[6] He further points out that the Lower Cambrian fauna, which he regards as a littoral fauna, is much more persistent in its character than that in the succeeding rocks, "for all the strata referred to the former horizon have been found to be characterised by essentially the same fauna." The Lower Cambrian fauna in America, according to Mr. Walcott, contains genera belonging to the following classes of invertebrate animals, viz., Spongida (4 genera), Hydrozoa (2 genera), Actinozoa (5 genera), Echinodermata (1 genus), Annelida (4 genera), Brachiopoda (10 genera), Lamellibranchiata (3 genera), Gasteropoda (6 genera), Pteropoda (4 genera), Crustacea (5 genera), Trilobita (15 genera).

Mr. Walcott's list contains a larger number of classes and of genera and species than is at present known in the Lower Cambrian fauna on this side of the Atlantic; but, with the exception of the Lamellibranchiata, all the classes are now known to occur in the Middle Cambrian either of Wales or Scandinavia. It will be seen that Trilobita and Brachiopoda are the dominant classes, and this is equally marked in the succeeding or Middle Cambrian fauna. The Trilobita, especially, give to these faunas their very distinctive characters, and the faunas are frequently referred to as the *Olenellus*, *Paradoxides*, and *Olenus* Zones after closely-allied genera, which also are the most typical occurring in them. The genus *Olenellus* occurs only in the Lower Cambrian, *Paradoxides* in the Middle Cambrian, and *Olenus* in the Upper Cambrian. The order in which they appear in the succession is the same wherever they have been discovered in all the American and European sections.

[4] Tenth *Ann. Rep. U.S. Geol. Survey*, part i., pp. 509–763, pls. 49–98: 1890.
[5] *Bull. U.S. Geol. Survey*, no. 81: 1891.
[6] *Op. cit. ult.*, p. 372.

It is a tempting field for speculation to enquire why this should be the case. It has been suggested that *Olenellus* was a form peculiarly adapted for living in shallow water and along shore-lines; *Paradoxides*, on the other hand, is a much larger form, and would require somewhat deeper and more open water. It is the dominant form in the Middle Cambrian, and attained to a size of over twenty inches in length, being the largest trilobite known. When the Middle Cambrian rocks were deposited, a considerable depression had taken place, and though the depth was not great, the sediments must have been deposited in a fairly open sea. *Olenus*, the typical genus of the Upper Cambrian, was of small size, and has the appearance of a degraded form of *Paradoxides*.

When I ventured to suggest that the "home of the earliest form of life seems to have been somewhere towards the south-west, and possibly not far from the equator,"[1] I was, of course, referring specially to the North Atlantic Ocean. The other oceans would doubtless be equally tenanted by life, and would form centres of dispersion. In these early oceans there would, of necessity, be forms of life suitable to the varying depths; but it is not to be expected that representatives of all these could occur in any of the earliest faunas with which we are acquainted. When deeper water faunas are found in the succession, there, as in the littoral faunas of the early Cambrian, abundant appropriate organisms suddenly appear, showing that they had occupied areas favourable to their existence contemporaneously with the earlier faunas entombed in shallow-water deposits. As an instance of this we may mention the sudden incoming of the very rich graptolite fauna of the Arenig rocks.

HENRY HICKS.

[1] *Quart. Journ. Geol. Soc.*, vol. xxxi., p. 555.

II.

Cephalopod Beginnings.

THE chief object of this article is to draw attention to some observations recently made in North America by Mr. J. M. Clarke of Albany, and published in the *American Geologist* (6, 7, 8); to make in passing such criticisms as may seem desirable; and to consider the relation of the new facts to the general question of Cephalopod evolution.

First, however, for the sake of those whose studies have not lain specially among the Cephalopoda, it may be advisable to explain a few of the terms employed.

The shell of a typical cephalopod, such as *Nautilus*, *Ammonites*, or *Orthoceras*, consists of a very elongate cone, which may be straight or curved or coiled, but which is always divided interiorly by transverse partitions, or 'septa,' into a number of chambers, or 'loculi.' Broadly speaking, these loculi gradually increase in size from the apex of the cone towards its aperture, not merely because the width of the cone increases, but because the septa are placed at a gradually increasing distance from one another. The shell, as thus described, may be called the 'conch' or the 'phragmacone,' the latter word meaning nothing more than septate cone. The soft body of the animal is lodged in the wide end of the shell, in front of the last-formed septum; and the cup-like space in which it lies is called the 'body-chamber.' That part of the body which abuts on the last-formed septum and which fills the bottom or hinder part of the body-chamber, contains the chief viscera of the animal, especially the generative glands, and is known as the 'visceral hump.' This hump is covered with a thin, glandular skin, which secretes membranes of an organic substance called 'conchiolin'; as these membranes become impregnated with lime (calcified), the septa and the inner lining of the shell-wall are thus formed. The formation of septa in the cephalopod shell is precisely parallel to the formation of septa in other molluscs and in other groups of animals, such as the tabulate corals; that is to say, the soft body with its posterior secreting surface moves forward at intervals in its cone or tube. As to the causes of this forward movement there has been much speculation, into which it is now unnecessary to enter. One thing, however, seems fairly clear, namely, that the formation of

regular septa was a gradual development from a primitive non-septate condition.

A peculiarity that distinguishes the septate cephalopod shell from the septate shells or tubes of most of the other animals above alluded to, is the fact that an extension from the soft body always passes backwards through the septa and, however long the phragmacone may be, always appears to maintain connection with its apex. In such a form as the modern *Nautilus*, this extension comes off sharply from the visceral hump in the form of a thin tube, which passes immediately through the centre of the last-formed septum and stretches back through the loculi and through the centres of all the preceding septa. This fleshy tube is known as the 'siphuncle.' It will, however, be noticed that, just before the siphuncle becomes attached to the visceral hump, that is to say just where it passes through the last septum, it enlarges slightly, so that between the visceral hump and the siphuncle proper there is a small cone, which contains a slight extension of the generative viscera, and which may therefore be distinguished as the 'visceral cone.' Were all cephalopods fashioned like *Nautilus* in this respect, there would be no need to use this term 'visceral cone'; but there are many in which the visceral cone assumes very large proportions and appears to have given rise to some misunderstanding in the minds of those who have described such cephalopods. If we define the visceral cone as that part of the soft body which lies posteriorly to the inner margin of the last-formed septum, but anteriorly to the narrow siphuncle, we shall at once render futile many discussions that have taken place, as, for instance, those concerning the presence or absence of a siphuncle in *Sepia*, and we shall have a ready explanation for many puzzling structures in archaic cephalopods, such as the 'sheaths' and absurdly named 'endosiphon' of *Piloceras* and *Endoceras*. These, however, are details rather foreign to the present purpose. It is enough to point out that, whereas the siphuncle proper does not secrete a definite shell and merely becomes impregnated to a slight extent with carbonate of lime, the visceral cone, on the contrary, retains to a very large extent, in many cases, the typical secretive power of the visceral hump, by which shell-substance is eventually formed.

Having considered the phragmacone, or conical-chambered conch, in sufficient detail for the object of this paper, we turn now to a structure to which, under Owen's name of 'protoconch,' much attention has of late years been directed. Examination of the phragmacone of *Spirula*, of *Belemnites*, or of any ammonite or, better, of such a loosely coiled goniatite as *Mimoceras compressum*, will show that at the apex of the cone, which of course lies in the centre when the shell is coiled, there is a little round bulb, usually separated from the conch by a slight constriction (compare Figg. 1, *a* and 2, *b*). The siphuncle passes through the end of the conch into the protoconch; but instead of retaining its simple tubular character, it generally

enlarges within the protoconch into a rounded swelling, which is not attached to the wall of the protoconch. In *Nautilus* the apex of the conch is well known to be of a different nature. There is no rounded protoconch attached to the end of the cone, but the outer wall of the first loculus is marked by an elongate scar, the 'cicatrix,' which is at a little distance from the actual apex and on the inner side of the coil. Correlated with the absence of the globular protoconch, there is always a passage left between the inner side of the first loculus and the succeeding whorl of the shell. In other words, the umbilicus is perforate; whereas in the closely coiled ammonites this space is entirely filled by the protoconch. Round the cicatrix the shell is perceptibly thicker, and forms a raised margin to it. Within the first loculus, the siphuncle is tubular and cylindrical, and is attached to the wall of the conch almost, though not exactly, opposite the spot where the cicatrix lies externally.

When attention had once been directed to this distinction between the protoconch-bearing shells of the Ammonoidea and of the forms allied to *Belemnites* and *Spirula*, on the one hand, and the simple conchs of the Nautiloidea, on the other hand, it was supposed by many eminent writers, such as Barrande and Branco, that an insuperable distinction had been found between the Nautiloidea and the Ammonoidea; and Barrande especially, with his anti-evolutionist opinions, was delighted to prove, as he thought, that the ammonites could not possibly be derived from the nautiloids. So far was this distinction pushed by some, and notably by that ingenious thinker Munier-Chalmas, that the ammonites, goniatites, and their allies were bodily transferred from their position with the Tetrabranchiata of Owen's classification to the opposite division, namely the Dibranchiata.[1] Others, of whom Waagen (16), though with some hesitation, was the first, have adopted the simpler and the safer course of doing away with the classification of Owen, which, so far as it applied to fossils, was based on characters of which we knew and for ever shall know nothing, and of substituting therefor a division into three Orders, based chiefly on the structure and relations of the shell. These three orders are (i) the Nautiloidea, (ii) the Ammonoidea, (iii) an order equivalent to the Dibranchiata of Owen, for which I have proposed the name Coleoidea (1, 2), but for which others have adopted the (as it seems to me) less appropriate and rather misleading name of Belemnoidea (15, 4.)

It would be going too far afield to consider the very remarkable views as to the relations of the Octopoda that are held by Steinmann (15), or the modifications of Steinmann's classification recently proposed by Schwarz (13). The observations of Hyatt concern us more closely. This suggestive writer maintained some time since (11), on the evidence of specimens from St. Cassian preserved in the

[1] See for instance Bernard's "Eléments de Paléontologie," reviewed in NATURAL SCIENCE, vol. v., p. 298; October, 1894.

British Museum, that two species of *Orthoceras* (a genus universally ascribed to the Nautiloidea) did indeed possess very visible traces of a protoconch in the form of shrivelled masses of calcareous matter, which overlaid and obscured the cicatrix. The shrivelling was supposed by Hyatt to indicate that the protoconch in these forms was only slightly calcified, that, in short, it was chiefly formed of conchiolin. This would similarly account for its destruction in so many individual cases, and for its final complete disappearance in the subsequent history of the Nautiloidea. Upon this theory the gap assumed by Branco and Barrande to exist between Ammonoidea and Nautiloidea was to a certain extent bridged. "There is," wrote Hyatt, "convincing evidence in the structures of [certain] Cambrian shells that the Ammonoidea, with their distinct embryos, arose from the orthoceran stock, and passed through a series of forms, in times, perhaps, preceding the Cambrian, which were parallel to those characteristic of genetic series among Nautiloidea, viz., straight, arcuate, gyroceran, and nautilian." Six years later, in his magnificent monograph on the Genesis of the Arietidæ (12), Hyatt considered that the accumulated evidence had become yet more convincing; one observation of special importance was that the longitudinal striæ, which are so characteristic of many young nautiloids, were believed to pass from the conch itself onto the surface of the shrivelled mass which Hyatt claimed to be the protoconch.

Now, since so much stress has been laid, by Hyatt and others, on these observations, it behoves us to consider them with great attention. Fortunately the critical specimens are preserved in the British Museum, so that it has been possible for my colleague, Mr. G. C. Crick, and myself to subject them to a prolonged examination with the aid of the microscope. I am compelled, before proceeding with this article, to formulate our conclusions, which have reference to the figures on page 10 of "The Genesis of the Arietidæ":—

Figures 1–3. This specimen shows absolutely no trace of longitudinal striæ; therefore it is impossible for longitudinal striæ to pass onto the so-called protoconch, a statement based solely on the evidence of this specimen. On the other hand, it shows distinct traces of concentric striæ, which would doubtless have been more distinct had the specimen not been rolled and rubbed. There is no great distinction in colour or texture between the so-called protoconch and the rest of the shell. There are, however, clear signs that the shell has been flaked off from the end, and it seems perfectly possible that it once extended over the so-called protoconch. There is absolutely no evidence that the supposed shrivelled remains of the protoconch rest upon the cicatrix, or that they extend over the apical plate; on the contrary all the appearances suggest that this wrinkled mass passes up from inside the cicatrix, and that it is merely a plug, or infilling of secondary calcite deposited within the shell during the life of the animal.

Figures 4 and 5 are more correct, but the distinctness of the protoconch is certainly exaggerated. There is, indeed, a slight constriction, but there is no sign of shrivelling.

Figures 6, 7, and 8. This specimen shows no definite protoconch and hardly a trace of constriction. There is an abrasion of the surface, as shown in Fig. 7, but the shrivelling is greatly exaggerated. The outline of Fig. 6 is correct, and shows a slight swelling a little distance from the apex.

In short, we see no essential differences between these specimens and that represented in Figures 18 and 19, in which the protoconch is said by Hyatt to be removed and the opening to be plugged by secondary calcite, and which is actually figured for the sake of contrast with the specimens said to retain the protoconch. In making these criticisms, which are based on independent observation, we have the advantage of following the opinion of the great palæontologist, Barrande.

It does not follow from the preceding remarks that the species of *Orthoceras* in question (assuming that they do belong to that genus) were unprovided with protoconchs at some period of their lives; nor does it follow that the little lump seen at the ends of the specimens in the British Museum may not be some representative of a protoconch. It may very well be the calcareous infilling of a thin-walled protoconch; but there is no reason for regarding it as the shrivelled remains of a conchiolinous shell.

If therefore the general conclusions of Hyatt were to be accepted, it became desirable that some concrete instances should be found, on the one hand, of undoubted Ammonoidea that should be straight like an *Orthoceras*, and on the other hand of undoubted Nautiloidea that should possess a definite protoconch. When such a form was found, it would obviously be a matter of some difficulty to decide whether it belonged to the Ammonoid or Nautiloid branch; and if it really were the case that no criteria could be discovered, then such a form might claim to be a true connecting link. Still, there are certain minute features which are generally held, in a rough way, to distinguish these two orders; such are the position of the siphuncular passage through the septa, the nature of the external ornament, the contour of the septum, usually called the suture-line, and the proportions of length and breadth in the conch itself. These are characters well known to all who have ever worked on Cephalopoda, they are the ready means by which we tell an ammonite from a nautilus, and, as they are given with more or less accuracy in all text-books of palæontology, we need not further consider them. Enough to say that any straight, protoconch-bearing cephalopod shell can, by their means, be referred with a high degree of probability to its proper order.

The first step in advance was made by Branco, who showed that the straight *Bactrites*, from the Devonian rocks of Germany, possessed a well defined protoconch of ovoid shape (5). The affinities of *Bactrites* had before this been a subject of some dispute: while some placed it near the goniatites, others regarded it as merely a close ally of *Orthoceras*, differentiated therefrom only by the marginal position of the siphuncle. It was natural that the discovery of a protoconch

should, at least for a time, turn the scale in the direction of its ammonite affinities. To this argument Branco added the fact of its resemblance to the young of *Mimoceras compressum* and the presence of a dorsal lobe in the suture-line. This leads us straight to the recent observations of J. M. Clarke.

At various localities in Ontario and Livingstone counties, New York, at a horizon corresponding with that of the German *Bactrites*, Clarke has found numerous specimens, representing three or four species, of straight cephalopod shells, all possessing protoconchs. Some of these he refers to the genus *Bactrites* (7), while another he prefers to regard as *Orthoceras* (6).

Concerning *Bactrites*, Clarke is doubtful whether the specimens described by Branco were really of this genus; but he is convinced that his own material "corresponds, even to its specific characters, with that upon which the genus was established." Assuming the correctness of Branco's reference, the descriptions now given by Clarke supplement and in some respects set right those of Branco. The protoconch of the American specimens is not ovoid, but "bubble-shaped, frequently a little unsymmetrical or directed to one side, very broadly sessile upon the end of the shell tube, from which it is separated by a sharp constriction" (Fig. 1, *a*). The position of the siphuncular passage, even in the first septum, is distinctly lateral (Fig. 1, *b*). Where the siphuncle passes through the septa, it is surrounded by backwardly directed siphuncular collars or 'septal necks.' These septal necks are often so inclined as to be attached to the shell-wall, so that when the shell-wall is removed, the outer portion of the neck is almost invariably carried away with it. Thus an apparent dorsal lobe is formed (Fig. 1, *d*); and it is on this lobe, which does not really exist, that Branco has based some of his argument as to the affinities of his specimens.

The specimen referred by Clarke to *Orthoceras* is briefly described (Fig. 2, *a*, *b*). "It consists of the first or apical chamber of the shell, to which the protoconch is attached. The upper end of the specimen shows the first septum (not counting the 'apical plate' separating the protoconch from the first chamber) to be circular and with a central sipho. The lateral walls of the first chamber taper rapidly to the plane of conjunction with the protoconch and its depth is about one-half that of the latter. The protoconch itself is semi-ovoid in shape," and tapers slightly towards the apex as does that of *Belemnites*. It is of large size, as compared with the protoconchs of *Orthoceras* described by Hyatt, and "shows no indication of shrinkage or other irregularity, and its distal extremity is perfectly smooth." The specimen is referred to *Orthoceras* because the siphuncle (as shown in Fig. 2, *a*) passes through the centre of the first septum, and is not lateral as in the associated specimens of *Bactrites*. Larger specimens referred to *Orthoceras*, but without

the protoconch, do occur in these beds. The large size and unshrivelled condition of the protoconch is supposed by Clarke to be due to the fact that it may have been "derived from a shell so young that atrophy and wrinkling have not manifested themselves as they may have done with the more mature development of the shell."

The caution of this writer is so extreme that it is a little difficult to understand exactly what he does think, and it would be hazardous to give his conclusions in any words than his own. "If," he says, "the evidence presented brings us to the conclusion entertained by many of the older palæontologists, that *Bactrites* is closely related to *Orthoceras*, this conclusion is attained, by means of data not before elaborated, namely, the existence in both of like protoconchs. This fact, fortified by the decisive evidence that in *Bactrites* the sipho is strictly intra-marginal, the formation of a dorsal lobe wholly casual, leads to the conviction that *Bactrites* is little else than an orthoceran nautiloid with a lateral sipho. Both Branco and Hyatt have suggested the probability of *Belemnites* having been derived from orthoceran stock. Hyatt demonstrates that the guard or rostrum in this genus is a hypertrophic secondary deposition about the earlier parts of the true conch, similar to the plug which is sometimes found filling the distal fractured extremity of the sipho of orthoceratites. The similarity of structure in the true conch of *Belemnites*, *Bactrites*, and *Orthoceras*, is now increased by the demonstration of like protoconchs in all."

It is not clear whether Clarke would deny the goniatite affinities of *Bactrites*, or at least ally it more closely to the nautiloid *Orthoceras* than he would to any ammonoid; or whether he believes in the existence of a straight protoconch-bearing ancestor for each order, *Bactrites* being the ammonoid and *Orthoceras* the nautiloid ancestor, and the coleoid ancestor being as yet doubtful. The latter view, or something like it, seems to me to be nearer the truth; but I fear it is the former that is held by Clarke. It is therefore well to point out that his new observations do not necessarily support that view. If the protoconch which he figures in *Bactrites* does not so closely resemble that of *Mimoceras compressum* as the one figured by Branco, at all events the whole initial part of the shell, with its "*Oncoceras*- or *Gomphoceras*-like swelling of the shell-tube directly above the protoconch," bears a very striking resemblance to the young of *Agoniatites fecundus*.[2] Of course no stress can be laid upon the backward direction of the septal necks, for, as this is the direction in the ammonoid young, it is also the direction that we should expect to find in the ammonoid ancestor. The lateral, even if not marginal, position of the siphuncle is a bit of positive evidence strongly in favour of the ammonoid affinities of *Bactrites*. The sculpture also reminds one of the more primitive goniatites rather than of a nautiloid. The evidence of the so-

[2] Barrande. "Système Silurien," vol. ii., Suppl., pl. 490, fig. ii., 1.

called *Orthoceras* (Fig. 2) is also not wholly convincing. In this case the central position of the siphuncle is not positive evidence, for, as shown by Branco, Hyatt, and others,[3] the siphuncle of Ammonoidea changes its position during growth. It is certainly open to us to suppose that an ancestral form may have had a central siphuncle, and on *à priori* grounds one would expect such to have been the case. Consequently this so-called *Orthoceras* may not be a true *Orthoceras*, and may not be a nautiloid at all. If this were the case there would be no grounds on which to base any argument. But, assuming for the moment that Clarke is right in calling it *Orthoceras*, then it tends to prove precisely what we anticipated, namely, that the ancestral nautiloid had a protoconch; but it certainly does not prove that *Bactrites* was a nautiloid, any more than it proves that *Mimoceras* was a nautiloid, which latter would be a *reductio ad absurdum*.

We pass now to Clarke's last paper, which he somewhat rashly entitles "Nanno, a New Cephalopodan Type" (8).

The specimens described were found in the Trenton series at various localities in Minnesota. Their combined evidence enables us to describe the complete shell as follows. The general outline of the shell may be gathered from Fig. 3, *c*; it appears to be assumed that this represents almost the entire length of the shell, but the evidence that such is the case is not satisfactorily brought out, and one would imagine that the outer shell-wall might have extended further forward than is shown in this drawing. The phragmacone, then, consists of a relatively wide tube, suddenly swelling and again diminishing towards the apex. The septa are concave and relatively close together. They are penetrated by a relatively wide siphuncular passage, which has an absolutely marginal position just as in the Mesozoic belemnites. "The septa," says Clarke, "are abruptly deflected immediately about the sipho." This may mean either that they are bent upwards on that side towards the shell-aperture, or that their margins are bent downwards around the siphuncle so as to enclose it in a tube formed of septal necks. Either of these explanations is consistent with the figures, and the latter agrees with what we know of most of these primitive cephalopods; it is, in fact, this 'neck-tube' that is usually, though wrongly, termed the siphuncle by palæontologists. Wrongly, because the siphuncle, as already pointed out, is a fleshy extension of the soft body, and an extension which, strictly speaking, contains none of the viscera. In the present instance the width of the passage is so great that we may well suppose it to have been occupied by a visceral cone, rather than by a simple siphuncle; and this supposition would explain the apparent shallowness of the body-chamber in this form as also in *Piloceras* (see Fig. 5, I). In figures 3, *b* and *c*, the siphuncular tube is seen to be constricted where it passes through the septa, and this

[3] This also took place in *Orthoceras* itself, as lately shown by Foerste, but that does not affect the argument.

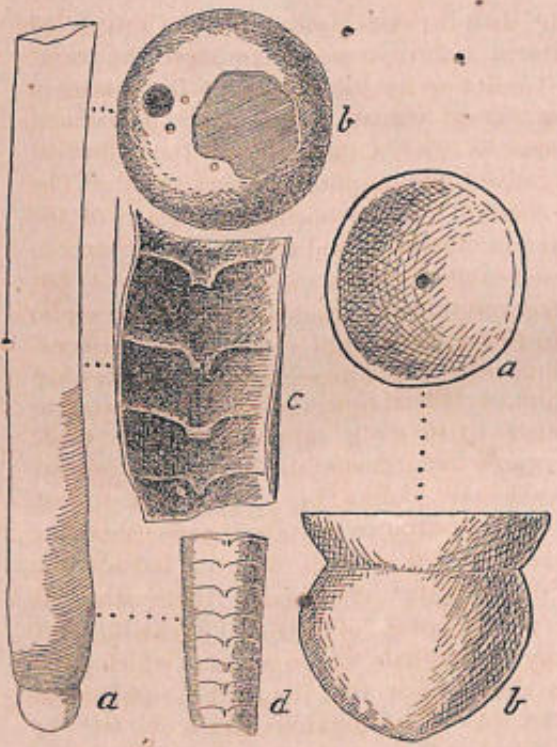


FIG. 1. FIG. 2.

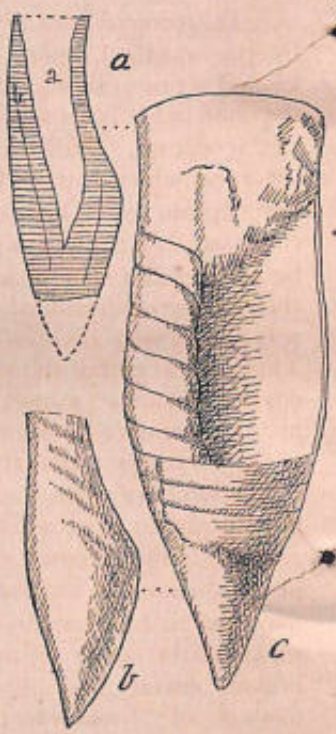


FIG. 3.

FIG. 1.—*Bactrites*. *a*. A shell retaining the protoconch and showing the expansion of the initial tube and the ornamentation (*B.* cf. *gracilis*). ×16. *b*. The distal surface of the first septum (somewhat broken), in *B. aciculum*, showing the lateral position of the siphuncular passage. The margin of the septum on the left is the line between light and shade. ×50. *c*. Interior of a portion of the shell of *B.* cf. *gracilis*, showing the intra-marginal position of the septal necks. ×4. *d*. Dorsal view of an internal calcareous cast of *B.* cf. *gracilis* showing the usual lobed appearance of the sutures. ×$\frac{3}{2}$.

FIG. 2.—*Orthoceras* (?), from Genesee shales. *a*. First septum, seen from above, showing central siphuncular passage. ×29. *b*. Protoconch and first loculus. ×35.

FIG. 3.—*Nanno aulema*. *a*. Vertical longitudinal section of the apical portion of the infilling of the siphuncular passage and protoconch; a. the internal cavity; b. calcite lining, with a streak indicating pause in deposition. *b*. A similar specimen seen from the exterior, showing swollen smooth apex and annulated siphuncular tube. *c*. The most complete example observed, showing siphuncular tube on left, constricted where it passed through the septa, lower down some septa, and then the conical apex. All ×1.3.

(All these figures are adapted from J. M. Clarke.)

also suggests that it was, during life, filled with some material that exerted pressure on its walls; the material would naturally be the generative glands.

The conical apex of the shell is not septate, but is entirely filled by the so-called sipho, which, before it passes through the first-formed septum, is greatly swollen so as to occupy all the space in the cone behind this septum, except a small wedge-shaped space next to the septum. This at least is what I gather from the following sentence, which I quote because I do not quite understand it. "The first septum seems not to conform to the contracted surface of the cone, which has a much greater obliquity, and there thus appears to be an irregular wedge-shaped cavity between these two surfaces, but there is no evidence whatsoever that the conical end of the sipho was in any way involved in this cavity except at its upper surface." This conical end of the siphuncular tube was covered by a very thin coating of shell, "a mere film." It has, however, been preserved to us, and preserved more constantly than any other part of the shell, owing to the fact that it became lined during the life of the animal by a secondary deposit of carbonate of line (Fig. 3, *a*). This deposit is very similar in structure and appearance to the well-known guard or rostrum of the belemnite; but, since it is inside and not outside the phragmacone, it cannot possibly be homologous therewith. It may better be compared to the plug of secondary calcite that repaired the ends of many individuals of *Orthoceras*, which were broken during life, or to the deposit that lines the siphuncular passage of *Actinoceras* (Fig. 6). Considerable stress is laid by Clarke on the fact that a break occurred during the deposition of this secondary calcite, so that it is divided into at least two portions "the line between which is represented only by a faint streak or difference in the texture." And this he regards as "indisputable evidence that the sipho enclosed or was composed of siphonal sheaths, as in *Piloceras*, *Vaginoceras*, and *Endoceras*." (See Fig. 5, I). This, nevertheless, I for one propose to dispute a little later on.

Before proceeding, however, with further criticism, it seems advisable to point out the very close resemblance that exists between these specimens now described by Clarke and specimens described nine years ago by Holm (10), which were found in beds of almost identical age in Oeland, Esthonia, and Prussia. A rough sketch (Fig. 4) made from some of Holm's figures by my friend A. H. Foord (9) is here reproduced, as it shows better than anything else could do the extraordinary likeness that obtains between the *Endoceras belemnitiforme* of Holm and the *Nanno aulema* of Clarke. In both we see the relatively close septa, penetrated by a wide siphuncular passage on the extreme margin (Fig. 4, *c*); the constriction of this passage by the septa, which bend downwards as septal necks and curve upwards towards the shell aperture on the side of the siphuncle; the swelling of the passage so as to fill almost, but not quite all the apex of the

cone; the thin shell-wall, and the lining of the apex by secondary calcite. The only differences to be detected from the figures are the cigar-like swelling of the slightly smaller phragmacone in *Nanno*, and the straighter external edge of the siphuncular passage in *Endoceras belemnitiforme*. These, however, are features of specific value at the very most, and it certainly seems that Clarke describes his specimens as representing "a new cephalopodan type," only because, in a state of oblivion most unusual with him, he has overlooked not merely Holm's elaborate paper but also Foord's well-known Catalogue of Fossil Cephalopoda in the British Museum. Whether Holm, who discussed the affinities of his species, will now be inclined to accept its generic independence must be left for him to say. But before these new

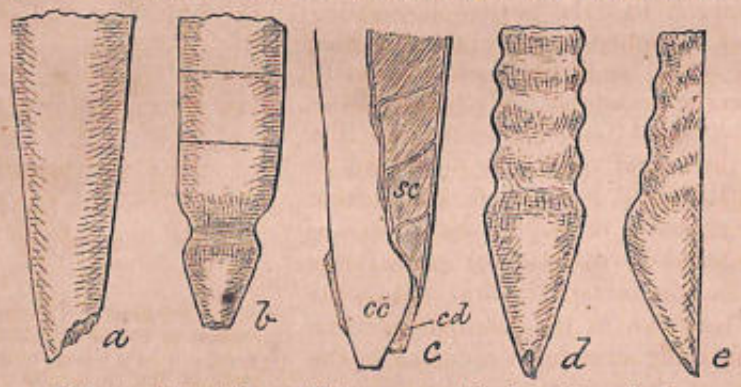


FIG. 4.—*Endoceras belemnitiforme*, Holm. *a*. Apical portion of phragmacone, covered with test. *b*. Internal cast of the same, denuded of the test, showing two septa. *c*. Sagittal section of the same, showing conical chamber (*cc*) at the apex of the siphuncular passage, some of the loculi (*sc*) separated from the siphuncular passage by the septal necks, and the secondary calcareous deposit (*cd*) lining the apex. *d*. Internal cast of the siphuncular passage and protoconch. *e*. Side view of same; compare Fig. 3 *b*. All × .6.
(Drawing by A. H. Foord, after G. Holm; the block kindly lent by Dr. H. Woodward, F.R.S.)

names find general acceptance, one would be glad of some hint as to their meaning. *Aulema*, every schoolboy will tell you, means a piece of music for the flute; but *Nanno*—what is *Nanno*? As a Japanese would say, "*nanno*?, of what?"[4]

To return to the meaning and structure of these undoubtedly interesting forms. There is, I submit, no evidence that *Nanno*

[4] Since this was in type, Dr. Clarke, whom I had informed of my sanguinary intentions towards his offspring, has kindly written me on the subject. He says, "I do not at all agree with Holm in placing the shell within the limits of *Endoceras* . . . the true *Endoceras* is septate to the narrowest apical extremity which has been observed." Also, "*Nanno* is a reactionary euphemism. I had thought at first to call it *Featherstonhaughoceras*, after an early explorer of Minnesota, but my mind gave way, and so I wrote it *Nanno*, though she is only a dead nymph who was fond of a flute." Dr. Clarke has clearly a larger circle of female acquaintance than was possessed by the compilers of my various classical dictionaries; and for this the gods should undoubtedly still be praised.

possessed 'sheaths' like those of *Piloceras* and some species of *Endoceras*. The faint streak described by Clarke is nothing more than one may see any day in a common belemnite guard, and merely indicates a slight pause in the deposition of the calcite, of no morphological significance whatever. The sheaths of *Piloceras*, well shown in Fig. 5, I, are clearly defined structures of a totally different origin. It may be worth while considering what that origin is. It seems pretty clear that in most of these archaic Cephalopoda the visceral cone extended to the depth of at least two loculi down the siphuncular passage. Since, like the visceral hump, it had the power of secreting shell, it formed, when in this position, long septal necks which often fused to form the neck-tube. We know that in *Nautilus* and *Spirula*, after the secretion of the septal necks, the outer coat of the siphuncle, both inside and outside the region of the septal neck, becomes hardened by calcium carbonate; this gives it a certain rigidity, and assists its retention in the fossil state. The same thing must have occurred in the coat of the visceral cone. Now, in *Piloceras*, when the animal advanced in the shell its viscera naturally followed it, and by suction the walls of the visceral cone were drawn in so as to form the narrow and empty siphuncle. At least such would have been the case had not the stiffness of the outer coat prevented complete yielding of the skin, especially at the posterior part where the siphuncle tended to begin, but where the coat was most calcified. It must therefore have happened that the inner layers of the skin were gradually torn away from the outer layers. Another stiffening of the skin would take place higher up, and the process would be repeated. Thus the 'sheaths' of *Piloceras* are nothing more than the remains of these old coats, left hanging on the siphuncle all the way up the passage (Fig. 5, I, *r* and *s*). The siphuncle is of course the structure that is often perplexingly called the "endosiphon." In some species of *Endoceras* the secondary calcification of the skin of the visceral cone seems to have advanced further, and the siphuncular cavity is divided into chambers

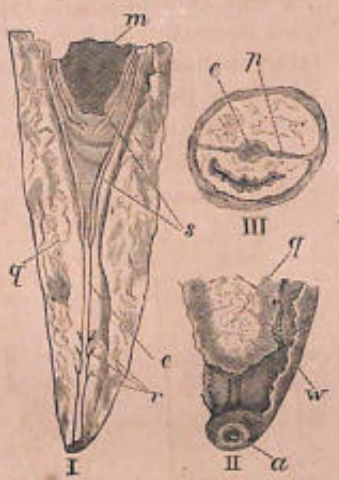


FIG. 5.—*Piloceras*. I. Longitudinal section of the siphuncular passage; *q*, quartz filling the greater part of the neck-tube cavity; *m*, matrix occupying the former position of the visceral hump; *s*, 'sheaths,' or calcified coats of the visceral hump; *e*, the siphuncle or so-called 'endosiphon'; *r*, remains of former sheaths (?). II. Posterior extremity of a similar specimen, much eroded, but showing an aperture, *a*, at the apex; *w*, the neck-tube; *q*, quartz infilling between neck-tube and one of the sheaths. III. Transverse section of a similar specimen, showing the outer wall or neck-tube and the siphuncle *e*; *f* is a partition the appearance of which is exaggerated and the significance of which is unknown.

(All figures of natural size; drawn by A. H. Foord; the block kindly lent by Dr. H. Woodward, F.R.S.)

by a series of hollow cones at irregular intervals. It is possible that the actual moment of casting off the slough was after the emission of the generative products, when the visceral cone was flaccid; this explanation coincides with Beeley's explanation of the origin of septation itself, but it is not exposed to the objections brought against the latter (14). There is, of course, no reason why "sheaths" should not have been formed in *Nanno*; but, to judge from the descriptions and figures of Holm and Clarke, no trace of them has yet been seen.

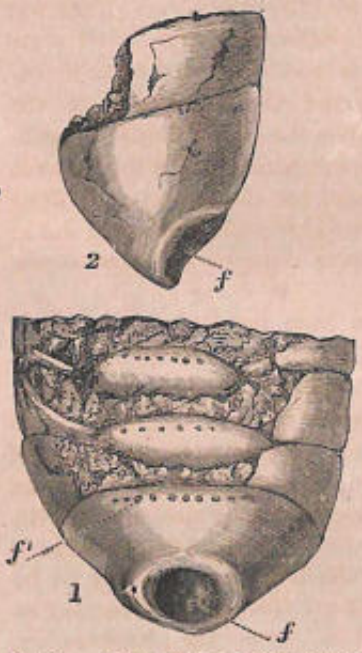


FIG. 6.—*Actinoceras*. 1. Front view of a weathered apical extremity, from which the shell has been removed. 2. Side view of another specimen. *f*, a large aperture at the apex; *f'*, the openings of small passages leading through the calcite infilling the neck-tube to the siphuncle, and probably representing the last relics of the attachment of the siphuncle to the wall of the neck-tube.

(Both figures natural size; drawn by A. H. Foord; the block kindly lent by Dr. H. Woodward, F.R.S.)

The feature of chief interest in *Nanno* is the apical swelling of the siphuncular passage. This was considered by Holm to represent the initial chamber, "Anfangskammer," by which he presumably meant the protoconch. Such, indeed, in spite of its large size, it appears to have been, and as Clarke observes, it probably contained the body of the young animal for a considerable period. That the forward movement of the young was quite gradual, is possibly indicated by the solidity of the calcareous deposit lining the walls of this apical chamber. If this suggestion be correct, it becomes interesting to compare this protoconch with the structures noticed above in the so-called *Orthoceras* from St. Cassian (see p. 425). The depression seen so clearly in Fig. 4, *b*, may well represent the constriction and the "shoulder" of the cicatrix alluded to by Hyatt in those specimens; while the plug of calcite in the *Orthoceras* is the diminished representative of the calcareous lining in *Nanno*. There is perhaps no importance to be attached to the fact that a swelling of the shell at a little distance from the apex occurs in *Nanno aulema*, *Orthoceras politum*, *Bactrites*, and *Agoniatites fecundus*; but it is certainly more worthy of remark than any resemblance to *Gomphoceras* and *Oncoceras*, such as Clarke insists upon.

Assuming that the somewhat similar structures seen in these forms are protoconchs in the strict sense, then it is clear that there is a great difference between them and such other primitive forms as

Piloceras and *Actinoceras*. As Clarke justly says of *Piloceras*, "were it possessed of such a solid apical sipho as is *Nanno*, that would be the part most readily preserved, as in this case." We have, however, more definite reason for believing that no such solid protoconch was present in *Piloceras* and *Actinoceras*. Figure 5, II., and figure 6 show at the end of the conch a wide aperture with a raised rounded edge. It is the opinion of Foord, with whom I heartily concur, that the siphuncle passed through this opening into the protoconch (9, pp. 159 and 166). The non-retention of the protoconch in a fossil state therefore seems due to the fact that it was composed of thin conchiolin, just as we have already inferred the protoconch of the ancestral Nautiloidea to have been; while the raised rim is remarkably like the rim of the cicatrix in a recent *Nautilus*. In these cases, not only was the protoconch more fragile, but the secondary infilling of the siphuncle tube did not extend into the protoconch as it did in *Nanno* and as it is here considered to have done in *Orthoceras elegans* and *O. politum*.

It appears a legitimate conclusion from the facts here quoted, that, however far back we trace the Cephalopoda, we still find two divisions: the one characterised by a relatively solid protoconch, often of relatively large size, and often preserved in a fossil state; and the other either possessing no protoconch, or one that was formed of material too delicate to be preserved, but characterised instead by an aperture with raised rim or by its degenerate representative, the cicatrix. To the former division are to be referred all the undoubted Ammonoidea and Coleoidea, while to the latter division are to be referred all undoubted Nautiloidea. We are therefore not entitled to say that the Ammonoidea were derived from the Nautiloidea, although we may not doubt that all three orders sprung from a common ancestral stock first evolved in far pre-Cambrian times. Of course it follows from this that the St. Cassian species referred to *Orthoceras* by Klipstein, as well as the specimen shown in Fig. 2, which Clarke refers to the same genus, cannot belong to the Nautiloidea at all, but are ancestral forms of the Ammonoidea and possibly of the Coleoidea. It is indeed probable that many other species hitherto referred to the loosely defined genus *Orthoceras*, or to some other genus of presumed Nautiloid affinities, will eventually have to find another resting-place. This is a question for those with more special knowledge than I possess.

Although in this article some adverse criticism has been bestowed on my Transatlantic friends, yet the facts that they have brought to our notice have a very real value. They have shown that the principles that have hitherto guided us have not been erroneous, and they have extended the basis of our argument by the width of a Hemisphere. I feel therefore some confidence in once more making a suggestion (3) originally placed before the Geologists' Association in March, 1888, but which, since it was not considered worthy of publi-

cation, did not find its way into print until quoted in the *Proceedings* of that Association in April, 1892 (4). The suggestion, put briefly, was that, at an early period, the Cephalopoda split into two great divisions: one division—which, starting with a very fragile protoconch, soon lost it altogether—may be called the *Lipo-protoconchia*, and is practically co-extensive with the Nautiloidea; the other division—which, starting with a stouter protoconch, preserved it, either by the coiling of the conch around it as in the Ammonoidea, or by its ensheathement in the mantle as in the Coleoidea—may be called the *Sosi-protoconchia*. And it was further suggested that such a classification, based upon ascertained facts of shell-structure, might prove superior to other classifications based upon suppositions as to the soft parts which could never be confirmed.

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F. A. BATHER.

III.

The Wing of Archæopteryx.

PART II.

I PROPOSE now to comment briefly upon a few of what seem to me to be the most important restorations hitherto made.

That of Professor (afterward Sir Richard) Owen first claims attention. This was the result of a careful study of the then recently-discovered fossil now in the British Museum. Bearing in mind the very fragmentary condition of the manus, this restoration must be admitted to be an exceedingly shrewd one. (See Figg. 1 and 2.)

We are told (14) that the "hand of Archæopteryx, besides being concerned in supporting the remiges or quill feathers of a wing, also supported two moderately long and slender free digits, each terminated by a strong, curved, and sharp-pointed claw. . . . The parts of the present skeleton show a certain amount of dislocation, and one of the claw-bearing digits may have belonged to the left [*i.e.*, right] wing." In the accompanying restoration, digit I. will accordingly be found to be marked with a ?, a doubt probably engendered by the obvious difficulty of accommodating the necessary carpal and metacarpal bones. We may, I think, safely predict that if Owen had had an opportunity of examining both the London and Berlin fossils at the same time, he would have given the London specimen a tridactyle manus, making his digit I. do duty for the penultimate and ungual phalanx of his digit III.; thus the wings of the two fossils would have been found to be precisely similar.

Of the Berlin example I wish to draw attention to at least six restorations. The first of these is that of Professor Vogt (20). In describing the manus he says: "The carpus shows only a single spherical bone." "On each manus (there are) three long slender digits, armed with claws" ". . . the radial digit or pollex (1) is the shortest, the other two are nearly of equal length, but the second is longest. These two digits were evidently united by tendinous and close aponeuroses; for in each manus these digits are placed in the same way, the one over-riding the other. The pollex is composed of a short metacarpal, a pretty long phalanx, and a terminal claw-bearing phalanx; the other two digits have, besides the metacarpal, three normal phalanges. The remiges were fixed to the ulnar side of the

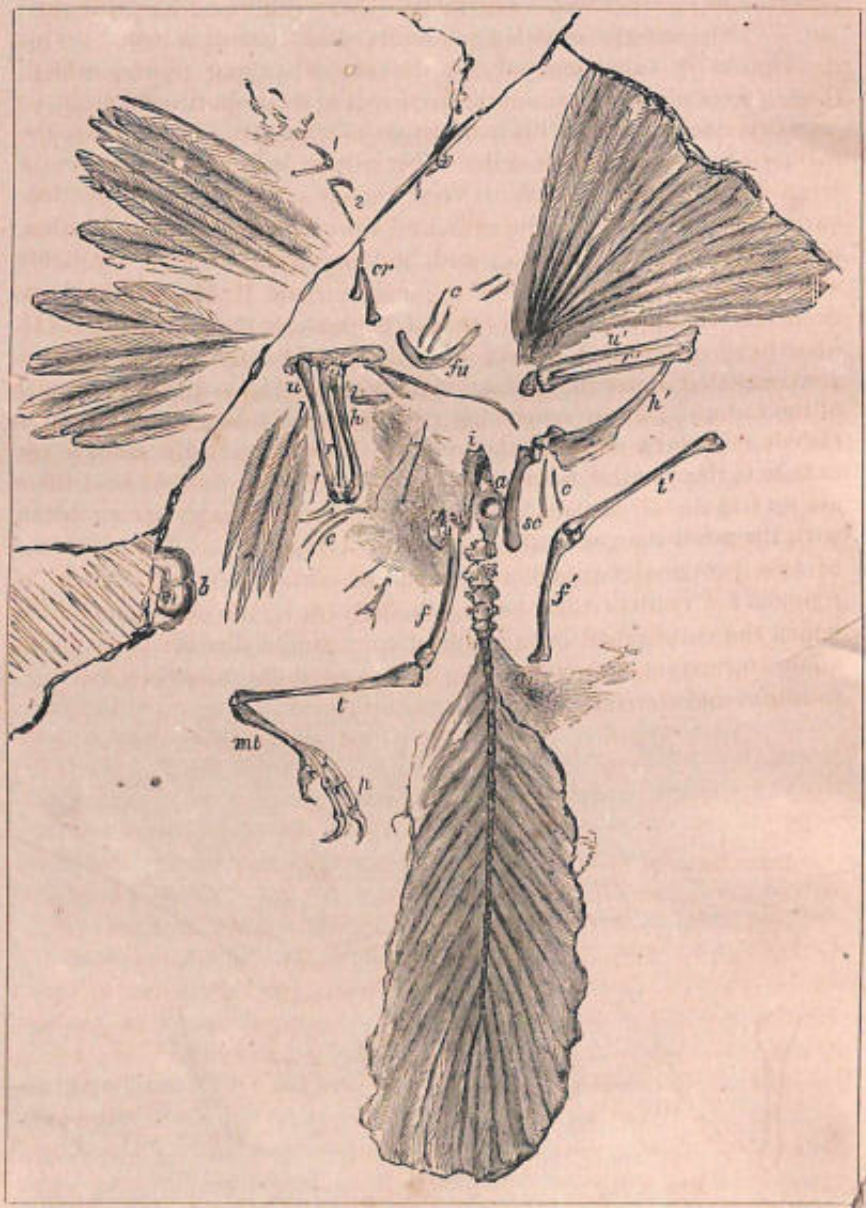


FIG. 1.—The specimen of *Archæopteryx* in the British Museum. About ¼ natural size. Explanation of letters:—*1*, *2*, portions of wing-digits, with claws; *cr*, metacarpals; *u*, *u'*, ulna; *r*, *r'*, radius; *h*, *h'*, humerus; *sc*, scapula; *fu*, furculum; *c*, ribs (partly sternal); *i*, feathers; *i*, innominate bone; *a*, acetabulum of the same; *f*, *f'*, femur; *t*, *t'*, tibia; *mt*, metatarsus; *p*, phalanges; *b*, supposed cast of brain.

The block kindly lent by Dr. H. Woodward, F.R.S.

forearm and manus, though no special adaptation to this end can be observed in the skeleton. The pollex . . . bore no bastard-wing. . . . The wing is rounded in outline, like that of a fowl." This description is supplemented by the accompanying figure, which, though somewhat diagrammatic, is correct in its proportions. (Fig. 3.)

Professor Dames in his exhaustive monograph (1) discusses the manus at considerable length. The carpus in particular receives a large share of attention. With Vogt, Dames agrees that only one free carpal bone is visible on the slab, and this, he says, from its position, must represent the radiale, and, judging from its size, probably afforded an articular face for the metacarpals I., II., and perhaps even III., though he suggests that it is probable that metacarpal III. may have articulated with a separate carpal—the ulnare, which is yet concealed under the stone. Attention is drawn to the large size of the radiale, Dames remarking that, while in adult living birds the radiale may vary in size relatively to the ulnare, in the embryo the radiale is the smaller bone, so that, coupled with the fact that there are no free distal carpals, the carpus of Archæopteryx agrees rather with the adult than with the embryo of living birds. The existence of two proximal carpals is considered almost certain, since he reminds his readers that, among modern birds, the only instances in which the carpus has been reduced to a single distinct bone[1] in the adult are instances in which the power of flight has been lost, *e.g.*, *Casuarius* and *Apteryx*.

FIG. 2.

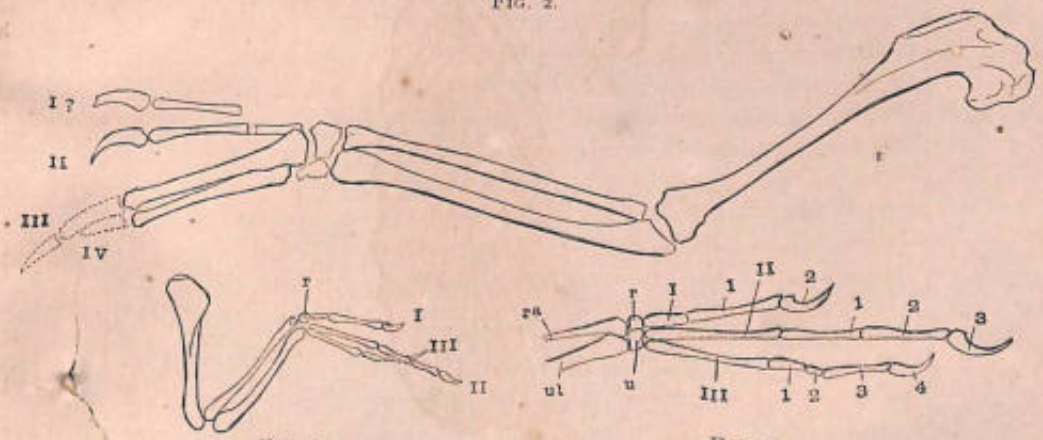


FIG. 3. FIG. 4.

RESTORATIONS OF THE WING OF ARCHÆOPTERYX.

FIG. 2. From the London specimen, after Owen.
FIG. 3. From the Berlin specimen, after Vogt.
FIG. 4. From the Berlin specimen, after Dames.

Further on Dames again refers to the carpus. Here attention is drawn to a groove running across the surface of the large free carpal

[1] The carpal in these cases is the ulnare.

(radiale) just described, which he suggests may—if it have any meaning at all—represent the boundary line between the radiale and ulnare, so that after all we may be dealing, not with one, but two bones, the homologues of the existing proximal carpals. As to the probability of a distal row of carpal bones, Dames writes: "There can be no doubt, speaking generally, that a distal row of carpals was laid down (angelegt), since they are laid down in every reptile, in every bird, indeed in every vertebrate, with the possible exception of the fish; and if this be admitted, the appearance of our specimen shows that they must have fused with the metacarpals." The nature of this view is next discussed. Quoting Rosenberg, he tells us that in the embryo of living birds the carpals 1–2 appear from the very first, not as separate elements, but as a single piece. Carpals 3–4 are similarly developed. The metacarpals[2] first approximate with their corresponding carpalia or their equivalents, and then fuse with them and with one another; but in Archæopteryx, we are reminded, these metacarpals are free: it is therefore open to question whether (i) the development of these carpals in Archæopteryx proceeded after this manner (*i.e.*, as carpals 1–2 and 3–4); or (ii) as carpals 1 and 2, 3 and 4; or (iii) whether the cartilaginous body, which Rosenberg supposed to represent 1 and 2, really only corresponds to 2, in which case the first metacarpal would articulate, not with a carpal of the distal, but of the proximal row—the radiale.

Now if my reader will turn to p. 353, Fig. I., he will see at a glance that Dames' interpretation of this region differs fundamentally from my own, inasmuch as what that writer believes to be the radiale I imagine to represent a fused row of distal carpalia, while the proximal carpals seem to me to be distinctly indicated, in the photographs, in the space between this distal row and the end of the forearm. On referring to the text, it will be seen that I have even gone so far as to suggest the presence of an intermedium. Now, supposing I am right, mark how much this matter will be simplified. Turning again to p. 353, it will be seen that in Fig. II.—the wing of the ostrich—we have precisely the arrangement of carpal bones that I believe to have existed in Archæopteryx, viz., two free proximal carpals—perhaps three—and a fused row of distal carpals. The difference between the two wings lies in the fact that in Archæopteryx the metacarpals were probably distinct from one another and from the distal carpal mass throughout life, whereas in the ostrich they become anchylosed when the adult stage is attained. Thus the carpus of Archæopteryx represents neither the early embryonic nor the adult stages of modern birds, but rather the late embryonic and post-embryonic stages. Although my view of the matter bears the stamp of probability, it cannot, of course, be accepted in any other light than that of a suggestion, and in this spirit

[2] Metacarpal IV. enjoys but a transitory existence.

I have submitted it to my critics, trusting that it may lead to a careful re-examination of the Berlin fossil, which, as I have previously stated, I have never seen.

Judging from Professor Seeley's description of this fossil (19), there seems a possibility that my rendering of the carpus may prove to be correct. He pointed out that it was a moot point whether the large free carpal described by Vogt belonged to the proximal or distal row, and in the same paper he says: "The middle metacarpal . . . so far as I can see, terminates proximally in a rounded carpal bone like that of a bird"; thus I imagine he believed, as I do, that the bone in question represented the distal carpals, and not the radiale, as Dames seems to think.

There seems to be some difference of opinion as to the number of phalanges in digit III., some describing three, others four.[1] Judging from the evidence of the wing alone, one would be inclined to think that there were not more than three, and that the appearance of four phalanges was due to the fact that the first phalanx of digit III. had been snapped in two across its distal third, the dismembered portion afterwards slightly drifting apart. Since, however, we get precisely the same effect in both wings, this can hardly be deemed the result of an accidental fracture; though it is, perhaps, equally hard to account for the fact that they happen to have separated in an exactly similar way in each wing. Though this arrangement of a short phalanx intercalated with two long ones is not unique, it certainly in this case appears in its most exaggerated form.

Professor Dames has supplemented his description by an outline restoration, Fig. 4; this, however, is probably not intended to do more than represent in a very diagrammatic way the salient features of the wing. It is strange, however, that he should have based this restoration upon Gegenbaur's figures of the developing wing of an embryo chick, since, as Dames himself shows, this eminent anatomist was deceived as to the real nature of the avian embryo carpus. In Gegenbaur's figure referred to, there is but one row of free carpals, which represent the radiale and ulnare. The ulnare is of great size and not only supports digits II., III., but contributes to that of digit I. There is no indication of a distal row, for Gegenbaur believed that at the time of the differentiation of the cartilaginous skeleton, only the radiale and ulnare were present, and that, even in their beginnings they showed no trace of fusion with other carpal elements. In Dames' figure accordingly, the ulnare entirely supports digits II. and III., while the radiale bears digit I. Thus the ulnare is much larger than the radiale, whereas Dames laid great stress on the large size of the latter of these two bones in the fossil; so large, indeed, did it appear to him there, that he imagined it must certainly have afforded an articular surface for digits I., II., and perhaps even III. It is probable that, in studying Gegenbaur's paper, the groove across the "large free carpal," which he at first dismissed from con-

sideration as probably having no real significance, came back upon him with new force, this time bringing conviction with it.

The remiges or quill feathers are briefly referred to. Of the primaries (metacarpo-digitals), Professor Dames tells us that they were attached, not only to the metacarpus, but extended down to the claw of the longest finger—digit II. The shafts of these primaries, he considers, having regard to their function as "Schwungfeder" (quill feathers), to be somewhat thin and slender. Here, again, it will be seen that my restoration does not quite agree with Dames' description. Turning to Pl. II., Fig. I., it will be noticed that the quills are supposed not to have extended beyond digit III., and I think there is every reason to believe that this was actually the case. The shafts of these quills are certainly slender, but not relatively more so than is the case with very many birds of active flight to-day. In the large plate accompanying Dames' paper, all the remiges are represented as having a proximal overlap, an arrangement exactly the reverse of that which I have supposed to obtain when discussing this matter in Part I.

Our fourth restoration is that given by the late Professor Romanes[3] (17), Fig. 5. It is difficult to know what to say of this, for it agrees with neither the London nor the Berlin fossil, and looks rather like the outcome of a process of mental evolution than the result of a careful study of the actual specimens. In the figure it will be seen that though the relative length of the digits roughly corresponds to that of the Berlin fossil, the hand, as a whole, is too short. All three fingers are represented as perfectly distinct, freely moveable independently of one another, and apparently were not supposed to bear any part in the support of the remiges. It is just possible, however, that the third digit may have been credited with supporting, say, two or three primaries. Not even with a great stretch of imagination can the outline of the wing be likened to that of the Berlin fossil. I have taken it for granted that this restoration is intended to represent the Berlin example, and is not a combination of this and the London fossil, since in Owen's restoration, there were four digits, of which I. and II. were separate and apparently freely moveable, while III. and IV. (resembling digits II. and III. of modern birds) were supposed to have supported the primaries. Had Owen's restoration been present before the mind of the restorer, it is possible that these digits III. and IV., with their primaries, would have been made to supplement what were evidently meant for the three digits of the Berlin specimen. The animal is thus described (p. 172): "The extinct Archæopteryx . . . presents throughout its whole organisation a most interesting assemblage of 'generalised characters.' . . . For example, . . . its still

[3] This, it will be remarked, is stated to be "after Flower." Sir W. Flower tells me, however, that he cannot recollect either having published such a figure or having used it in lectures.

unreduced digits of the wings which (like those of the feet) are covered with scales." There is no *evidence* of the presence of scales on the feet, though they probably were thus clothed, but there seems

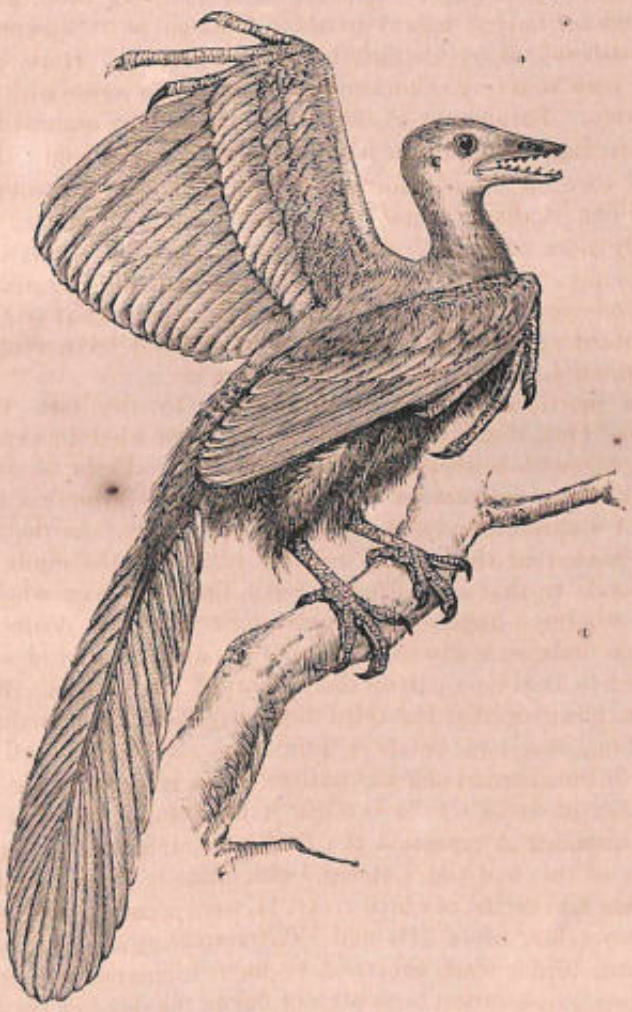


FIG. 5.—Restoration of *Archæopteryx*, $\frac{1}{6}$ nat. size, from "Darwin and After Darwin." The block has been kindly lent by Mrs. Romanes.

to me to be neither evidence nor *reason* to assume their presence on the fingers.

Dr. Hurst's restoration (6) took the world by surprise, but it remains to be seen whether it will stand the test of criticism. In order

that we may see the matter from Dr. Hurst's point of view, and thus enable ourselves to understand maybe the conclusion which he has arrived at, let us examine a few passages culled at random from his paper.

"The photograph accompanying this essay [reprinted on p. 351] shows the primary quills to have been supported by none of the first three digits, and justifies, if it does not even prove, the view that those quills were supported by the digits IV and V (or one of them). Portions of these large digits were figured by Owen thirty years ago, and they are seen in the London specimen and are quite unlike anything seen on the surface of the Berlin specimen, in which these digits probably still lie hidden."

Again: "If the dissected wing of a common bird . . . be compared with the plate of *Archæopteryx*, the conclusion that those two wings are essentially alike will be inevitable. It will be impossible to avoid the conclusion that the two digits which support the quills of the ordinary bird existed also in *Archæopteryx*, and their position will be seen to be indicated faintly in the photograph by a shadow which runs parallel with and behind the slender digits. The carpal angle of the wing will be seen in front of the carpal ends of the slender fingers, and from this point the outline of the anterior margin of the wing can be traced to the tip. This margin lies under those fingers. Not only did these, as their form and structure show, *not* support the quills, but they did not even contribute to the support of them. These fingers lie not in the wing at all, but *upon its feather-clad surface*. Those slender fingers, like the free fingers of the *Pterodactyla*, or of the recent 'flying' phalangers and squirrels, or of *Galeopithecus*, or like the pollex of a bat, are admirably adapted for climbing in trees. They proclaim *Archæopteryx* to have been a winged quadruped, . . ."

"Dames . . . states that the primary quills . . . were attached to the longest finger (II). If . . . we consider what would be the result of such an attachment, it must be obvious that it would be twofold. Firstly, the attachment of such a series of quills would render the fingers perfectly useless for climbing, and secondly, a single-flap of that wing would twist the phalanges off at the joints."

Now as touching Owen's restoration. I have already pointed it out as more than likely that the phalanges Owen marked as digit I.? were really none other than the ungual and penultimate phalanges of digit II. Supposing this to be the case, it will be seen that the wing of both London and Berlin specimens are precisely similar.

We may therefore assume that the remiges in both fossils would have been attached in the same manner. If it can be proved then, so far as proof is possible, that these digits *could* have supported the remiges, we shall have gone a great way towards demolishing, or at any rate rendering pointless, Hurst's elaborate hypothesis. I feel convinced

that I have already shown this to be more than probable, and it remains for my readers to decide whether I am right, or whether, with Dr. Hurst, we are to regard these digits as too "*weak-jointed*" to bear the "torsional stress" to which they would be subjected.

As to the slenderness of the digits, I have already pointed out that relatively to the size of the wing they are not more slender than is the case in living birds; further, in so far as the metacarpals are concerned, that of digit III. is stouter than obtains to-day. The wing of the young ostrich presents an interesting exception (see p. 353, Fig. II). Here metacarpal III. is a relatively stout bone, as in Archæopteryx, but differs therefrom in being more bowed. Indeed this wing seems to represent a stage in the evolution of the avian manus between those of Archæopteryx and modern Carinatæ, since it possesses a fused mass of distal metacarpals, approximated to, but not fused with the three free metacarpals, and since all its digits end in a claw. Another stage is afforded by those birds that have the third metacarpal reduced to a slender bar, and only digits I. and II. ending in a claw; while in the highest birds even the claws are lost. As both Professor Fürbringer and Dr. Gadow have already shown, it is very evident that the ancestral ostrich must have possessed the power of active flight, and this power must therefore have been acquired while the metacarpals were yet free, as in Archæopteryx. Thus, then, we have a chain of connecting links between the wing of this ancient fossil and those of the highest living birds.

The weak-jointedness of the phalanges hardly need detain us long, inasmuch as, so far as I can see, the phalanges of living birds seem to be scarcely if at all more firmly bound together than were those of Archæopteryx; indeed, remembering the strong tendinous fibres and bands which bind together both skeleton and the supported remiges, any special mechanism or anchylosis of the joints would seem to be unnecessary. The presence of the digits which supported the primaries, Dr. Hurst tells us, is indicated on the slab by a "shadow which runs parallel with and behind the slender digits." Among some notes on the subject taken in Berlin, which Dr. Hurst has generously furnished me with, I find the following reference to these digits and the shadow:—"Supposed position of IV. and V. wrong! Supposed shadow is a yellow stain—? iron, and the supposed IV. and V. if present are completely hidden." This last qualification, "if present," looks very like a doubt and but for the fact that in a letter lately to hand he says "I do not in the least think that what you brought forward at Oxford weakens my case," I should have believed that he had surrendered his position, and therefore have considerably curtailed my review of his paper.

Yet a few more words remain to be said as to these additional fingers. Supposing them to exist, where did they articulate? The whole of the carpal space is evidently monopolised by the "three free digits," so that there seems no room for two, or even one more

finger, especially since that digit (or digits) must have been larger and stronger than any of the three now exposed.

A very serious objection, to my mind, against the probability of Dr. Hurst's theory is the obvious fact that the manus of Archæopteryx must have been highly specialised in two directions to fit it for two distinct modes of locomotion—flight and climbing—modifications, too, which must have gone on hand in hand, a possibility which I can

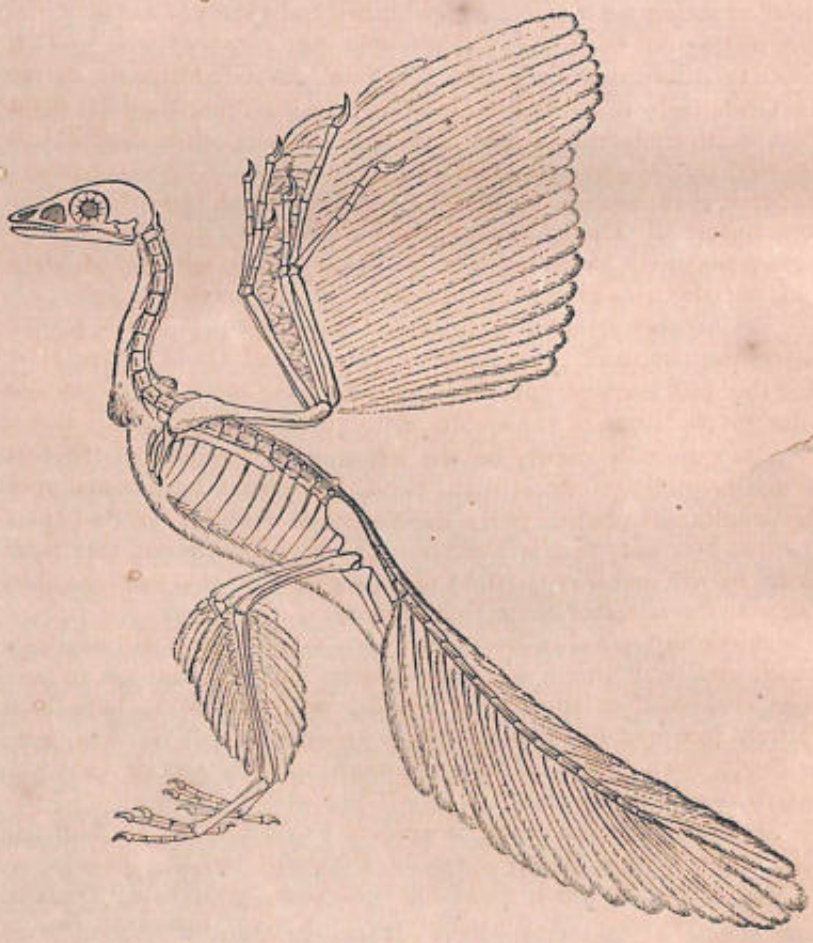


FIG. 6.—Reconstruction of *Archæopteryx*, from Koken, "Die Vorwelt"; by kind permission of the publishers.

hardly imagine. That the free fingers *have* become specialised, either for the purposes of climbing or those of flight, there can be no doubt, since no one would regard these digits as representing even a part of the unspecialised primitive manus adapted for ordinary walking or climbing purposes. In conclusion, I perfectly agree with Dr. Hurst when he says: "If Natural Selection has been operating long enough

and efficiently enough to determine the evolution of so perfect a series of feathers, it is perfectly certain that the same selection will have led also to the evolution of supports for those feathers as fully fitted to support the feathers as the feathers are fitted for flight; and even if there had been no indication of those supports on the slab, we need have still had no doubt as to their existence."

Professor A. Andreæ has recently distributed among his friends an outline restoration of Archæopteryx, which I have had an opportunity of seeing. Since it is not published technically, I am debarred from meting out to it the criticism that seems appropriate. Let it suffice to draw attention to the fact that the metacarpals are too short relatively to the length of the forearm, and that digit II. is too short relatively to the length of the metacarpals, while digit III. is too long by the same standard of measurement. This comparison, of course, is made with the Berlin fossil, from which these digits have been restored. The additional digits demanded by Hurst for supporting the quills have also been added, for the sketch was, as I am told, intended as a graphic interpretation of Hurst's paper.

Yet another restoration is that of Koken (10). This is a very interesting one, and is evidently founded upon Dames' hypothesis that the quill feathers "arose, not only from the metacarpus, but also from the phalanges of the middle of the three fingers, *i.e.*, II. It can be seen especially clearly on the left wing, if the quill of the first feather be prolonged towards the hand, that it must have rested upon the penultimate phalanx of the middle finger, close behind the claw." I think, however, that if Professor Dames will examine this fossil again, he will find that he ought to substitute the third for the middle finger as the recipient of the first primary.

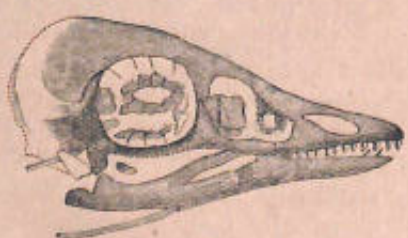
According to Koken the wing of Archæopteryx was still relatively small, and as Figure 6 shows, he believes the III. digit not to have been concerned in the support of the remiges, but to have been entirely free and functional. These remiges, it will be seen, have evidently been drawn without reference to their precise form and length as seen on the slab.

But for fear of becoming wearisome to my readers, I would fain pass on to review briefly a few of the more striking theories as to the manner in which flight has been acquired, having, of course, especial reference to Archæopteryx. I feel, however, that it would be wiser to leave this matter for a future paper. This course is the more advisable, since it is hardly possible to enter upon a discussion of this nature without taking into consideration the whole skeleton. Already I have found it difficult at times to confine myself to the narrow limits of the title of the paper. In such a discussion the reptilian affinities of Archæopteryx, real and imaginary, would necessarily take a large share, and this would mean straying from the point at issue as well as doubling the length of the article itself.

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Oxford. W. P. PYCRAFT.





ARCHÆOPTERYX.

FROM a photograph of the specimen now in the Berlin Museum, ⅔ natural size. Reprinted from "Lectures on the Darwinian Theory," by A. Milnes Marshall, with the kind permission of Dr. C. F. Marshall, at whose special request the figure is printed in the reverse position to that in which it is usually presented.

IV.

Pseudo-Biology.

OF all the pseudo-sciences that flutter abroad in this *fin de siècle* and attempt, often with too much success, to inveigle the unwary, none is so popular, none so fatally attractive, as that whose professors preach from the pseudo-biological pulpit. It is because it deals so largely with an assumed biology in its bearings on the conduct and progress of our selves, our nation, and our race, that it attracts us first, and that if we follow its will-o'-the-wisp, we founder in ignominious error. Its vogue is also great because so many of us poor mortals are wandering around in the darkness, having deserted the hand that formerly guided us and still searching for another; then to us wanderers there appear evangelists of a new gospel, which is superficially intelligible, and which we readily accept because we really understand it no more than do its preachers.

There has been lying on our table for some months, now looked at and now cast aside, a book by that extraordinarily popular Professor of this subject, Mr. Henry Drummond. It has been on our minds, at intervals, to give some review of this book, but we have never been able to decide whether we should be warranted in devoting any of our space to its consideration. And we only do so now in response to the request of numerous correspondents, whose minds seem, not unnaturally, to have been disturbed by the diverse criticisms that this work has called forth.

The title of the work in question is "The Lowell Lectures on the Ascent of Man." The Lowell Lectures are, we believe, delivered in Boston, one of the chief intellectual centres in the United States; and we doubt not that the audience attending them would be worthy of any lecturer in the world. Remembering this, we have, often and often as we perused Mr. Drummond's pages, wondered with a mirthful and a sorrowful wonder to think of our cultured Bostonian friends and the keen youth of Harvard sitting in solemn rows, agape and aghast at the meretricious and fallacious periods of this pseudo-biological pulpiteer. What did the cute sophomores from Cambridge make of this marvellous utterance?—"Run the eye for a moment up the scale of animal life. At the bottom are the first animals, the Protozoa. The Coelenterates follow, then in mixed array, the Echinoderms, Worms, and Molluscs. Above these come

the Pisces, then the Amphibia, then the Reptilia, then the Aves, then—What? The Mammalia, THE MOTHERS. There the series stops. Nature has never made anything since . . . the one motive of organic Nature was to make Mothers. Ask the Zoologist what Nature aspired to from the first, he could but answer Mammalia—Mothers." And so on. Surely, like the old maid in the story, those young men from Harvard shouted back to the Professor, "SPEAK FOR YOURSELF, SIR!"

But, after all, there are many worthy people, especially among the purchasers of Christmas booklets, who adore Mr. Drummond's eloquence, so we will not mar the goodwill of the season by over-insistence on our own squeamishness. Only, if the odious comparison be for a moment allowed, we would venture a word of praise for the sober and lucid style of the professor's rival in this field, Mr. Benjamin Kidd. A book may be written well or ill; its solid scientific or, if you prefer it, theological merit is not thereby affected. It is when rhetoric usurps the place of reason and leads to an irreverent disregard of truth that we have a right to complain of its abuse. And it cannot be denied that there are in this book many mistakes, perhaps of no great importance, but which, when found in the writings of a professor, it is kinder to ascribe to rhetoric than to ignorance. Here is a curious instance (p. 14), "The man who is busy with the stars will never come across Natural Selection, yet surely must he allow for Natural Selection in his construction of the world as a whole. *He who works among star-fish will encounter little of Mental Evolution*, yet will he not deny that it exists." We should think not, indeed. Has Mr. Drummond ever heard of G. J. Romanes, whose work "Jelly-fish, Star-fish and Sea Urchins" was distinctly subservient to his work "Mental Evolution"? Here is another case, more patent and less excusable (p. 240), "The Birds took one road, the Vertebrates another; the Vertebrates kept to the ground, the Birds took to the air." There are, it is true, people who still insist that "an animal" must have four legs and a tail; but surely even a Professor of Pseudo-biology might be expected to put the Birds among the Vertebrates. Here again are two statements, either of which is questionable, but which, occurring in adjacent chapters, also involve a contradiction: on page 245 it is written, "The first law of Evolution is simply the first law of motion. 'Every body continues in a state of rest, etc.'"; but on page 309 it is written, "The first commandment of Evolution is 'Thou shalt mass, segregate, combine, grow large.'" We hope that Mr. Drummond is obeying the former rather than the latter, we had liefer see him in a state of rest than growing large.

Mr. Drummond will complain that we are trifling with him and not arguing. That is so; but then Mr. Drummond should not trifle with us. The lapses to which we have referred will mislead no sensible person; it is merely inexcusable that a sensible person should

be subject to them. But there are other erroneous statements, which often form the bases of arguments, and which would naturally deceive the lay reader. The following sentence (p. 22) is an important link in one of Mr. Drummond's main chains of argument, viz., that proving Altruism to be the "direct outcome and essential accompaniment of the reproductive process." The sentence is, "Without some rudimentary maternal solicitude for the egg in the humblest forms of life, or for the young among higher forms, the living world would not only suffer, but would cease." There are sea-urchins that carry their young in a little pouch on their backs, there are fish that make nests, there is a toad whose back is pitted with cells in which the little toads develop; but where is the maternal solicitude in the common *Echinus*, in the fishes of the sea, or in the frogs of the marsh? No one would deny maternal care to be a great aid in evolution, serving as it does to transmit the experience of a lifetime; but it is stretching a good many points to say that "for a time in the life-history of every higher animal the direct, personal, gratuitous, unrewarded help of another creature is a condition of existence." One begins to suspect that this professor of pseudo-biology has never seen so much as a tadpole. Then again, in the chapter on the evolution of language, we are treated to this remarkable pronouncement, "Any means by which information is conveyed from one mind to another is Language. And Language existed on the earth from the day that animals began to live together. The mere fact that animals cling to one another, live together, move about together, proves that they communicate." So doubtful is this that one might even take so highly developed an animal as the Rosy Featherstar of our seas, and ask Mr. Drummond what real proof he has that the individuals of its floating colonies communicate with one another.

The preceding instances should show the admirers of this latter-day pamphlet-writer that they ought to be careful in accepting his premises as gospel-truth. And if they are not yet convinced, let them ask some biological friend what he thinks of the following observations. "It is one of the most astounding facts of modern science that the first embryonic abodes of moss and fern and pine, of shark and crab and coral polyp, of lizard, leopard, monkey, and Man are so exactly similar that the highest powers of mind and microscope fail to trace the smallest distinction between them" (p. 79). Astounding indeed! Small wonder that in the description of the development of the human embryo which immediately follows, Professor Drummond definitely introduces the formation of a gastrula by simple invagination. This prepares us for the description of the simplest case of cell-division, on p. 282, "At one moment it will call in matter from without, and assimilate it to itself; at another moment . . . it will set a portion of that matter apart, add to it, and finally give it away to form another life. Even at its dawn life is receiver and giver; even in protoplasm is Self-ism and Other-ism." The only difficulty is

that, let Professor Drummond study his divided cells for ever so long, he will never be able to tell us which is the Self and which is the Other—

> For each of them is born a twin,
> And not a soul knows which.

Or what will the biologist say when, to impress upon us his idea that all sociability depends on Sex, Professor Drummond writes, "There is no instance in Nature of Division of Labour being brought to such extreme specialisation" as in the evolution of Sex? He will remind us of the colonies of jelly-fish, in which some individuals do nothing but swim, others nothing but eat, others nothing but produce their kind, while one serves as a swollen bladder or float to support the whole colony; or of the coral-like colonies, where some individuals are mouths and some are only fingers to feed the mouths; or of the sea-mats, in which some of the polyps in a colony are nothing but tweezers to hold on to the food, while the other individuals eat it; or if colonial animals be put out of court, he will adduce the ants, with their castes of soldiers, workers and the rest, and the bees, with workers and drones and a single queen. All this specialisation has nothing to do with Sex, and yet in many cases Mr. Drummond's very words are applicable; each individual has "so entirely lost the power of performing the whole function that, even with so great a thing at stake as the continuance of the species, *one* could not discharge it."

This last quotation and the criticism of it that any biologist might be expected to offer, lead us directly to Mr. Drummond's main argument, "the fundamental omission," as he calls it, from all previous theories of the world, and also show us how greatly Mr. Drummond has overstated his case. We are told in the first place that all except a few powerful intellects, such as "Le Conte, Mr. Herbert Spencer, Romanes, Miss Buckley, and Prince Kropotkin," have taken the Struggle for Life to be the all-important factor in the Evolution of living beings. Drummond (let us drop the unnecessary "Mr.") insists that the Struggle for the Life of Others is equally important. The Struggle for Life is connected with the function of Nutrition, with the struggle for food, which alone has been considered heretofore. The Struggle for the Life of Others depends absolutely on the function of Reproduction. This great discovery, which is to "readjust the accents," must have occurred to Drummond very shortly after reading a book, not altogether unknown, entitled "The Evolution of Sex," and written by those original thinkers and writers, Patrick Geddes and J. Arthur Thomson. To insinuate, or even to suppose, that our leading writers on these subjects have overlooked the principle of altruism throughout the animal kingdom, evidences a remarkable state of mind. Everyone has long recognised it, and everyone would be prepared to grant to the various actions of animals grouped around reproduction, whether the attraction of the male to the female, or the tending of the offspring by the mother, a large share in the fostering of an altruistic

spirit. But Drummond rides a willing horse too hard. He tries to make his phrase cover many things to which it is not applicable. What does he mean by the "Struggle for the Life of Others"? On page 39 it appears to mean nothing more than the struggle to produce, the tendency to grow fat and multiply. But this is not an obvious meaning of the phrase. The fact is that, in ascribing all animal acts to the two functions of nutrition and reproduction, Drummond overlooks the inner impulse of life that pervades all living beings, the joy of life, so wonderfully described by Mr. Hudson in "The Naturalist in La Plata," the Over-Soul as Emerson calls it, the excess of vitality that is the very spur to sexual love and the essence of all reproduction. There is no "Otherism" here; it is the intensest of "Selfism." There is hardly a passion in all animal life so purely selfish as this one, which Drummond places at the base of the Struggle for the Life of Others. Moreover, this Over-Soul it is that produces over-population, which is the great goad of progress. But this is the very thing that makes the Struggle for Life keen, and he is either very bold or very foolish who would try to capture this for the side of Altruism.

Surely the truth is, what we thought everyone recognised long since, that the altruistic principle springs chiefly out of the gregarious habit, which has not necessarily anything to do with reproduction. Regarded in this light the Struggle for the Life of Others is merely a development of the Struggle for Life itself. But here is the point which so many of these pseudo-biologists seem to miss. The individual is not the unit of evolution. The unit is the Species, and, as Mr. Kidd has so well pointed out, the interests of the species are directly opposed to the apparent and immediate interests of the individual. In the lower animals, where reason does not come into play, there is nothing to run counter to the forces that make for the progress of the species, and these have produced the subservience of individuals to the colony or tribe in those cases above referred to. Even in the human species, we should in many cases be inclined to reverse Drummond's idea (p. vii.) that Family life precedes the Tribe.

It is when we turn to rational man, that we see difficulties in the way of further altruistic developments. Man is an egoist as well as a gregarious and political animal. He is an individualist by reason, if not by nature. Mr. Kidd's point appears then to be this. Religion is neither true nor false, natural nor supernatural,—merely the apotheosis of the principle that governs the life of all herds, families, tribes, nations, viz., self-sacrifice and the subordination of the individual. The apotheosis of this altruistic principle gives it what Mr. Kidd calls a "sanction," which, so far as the individual is concerned, is "ultra-rational." The philosopher understands the drift of altruism, he sees its origin and its necessity. Not so the hungry man in the street or the forest. He needs something outside to keep him in the path of allegiance to the race. This is religion,

the "something not ourselves that makes for righteousness," which, whether false or true, as we now choose to understand falsehood and truth, has been the motive force in the rise of many nations and of vast empires. This is the force, strictly subordinate to the great principle of Natural Selection, and subject like all else in this world to the process of Evolution, this is the force that has worked side by side with the simple struggle for the life of the individual, to produce the world as we see it to-day, with its men and its women, its fathers and its mothers, its citizens and its patriots.

But Drummond understands neither this, nor Mr. Kidd, nor even Professor Huxley. Wresting from its meaning a sentence by the latter, he tells us (p. 4) that "Evolution is simply 'history' . . . neither more nor less than the story of creation." This is disingenuous: Evolution is more than this; it implies, as Huxley proceeds to say, "a series of genetic changes," "progressive differentiation," and "the growth and modification of primordial germs." "Evolution," says Drummond again on p. 20, "began with protoplasm and ended with Man." Who is this person, so ignorant of Laplace and Kant, of Lockyer and Crookes? Who is this presumptuous prophet that sets a limit to the thoughts of God? After this we do not wonder that every thesis of this chatterer should be vitiated by his idea of his own self-importance. *He* knows the end of Creation, and so he tells us that, "the beginning must be interpreted from the end": the cart, with Professor Drummond holding the reins, must be put before the horse. We prefer to wait; the end is not yet.

With its cheap philosophy and its reading into the lower world all the passions and ideas of our own highly-organised consciousness, this book is a sad example of teleological argument. Teleology it is that causes Drummond to speak of "the gradual tempering of the Struggle for Life. Its slow amelioration, &c." The nature of the struggle changes, it passes to a higher plane; but the fight is not less keen, the pain not less bitter. Let this Professor in his chair contrast the social wreckage of our race, the tramp, the prostitute, the starving seamstress, with the jelly-fish stranded on the beach or the larvæ swallowed at a gulp by the monsters of the deep, and then let him dare repeat to a despairing world, that the harsher qualities of the struggle are passing away!

Our author is pledged by his preface to be original, and this is how he effects his purpose. "One has only to read the average book of science," he writes on p. 42, "to wonder at the wealth of knowledge, the brilliancy of observation and the barrenness of idea. On the other hand, though scientific experts will not think themselves, there is always a multitude of onlookers waiting to do it for them. Among these, what strikes one is the ignorance of fact and the audacity of the idea." "The Ascent of Man," then, is original because it falls under neither of these censures; for it combines "ignorance of fact" with "barrenness of idea." F. A. BATHER.

SOME NEW BOOKS.

The Naturalist in India.

A Naturalist on the Prowl, or in the Jungle. By Eha. 8vo. Pp. xii., 257. London: Thacker & Co., 1894. Price 8s. 6d.

"For their first introduction to the public," writes the author in his preface, "these papers are indebted to the *Times of India*." The editor of that newspaper is clearly a discerning person, and the author certainly cannot be mistaken in thinking that he "detected on the kind countenance of the public" much approbation of the articles—at least that is our opinion after reading this charming volume. "Eha," whoever he may be, has clearly some leisure which he has well spent in communing with nature in India. Nature in India is much more of a "rum'un" than she is at home here. She has produced, for instance, a serpent, the King Cobra; this *Ophiophagus*—whose bad example of eating other snakes has been recently followed at the Zoo by the Boa, proving the adage that evil communications corrupt good manners—is one of the few wild beasts that will deliberately go out of its way to attack a man. The power of the human eye may be a delusion, but there are not many wild animals that take the initiative in declaring war upon our species. The particular Hamadryad upon which nearly a chapter of the book is written was discovered coiled up upon a tree, and was at first asserted by the natives to be a Python. The reason for this varnishing of the truth was that the native preferred that "Eha" should dislodge the brute. A glance at the face of the reptile at once dispelled the python theory. "A python's eyes look nowhere in particular," we are told, "this creature's eye met mine with a truculent stare like nothing I had seen before." Some more information about the Hamadryad is given which runs as follows: "On the way I learned some things about the King Cobra which are not generally known. So swift is it that, when it pursues, escape by flight is impossible. When it has caught a man it swallows him whole, then climbing a tree, it winds itself round the trunk and tightens its coils until the man is crushed all to nothing in its inside. Thus it digests him. Facts like these are becoming increasingly rare in books. You must glean them among the simple folk who spend their lives with the beasts of the field and from infancy hold converse with nature's charms and view her stores unrolled. I have gathered many such very wonderful and known to few." It is not often, we must add in bare justice, that "Eha" is so newly-humorous as in the above passage.

A chapter entitled "The Voice of Mirth" gives the author the opportunity of saying something about the Cicada. The present writer has himself heard the "protracted ear-rending scream" of this insect, which is far worse than that of any other insect known to

science. Here it is the male who indulges in the shrill music. "Happy is the Cicada," as the ancient and misogynistic philosopher remarked, "in that he has a voiceless wife." "Cherchez la femme" is the explanation that the author, unlike others, is not altogether disposed to adopt in explanation of the sounds. He thinks that the vibration caused to the body by the noise is probably pleasing to the Cicada himself, and that he sings purely for his own amusement. It has been remarked that in the east there is no object in the sluggard fatiguing himself by going to the ant; the ant comes to him in some force. Chapter xvii. commences with one of Mr. Sterndale's excellent and humorous sketches of the author in the very act of grappling with ants upon his neck and ants upon his leg. The red ant of the east is one of the drawbacks to residing there. A study of the ways of this particular species is not conducive to the reception of moral lessons of any great value. They wait for you, "dancing with excitement on the point of every prominent leaf," as you take your walks abroad; the older members of the community have even, according to "Eha," an accurate anatomical knowledge of the human body, and at once make for such points as are more especially tender. Everybody knows that in this country there are ants which foster Aphides in order to feed upon the sweet secretion produced by them. The little black milkmaids throng round the little green cows, tap them gently with the antennæ, and drink up the milk which they then furnish. The red ants in India keep a butterfly caterpillar of more than an inch long for the same purpose. In gratitude for past favours, the chrysalis is carefully watched over, and the resulting butterfly allowed to fly away in perfect safety.

This is a sample of much new and readable information.

INDIAN GEOLOGY.

A MANUAL OF THE GEOLOGY OF INDIA. Chiefly compiled from the Observations of the Geological Survey. Stratigraphical and Structural Geology. Second Edition. Revised and largely re-written by R. D. Oldham.[1] Calcutta, 1893 (received 1894). 8vo. Pp. xxiii. and 543, with folding map, 23 plates and 27 figures.

BLANFORD & MEDLICOTT'S "Manual of the Geology of India" has long been an ideal book of its kind. Detailed descriptions of the geology of extra-European countries have been issued in a separate form far too infrequently. The geologist, and especially the palæontologist, who has occasion to refer to some point in the stratigraphy of such a country has, as a rule, to search through numerous scattered memoirs for the simple fact he needs. Moreover, probably most geologists enjoy the occasional perusal of the description of some country if written as a connected story, and not in a series of scattered memoirs, dealing with local details. The publication, therefore, of a general account of the geology of India was an act for which all geologists must have felt grateful to the authorities of the Geological Survey of that country. The first two volumes, which contain the general stratigraphy, were issued in 1879, and have ever since served as indispens-

[1] We understand that a new title-page has been issued, owing to the omission of the names of the two original authors, Messrs. Blanford and Medlicott. For this, however, we understand Mr. Oldham was not responsible. He had inserted the names of the two original authors. He also enables one to see at a glance how much credit is due to Messrs. Blanford and Medlicott by printing, in the list of contents, the titles of all paragraphs taken from the former edition in different type to that used for those for which he has contributed.

able books of reference, and as models of what this sort of work should be. The fact that they have been out of print for some years showed how well they did what they were planned to do. Great progress has meanwhile been made in the geological survey of India, and a new edition, brought thoroughly up-to-date, was therefore much to be desired. The preparation of this was entrusted to Mr. R. D. Oldham; his "Bibliography of Indian Geology" (1888) suggested him as the most suitable man for the task, since a thorough acquaintance with the literature of the subject was one of the most essential qualifications. Mr. Oldham, moreover, has shown in numerous contributions to the *Records* and *Memoirs* of the Indian Survey, and in his recent sketch of "The Evolution of Indian Geography" in the *Geographical Journal* (March, 1894), that he is thoroughly in sympathy with the enquiries regarding the past physical geography of the country. And consideration of these is probably the only method of compiling a work that shall be interesting to read as well as valuable for reference.

In comparison with the former edition, the present has many advantages; it is issued as one volume instead of two; the scale of the map is reduced by about a third, and is, therefore, handier of reference, though there is more information in it; the long glossary is omitted; a most useful novelty is introduced in the index of localities, which occupies 32 pages; the latitude and longitude of each place is given, so that those unacquainted with the details of Indian topography can follow the descriptions with ease. The greatest improvement, however, is the abandonment of the system of describing the country in provinces; this method of treatment was no doubt inevitable in 1879, when the correlation of the rocks in the different regions was much less advanced than at present. The fact that description by formations instead of by provinces is now possible is one of the principal measures of the progress effected, which this volume attests. It is owing to this change that the work has been rendered more compact, for there is now one story instead of many.

In the list of contents Mr. Oldham has adopted the useful plan of printing in heavy type the titles of all those paragraphs which are either quite new or have been almost entirely rewritten. This, besides showing how much is to be credited to the original authors, directs attention to the subjects on which most work has been done in the interval between the two editions. Thus we see at once that little has been added to our knowledge of the pre-Palæozoic, the Palæozoic, the marine Jurassic, and the Cretaceous rocks of the Peninsula, or of the Deccan Traps. We note the absence of fresh information on the last of these with regret, for though Dr. Blanford's sketch of the subject (chap. xi.) is an admirable one, there are many points in the detailed stratigraphy, and especially in the petrography, on which further information is most desirable. In fact, in most of the Peninsular area, the facts remain much as they were, the principal additions being made to the knowledge of the correlation of the rocks with other areas in Asia, Africa, and Australia. Thus the description of the stratigraphy of the Gondwana system remains practically the same as before, though the chapter on the homotaxis of this formation is almost wholly new or rewritten. The fact that the great controversy as to the true correlation of this system is now at an end is noted with relief.

The principal changes in the volume are in chapters viii., xviii., and xix., which are entirely rewritten or new. The first is the one

already alluded to, dealing with the correlation of the Gondwana system. The other two summarise the evidence as to "The Age and Origin of the Himalayas," and give a sketch of the "Geological History of the Indian Peninsula." Attention is paid to the recent discussions upon the former, and the effort to prove that the Himalaya are of only very late Tertiary age, and that they are even now undergoing elevation. It is, however, maintained that the old view that the Himalaya are post-Eocene and pre-Pliocene is the correct one. The contentions of Middlemiss that the mountains were partially formed in pre-Tertiary times is also dismissed, but here the evidence is apparently less conclusive; the discussion of the subject leaves one with the impression that perhaps Mesozoic movements in the area were more important than has been believed. In discussing the formation of the Himalaya, attention is called to the Rev. Osmond Fisher's theory of mountain structure; a modification in this is proposed, and with this alteration the theory is said to find much support and illustration in the facts of Himalayan geology.

One of the most interesting points in which this edition differs from its predecessor is the different conclusion arrived at in respect to the formation of the Western Ghauts or Sahyadri Mountains. These are unlike the hills on the eastern side of the peninsula, which were grouped together in old geography books under the name of the Eastern Ghauts. For the Sahyadri Mountains have long been known to have a unity of structure, and to be of comparatively recent age. The hills on the Madras side of the peninsula, on the other hand, are not a single range, and were, with the Aravalli Mountains in Rajputana, among the oldest geological features in the country. In the 1879 edition the conclusion was maintained that the western scarps of the Sahyadri Mountains were formed by marine denudation, and were carried back some distance into the Tertiary rocks. Mr. Oldham, however, maintains that these mountains are the newest feature in Indian geography, and that their abrupt western scarps are the direct result of a dislocation. His arguments, moreover, seem absolutely conclusive as to the truth of this interesting and important view.

A charming photo-gravure of the eastern spur of the famous peak of Kanchanjanga (or Kichinjunga as it is probably better known in England) forms a frontispiece to the volume. This is so good that one wishes some more photographs of Indian scenery had been given in the book. We should have been glad to have seen some of the plates of fossils replaced by some of the photographs which have adorned recent volumes of the Indian Survey *Memoirs*. In a general book of this character, these would probably have been found of more general use and interest than figures of fossils.

Mr. Oldham is to be congratulated on the completion of his laborious task of revision, and we hope when a third edition is required the task will be placed in the hands of one who will do it with as much knowledge and originality.

J. W. G.

AMPHIOXUS.

AMPHIOXUS AND THE ANCESTRY OF THE VERTEBRATES. By A. Willey, B.Sc. Pp. xiv. and 316, with one plate and 135 illustrations, being Vol. II. of the Columbia University Biological Series. New York and London: Macmillan and Co. Price 10s. 6d. nett.

THE literature of *Amphioxus* is not so large as that of many other groups; but it is nevertheless of a decidedly respectable bulk, particularly if various papers which bear indirectly upon the problems

which the structure of this animal gives rise to are included. Mr. Willey quotes no less than 133 papers which have been consulted by him in the course of preparing the volume now before us, and of these at least 100 may be fairly put down to *Amphioxus* itself. It is therefore a saving of much time to the hard-worked zoologist of the present day to have all this in a nut-shell—a cocoa-nut-shell possibly,—but still a work of moderate dimensions. Apart from this merit, the practice of writing a book about a definite animal of interest seems to us to be an excellent plan. It is a pity that not more is done along this line, which was really initiated by Mr. Huxley in his "Crayfish." The anatomy of birds, for example, might be woven into a volume upon such a type as *Apteryx*, one upon the Amphibia upon a plain account of the Axolotl, and so forth.

The present work contains everything that should be known about *Amphioxus*, besides a great deal that it is advantageous to know about the Tunicata, *Balanoglossus*, and some other types which come into structural relations with *Amphioxus*. *Amphioxus* was originally put down as a Mollusc, a fate which also overtook another equally remarkable and not less isolated type, *Peripatus*; the Mollusca in the early days of this century and the last days of the seventeenth appear to have been pretty well what the worms are now, a receptacle for anything unclaimed. The Russian naturalist Pallas, the discoverer of the "fish," was responsible for this placing of it; it was not until sixty years had elapsed that Costa redescribed *Amphioxus* and put it in its proper place, approximately at any rate. Nowadays there are really two parties which surge round *Amphioxus*; there are those who would look upon it as much degenerate and those who regard its simplicity as a mark of antiquity. There is no doubt much of both in the organisation of the animal. This "exquisite form," as Professor Osborn enthusiastically calls it in the preface which he contributes to Mr. Willey's book, is common in many parts of the world; it is, indeed, practically world-wide in range, or is represented by a considerable number of species which differ in but slight characters, the number of the myotomes being the chief means by which they are distinguished. Mr. Willey argues from its wide range and the trifling specific differences that the form is extremely ancient. We fully agree with Mr. Willey that the creature is in all probability an extremely archaic form, but his reasoning does not seem to us to prove the fact. In ancient forms it is surely more usual for the differences between species, or genera, as the case may be, to be great rather than small; intermediate forms have had time to die out, and long isolation has done its work. It is, rather, the newer groups that fade imperceptibly into each other. The very fact that there are so many diverse opinions about the classification of birds is indirect evidence of the comparative newness of the group; while the strikingly marked lines which divide from each other the several groups of reptiles are confirmatory of their age as shown by the rocks. The number of described species of *Amphioxus* is eight, but we believe that a ninth species was described at the recent meeting of the British Association at Oxford.

Amphioxus is, as will be inferred from what we have said concerning the bulk of the literature devoted to it, one of those animals which most naturalists have had dealings with. It is surprising, therefore, to learn how recently the excretory system has been discovered. For a long time the animal was believed to be without any specialised excretory organs of the vertebrate type—a piece of evidence which appeared to

indicate its degenerate character. But now, thanks to the investigations of several workers, we are in full possession of the details of the excretory system, which has been proved to be constituted on the plan of that of other Vertebrata, but, of course, with differences in details. The excretory tubes of the *Amphioxus* appear to represent the "head-kidney" or pronephros of other vertebrates. But they have no continuous longitudinal duct such as exists in all other types. The place of this is, however, physiologically taken by the atrial cavity, into which the series of tubes opens. It is now the general opinion that the longitudinal duct of the pronephros in the higher vertebrates is a formation from the epiblast—a groove to begin with, which is later converted into a tube; since the atrial cavity is also an epiblastic formation, no great gap is indicated by the absence of a special longitudinal duct.

It is, however, rather the comparisons with lower forms of life that are so interesting in *Amphioxus*. The connecting link between vetebrates and invertebrates has been long sought, and in spite of *Amphioxus*, Ascidian, *Balanoglossus*, or even the latest claimant, *Cephalodiscus*, it has not yet been found. For as all the animals which have been mentioned show the three essential Chordate characters, the dorsal nervous system, the notochord, and the gill-slits, we are really no further than we were before, and there is still hope for the Annelid to put in a claim which will be admitted. Mr. Willey gives, in his introductory remarks, an excellent *resumé* of the celebrated opinion of Geoffrey St. Hilaire upon the matter. The boldness—yet, so far as it went, logic—with which that great naturalist compared the exoskeleton of an insect with the ribs and vertebral column of a fish, have been equalled, if not surpassed—at any rate, so far as regards the boldness—by modern comparisons of Crustaceans and *Limulus* with vertebrates. Of the three essential chordate characters, the gill-slits would seem to be the hardest to account for; the existence of breathing-organs as mere processes of the skin seems so natural as it is very widely spread. There has, however, been a suggestion made, which is duly quoted by Mr. Willey, and which seems to shed at least a pin-point of light upon the matter. Mr. Harmer and Mr. Brooks have thrown out the hint that the function of the gill-slits was originally that of getting rid of the superfluous water; a form like *Cephalodiscus*, with copious arborescent gills, would appear to have no need of aërating the blood in the other, so to speak, far-fetched manner. And yet *Cephalodiscus* has gill-slits. The only invertebrates not within the charmed circle of the forms already referred to, which are seriously considered by Mr. Willey, are the Echinoderma and the Nemertines. The former group come in, of course, through the likeness of their larva to the *Tornaria* larva of *Balanoglossus*, as was fully explained in our October number, on p. 245 of this volume. The Nemertines have a proboscis sheath, which Professor Hubrecht compared to the notochord, and a pair of nerves which tend to become approximated dorsally, and therefore to occupy the position of the central nervous system of the vertebrate. Mr. Willey, however, at the conclusion of his book, feels unable to do more for the vertebrate pedigree than to suggest "that the proximate ancestor of the vertebrates was a free swimming animal, intermediate in organisation between an Ascidian tadpole and *Amphioxus*. . . . The ultimate or primordial ancestor of the Vertebrates would, on the contrary, be a worm-like animal, whose organisation was approximately on a level with that of the bilateral ancestors of the Echinoderms."

Index Kewensis.

Towards the end of October the third part of Jackson and Hooker's "Index Kewensis" appeared. This part ranges from *Kiblikia* to *Psidium*, and includes the large and difficult genera of grasses, *Paspalum*, *Panicum*, and *Poa*, the synonymy of which is most complicated, and for which there has been no guide beyond the unsatisfactory monograph of Steudel. The volume bears the signature "Vol. II.," and is paged 1–640. We do not see any advantage in this new pagination in the middle of a reference book, as it only causes trouble in quotation. The work has already been referred to at length in this Journal (Natural Science, iii., p. 379), and all we need do now is to express our admiration at the rapidity with which the Oxford University Press and Messrs. Jackson and Hooker are passing their proofs through the press.

The "Orient Guide."

This new edition of the "Orient Guide" is the fifth, the first having been issued in 1884. As each edition has consisted of from 10,000 to 15,000 copies, there must be a good many "Orient Guides" scattered about in the world. In the present issue a considerable number of alterations are to be found, some, but not many of them, being improvements. The opening chapter is no longer an account of the principal ships, but an essay by the newly-knighted Sir Wemyss Reid on ocean travelling in general. The medical chapter is omitted, as are Mrs. Henry Fawcett's delightful notes on Italy and Germany. Nearly as interesting was Mr. W. B. Richmond's account of Greece, of which only the Athens fragment remains. An account of the Australian colonies by Mr. Matthew Macfie has been added, and forms an attractive feature. Mr. Middleton Wake's exhaustive list of birds likely to be seen at sea is supplemented by a short description of Australian zoology. There seems to be some confusion on p. 257 between Bauer's parrakeet and Barnard's, whose name is spelt wrongly as Bernardius. There is quite a little treatise on the mariner's compass, a difficult subject, but clearly and simply explained by Commander Hull, R.N. The Rev. W. J. Loftie, the editor of the volume, must have had a laborious task in cutting and fitting together the varied mass of information of which we have given a brief epitome. He is to be congratulated upon the compact result.

Mr. J. W. Taylor is publishing a Monograph of the Land and Fresh Water Mollusca of the British Isles, of which the first part has just been received by us. It contains abundant illustration, and each part will be sold at the moderate cost of five shillings. The leading malacologists have been freely consulted in preparing the volume, which will doubtless be found of great use. In the circular which we have before us, some MS. opinions of eminent naturalists, such as the Rev. Canon Norman and the Rev. A. H. Cooke, are quoted. We hope to review the first volume when completed.

The first volume of the Cambridge Natural History is to appear, we are informed, either before or just about Christmas. This is to be upon the Mollusca, and is written by one of our foremost authorities on the group, the Rev. A. H. Cooke. The recent Brachiopoda are to be treated of by Mr. Shipley, one of the editors of the work; the fossil Brachiopoda by Mr. F. Cowper-Reed; the Polychaet worms by Dr. Benham; the Oligochaetes by Mr. Beddard, and other groups by other specialists.

Messrs. Swan Sonnenschein & Co. advertise a second edition of Mr. Beddard's "Animal Coloration," which appears to be a simple reprint without any further additions.

Some Recent Journals and Pamphlets.

On November 1 our contemporary *Nature* attained its twenty-fifth birthday, and started its fifty-first volume with a leader by Professor Huxley, in which he points out the remarkable effect of Darwinism on Natural Science. Professor Huxley wrote the first article in *Nature* when it first appeared as a weekly newspaper in 1870, and the present editor has just cause to congratulate himself on the fact that so distinguished a send-off has again been accorded to him.

Another contemporary, the *Feuille des Jeunes Naturalistes*, also proudly announces that it starts its twenty-fifth year of publication with the November 1 number. We congratulate Mr. Adrien Dollfus on the success which has crowned his energy and enthusiasm. Besides a paper by the editor on the Idoteidæ (Isopod crustacea) of the French coasts, there are others on the noxious insects of the Pines in Champagne by R. Hickel, and a long account, by H. Hua, of the recent Congress of the Botanical Society of France.

The Geological Society has just issued the fourth part of the fiftieth volume of its *Quarterly Journal*. It is a number of which any society might be proud, and forms a fitting conclusion to the fifty volumes that are to be indexed by the Society. The part consists of eleven papers, illustrated by sixteen plates, of which no less than six are exclusively palæontological, and all this quite apart from the "Additions to the Library," which forms the subject of a special paragraph in our "Notes and Comments." Of the papers themselves the most interesting, perhaps, are Dr. Gregory's observations on the Glacial Geology of Mt. Kenya, in which he shows that the glaciation has had a much greater extent formerly than now, and has in no inconsiderable manner determined the geographical distribution of animal and vegetable life. African geology is much to the fore, for there is a long paper by Captain Lyons on the Libyan desert, and two other papers by Mr. Draper on the Geology of South-Eastern Africa. Petrology is represented by papers on the older fragmental rocks of North-West Carnarvonshire, by Professor Bonney and Miss Raisin; on the structures of the Carboniferous Dolerites and Tuffs of Derbyshire, by H. H. Arnold-Bemrose; and on the banded structure of Tertiary Gabbros in Skye, by Sir A. Geikie and Mr. Teall. Some interesting plates accompany the two last-mentioned papers.

In Palæontology, Mr. Peach writes on additions to the fauna of the *Olenellus*-zone of the N.W. Highlands. This is a paper of the greatest interest and value. After a few words as to the locality, Mr. Peach describes the fossils, of which the following are new: *Olenellus reticulatus*, *O. gigas*, *O. intermedius*, and *Olenelloides*, a new genus with species *armatus*. Four beautiful plates by F. H. Michael illustrate the paper. The second palæontological contribution is that of Frederick Chapman on the Microzoa of the Bargate Stone, a series he has worked out, described with considerable care, and illustrated with two plates. Seven of the Ostracoda and eleven of the Foraminifera are described as new. Mr. Chapman notes that this comparatively large number of new forms is due to the fact that the Bargate series belong almost exclusively to the Laminarian and

Coralline zones of the old Neocomian Seas. The part concludes with a paper by Mr. Gresley on Cone-in-Cone structure, as it occurs in the Devonian series of Pennsylvania.

THE September number of *The Journal of Marine Zoology and Microscopy* is in our hands, and appears to be fully up to the standard of other numbers of this useful periodical for the sea-side naturalist. There are four plates dealing respectively with the development of the *Balanus*, *Alcyonium*, and larval Ascidians, the anatomy of *Polynoe propinqua*, and the life-history of *Obelia geniculata*.

DR. PACKARD sends us a paper, anatomical and systematic, upon the Siphonaptera, more generally known as Fleas (*Proc. Boston Soc. Nat. Hist.*, vol. xxvi.). The paper is largely a *resumé* of the work of others, but it contains some original matter. The fleas, like most other animals, have been tossed about in the system. The general opinion about them, however, at the present day is that they are closely allied to, if not forming one order with, the Diptera. Professor Packard puts them a little further away from the two-winged flies than many do. But he decidedly thinks that "they stand nearer to the Diptera than to any other order, and that they must have diverged from the ancestral dipterous stem before the existing forms of Diptera had become so extremely specialised as we now find them to be." The paper, which occupies 43 pages, is copiously illustrated by cuts.

DR. A. G. BUTLER records, in the *Annals and Magazine of Natural History* for November, an amusing case of Walkerism—if we may so call it. It is the case of an unfortunate moth from Venezuela, which was described by Walker in vol. xv. of the British Museum Catalogue of Lepidoptera Heterocera under the name of *Celæna diffundens*. This same insect was described by the same author no less than six different times under six different specific names, and actually described under four of these names in one volume of the Catalogue (xxxiii.). As Dr. Butler truly says, "This kind of work needs no comment." But it is not an isolated instance.

IN the *Geological Magazine* for November Dr. Henry Woodward describes an interesting series of Carboniferous Trilobites from the upper Limestones, Bank of the Hodder, near Stonyhurst, Lancashire. These include two new species of *Phillipsia* (*P. vandergrachti* and *P. polleni*) and the types have, by the generosity of the College authorities and the discoverers, been placed in the British Museum. The same number of the *Geological Magazine* contains a photograph of an erect tree, *Sigillaria*, in the Coal-measures of the valley of the Roach, Rochdale, contributed by Mr. J. C. Brierley and Sir Henry Howorth.

DR. L. RHUMBLER has a note in the *Zoologischer Anzeiger* (September) in which he shows that *Peneroplis* belongs to the perforate rather than to the imperforate Foraminifera.

DR. JENTINK has published a catalogue of the Crommelin collection of Dutch Birds, now in the Leyden Museum. Full records as to age, sex, and locality are given, and the book should therefore be of considerable value.

OBITUARY.

NATHANAEL PRINGSHEIM.

BORN 1823. DIED 1894.

THIS famous German botanist, whose death was recorded in our last number, was born in 1823 at Wziesko, in Upper Silesia. Starting as a medical student in the University of his native province, he afterwards went to Leipzig, and later to Berlin. In the meantime, however, he decided to devote himself to botany, and it was a botanical thesis in which his Ph.D. was gained at Berlin. After taking his degree he visited Paris, and on his return commenced his career, in 1851, at Berlin University. Five years after, at the age of 33, he was elected Member of the Berlin Academy of Sciences. In 1864 he was called to Jena to replace Schleiden as ordinary Professor, an appointment he held for four years. During his stay in Jena he founded an Institute of Plant-Physiology, the first of its kind. The idea took, similar laboratories were established at other Universities and agricultural high-schools, and a tremendous impetus was given to the study of plant-physiology. In 1868 Pringsheim returned to Berlin, but did not join the teaching staff of the University. From his own resources he founded a botanical laboratory, and soon established a school, to which belonged Strasburger, Tschirch, Vöchting, and other well-known botanists.

Pringsheim's great work was on the sexual relations in Cryptogams, especially Algæ, following up the results of Thuret and others. In a series of researches on the history of growth in individual families, he showed the existence in the Algæ of widely different forms of sexuality and development. Another subject attacked by Pringsheim was Chlorophyll. He was the first to demonstrate the true relation between the green colouring matter and the protoplasmic corpuscle; as to the function of chlorophyll, he held a view the reverse of that generally accepted, namely, that the colour acts as a shade, absorbing those rays of light which promote destructive metabolism, and thus making possible the constructive process.

A biographical notice would be incomplete without mention of the familiar "Jahrbücher für wissenschaftliche Botanik," started in 1857 and continued up to the present. Pringsheim was one of the oldest foreign members of the Linnean Society, having been elected on May 3, 1866.

PIERRE DUCHARTRE.

BORN 1811. DIED 1894.

THE eminent French botanist who has just died was Professor of Botany in the Faculty of Sciences at Paris, a member of the Institut, and also a foreign member of the Linnean Society of London, having been elected on May 3, 1877. He was the author of numerous papers on plant-biology, including observations on the flower and its organogeny, germination, and the young seedling, among which we may recall some valuable contributions on polycotyledonous embryos, respiration, transpiration, the question of the absorption of water by leaves, the vegetation of epiphytic plants and teratological notes. In 1867 he published a large and valuable text-book, entitled "Elemens de Botanique," of which a second edition appeared in 1877, and a third in 1885. In 1868 he produced an exhaustive report on the progress of plant-physiology. He was also responsible for the fourth volume of Jacques and Heurincq's "Manuel des Plantes," a systematically arranged encyclopædia of plants in cultivation, or of interest from an economic point of view. In his younger days he edited for two years (1845–1847) a monthly periodical, the *Revue Botanique*, a series of abstracts of the chief botanical works published at home and abroad. He was led to undertake this onerous piece of work by Benjamin Delessert, but, as many another young and enthusiastic scientific worker has found, it meant giving up a very large proportion of time, and stopped scientific research. The death of Delessert in 1847 was therefore made the pretext for the cessation of the *Revue*.

GUILLAUME LOUIS FIGUIER.

BORN FEBRUARY 15, 1819. DIED NOVEMBER 9, 1894.

THE death of this eminent French chemist and naturalist is announced. Figuier was born at Montpellier, at which town his father Jean was a chemist. He received his doctor's degree from the Faculty of Medicine of Montpellier, and left for Paris, where he worked in the laboratory of the Sorbonne, under Balard. In 1846 he became professor to the School of Medicine at Montpellier, where he remained five years, retiring to Paris in 1852. Since 1855 he had charge of the scientific columns of *La Presse*, and subsequently of *La France*. In 1856–57 he attacked the views of Claude Bernard regarding the secretion of sugar by the liver, and this at once brought him into prominence. About this time, too, Figuier resigned his appointments, renounced scientific research, and devoted himself entirely to the popularisation of science. In this he was conspicuously successful, and his popular books enjoyed a large and well-deserved circulation. His best-known works are "La Terre avant le Déluge," 1863; "La Terre et les Mers," 1864; "Histoire des Plantes," 1865; "Les Zoophytes et les Mollusques," 1866; "Les Poissons,

les Reptiles, et les Oiseaux," 1867; and a long series of similar publications. In one of his later works he was concerned to uphold the immortality of the soul. Most of his books have been translated into the chief European languages. When, however, he attempted to teach science from the stage of the Cluny Theatre by means of a play, entitled "Les Six Parties du Monde," he met with a less hearty welcome. Figuier died at Paris, aged 75 years.

The distinguished oculist and microscopist, Ludwig Mauthner, died at Vienna on October 20, at the comparatively early age of fifty-five. After studying at Vienna, he went to Berlin and studied under Albrecht von Gräfe. His famous work, "Lehrbuch der Ophthalmoscopie," appeared in 1868. He also published "Die Bestimmung der Refractionsanomalien mit Hilfe des Augenspiegels" in 1867, and "Vorlesen über die optischen Fehler des Auges" in 1876. He was part editor of the *Archiv für Augen- und Ohrenheit Kunde* for some years.

The well-known collector of Fossil Fishes, the Rev. Hugh Mitchell, LL.D., died at Aberdeen, about the middle of the month. His chief collections were made from the Old Red Sandstone of Forfarshire, and included the forms known as *Acanthodes mitchelli*, *Ischnacanthus gracilis*, and *Climatius scutiger*. Sir Philip Egerton, Mr. Powrie, and Professor Ray Lankester largely drew upon his collections when writing their important monographs. His collections are now in the British Museum of Natural History, having been purchased last year.

The death of Mr. Oscar Koechlin, of Alsace, which took place in September, robs that district of an enthusiastic entomologist, and the Mulhouse Society of one of its original and most energetic members. Dr. B. N. Rake, who was elected President of the Trinidad Field Naturalists' Club on August 3, in succession to Mr. H. Caracciolo, died on August 24 at Port-of-Spain. Mr. Henry Tate, the acting Government analyst of the same island, also fell a victim to the prevailing malignant fever on August 18.

The news from Africa during the past month is very serious. Telegrams have appeared which record the deaths of the German botanist Dr. Lent, and the zoologist Kretzschmar, in the Kilimanjaro district, killed by natives, and of Mr. G. L. E. Andreæ, a member of a Belgian trading company, at Mukikamu, near Stanley Pool. Mr. Andreæ was an entomologist, and several parcels of insects have been sent by him from Africa.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

DR. WILLIAM PETERSON has been appointed Principal of the McGill University, in the room of Sir William Dawson. Principal Dawson resigned about a year ago. Dr. Peterson has been for twelve years head of University College, Dundee.

DR. P. L. SCLATER has been elected an Honorary Fellow of Corpus Christi College, Oxford. He was formerly a Fellow of this, his old college.

SIR THOMAS TEMPLE, of Ballintemple, Wicklow, succeeds to the Vice-Presidency of the Royal Dublin Society. The office was rendered vacant by the death of the Right Hon. William Cogan.

DR. CARL V. HEIDER, formerly an assistant in the Zoological Institute of Berlin University, has been appointed Professor of Zoology at the University of Innsbruck, and Dr. Michael von Lenhossek, Professor of Anatomy. Dr. von Lenhossek comes from Würzburg. Dr. R. von Lendenfeld has become Professor of Zoology in the University of Czernowitz; and Dr. Schewiakov leaves Heidelberg and goes to the Imperial Academy of St. Petersburg as assistant in the Zoological Institute.

MR. AND MRS. THEODORE BENT have started on another expedition to Southern Arabia, where they hope to continue the archæological and botanical work commenced last year in the Hadramaut Valley. They expect to return to England next April.

MR. A. TREVOR-BATTYE, about whose safety so unnecessary a fuss was made in October last, has returned from Kolguev Island, where he went about the end of June to study and collect the birds of the island.

WE regret to learn that M. de Brazza, the well-known explorer, suffered shipwreck near Tchumber, on his way back from Kounde to Brazzaville on September 30. M. de Brazza was no worse for his experience, but all his papers were unfortunately lost.

A LIFE of Joseph Wolfe, the famous painter of animals, has just been completed by Mr. A. H. Palmer. Many examples of his work are in the possession of the Zoological Society, some of the most striking of which are those of the larger apes, and hang on the walls of the meeting room.

MR. SPENCER MOORE, who, under somewhat unfavourable conditions, recently made a successful botanical exploration of a portion of the Matto Grosso district in Brazil, has gone to seek a better return for his labour in the gold-fields of West Australia. We trust he will make money enough for him to devote the rest of his life to botanical work.

SCIENCE at Oxford University will lose a friend in Professor Sir Henry W. Acland, who has stated that he will resign the Regius Professorship of Medicine at the close of the year. He has fought the cause of Science for many years at Oxford in the teeth of much opposition, and has carefully promoted and watched over the development of museums and educational matters in the University. But no doubt his withdrawal from the cares of office will not altogether deprive his colleagues of his valuable aid.

CAMBRIDGE University benefits under the will of the late Mr. Samuel Sandars to the extent of £2,000. The money is left for the endowment of a reader in Bibliography.

IN our third volume (p. 451) we published an account of the proposed Sedgwick Memorial Museum at Cambridge, by Mr. H. Woods. We regret to learn from the *Cambridge University Reporter* that "even on the lowest tender, the cost of the building would exceed by £4,500 the estimate which was before the Senate when the Grace approving the plan was passed." Since it is considered that the University cannot afford so large a sum, the Syndicate appointed to report on the matter have recommended that the plan be reconsidered, and if necessary a new one substituted for it.

AMONG the examiners for the Natural Science Tripos at Cambridge will be found Professor Lewis and H. A. Miers for Mineralogy; Philip Lake and Professor Cole for Geology; W. Bateson and Professor Hickson for Zoology; Professor Oliver and W. Gardiner for Botany, while for Human Anatomy Professor Macalister and Dr. Rolleston, and for Physiology W. B. Hardy and Professor Schäfer have been appointed.

FROM the Report of the Director of the Science and Art Museum, Dublin, we learn that the electric light will be extended to the Natural History Museum and various other buildings during the current year. We wish the same could be said of our London institution. One of the most valuable additions made to the collections last year was the collection of British Marine and Land Mollusca formed by Edward Waller. This collection contains a large number of the rarities belonging to Gwyn Jeffreys, whose own collection was unfortunately allowed to go to America. A small timber museum is being prepared, and arrangements are being made for the exhibition of illustrative sets of Irish plants from various localities. The Botanical laboratory is now in good going order, and original investigations on Irish Marine Algæ are in course of progress by Miss Hensman. The attendance of the public at the Museum is in excess of previous years, and the daily average was 1,000. With regard to Sunday attendance the average for the Museum was 979, and that for the Botanic Gardens 5,386. This is most encouraging. The Director insists on the importance of keeping up the library, the grants being insufficient for the purpose of the purchase of books necessary for the determination of the specimens. This is a want also keenly felt in our London institutions.

WE regret that the Raffles Museum at Singapore is still without a curator. It will be remembered that about a year ago, Dr. Haviland, who had come from the Sarawak Museum in Borneo, and who had done much to improve the condition of the Raffles Museum and to save money to its Committee, was compelled to resign on account of the insufficiency of his salary, only 200 dollars (£20 16s.) a month, which the Government refused to increase. This valuable museum has since then been in the charge of a clerk, at 150 dollars a month, nor is there any immediate prospect of a more competent individual being appointed. We regret the financial position of the Colony, but hope that it will not be thought necessary to extend this penny-wise policy to other of its scientific appointments.

On the completion of the decorations and renovation to which we have already referred, the Bristol Museum was re-opened by the municipal authorities on November 12. The hours are now extended from 10 a.m. to 9 p.m. daily. A Reference Library has been added. An extension of the Oldham Public Library and Museum has also been opened this month, in the form of a lecture-hall capable of seating 900 people. Free lectures are given each Saturday evening during the winter. The Art Gallery at Oldham is already famous among Lancashire institutions, and it is now proposed to devote attention to providing an adequate Natural History Collection.

The Royal Society Medals have been awarded as follows:—Professor Cleve, of Upsala, receives the Davy medal for his researches on the rare earths; Professor Huxley, the Darwin Medal; Professor Victor Horsley, one of the Royal Medals. No more appropriate award could be given to Professor Huxley, and we are glad to hear that Professor Horsley is to have a Royal Medal for researches so beneficial to mankind, though mankind is already sufficiently grateful to so distinguished a benefactor.

We learn from the *Birmingham Post* that the card catalogue in the library of the Royal College of Surgeons will be utilised, when finished, as copy for a new catalogue of the library. A printed catalogue of this rich medical library would be of great service, as many books dealing with general natural science are contained therein. The College has also acquired a ground plan of John Hunter's house in Leicester Square, drawn by William Clift, Hunter's pupil, which shows the arrangement, not only of his rooms, but much of their contents.

At the opening meeting of the Zoological Society, held on November 6, Professor Sir William Flower exhibited and read extracts from a letter and diaries from Emin Pasha. The letter recorded the dispatch of specimens for the British Museum, but was undated. The diary commenced on May 29, 1892, and was continued till October 12, 1892, when Emin was close to Stanley Falls. It consisted chiefly of notes on the birds he had collected on this his last journey. The collections have never been received in this country, and it is probable they were destroyed by the band that murdered the traveller. The manuscripts had been received by Sir William Flower from the agent of the Congo State at Brussels.

The Geologists' Association inaugurated their session on November 2 by an exhibition of lantern slides of photographs of places of geological interest, instead of holding the usual conversazione. The exhibition, which seemed to be of much interest to the members, was chiefly remarkable for some views taken in deep mines in Cornwall, by Mr. W. Thomas, and others of the Festiniog district, taken by Mr. J. C. Burrows. The views were confined to British subjects, and Messrs. Preston, Lardeur, and Hiddon showed excellent results taken during the various excursions of the Association.

At the next meeting of the Association, on December 7, Mr. A. Smith Woodward promises a note on the Megalosaurian teeth that have been discovered by Mr. J. Alstone in the Portlandian of Aylesbury, after which (the Honorary Secretary asks us to state) Professor McKenny Hughes, Mr. H. W. Monckton, and Dr. Frazer Hume will describe the geology of their recent tour in Switzerland.

From the *Journal* of the Oxford University Scientific Club, of which no. 18 has just reached us, we learn that Mr. M. D. Hill has been granted the use of the British Association Table at Naples, where he intends to continue his researches on artificial fertilisation. At a meeting of the Club on October 26, Mr. Garstang read a paper "On some modifications of the Tunicate pharynx induced by the violent ejection of water."

Despite the exertions of this energetic Club, we regret to observe that the study of geology still thrives so ill in this same University of Oxford. The examiners for

the Burdett-Coutts' Scholarship have once more had to report that no candidate of sufficient merit has presented himself.

THE *Journal of the Marine Biological Association* (vol. iii., no. 3) contains the statements of receipts and expenditure for the year ending May 31, 1894, as well as the directors' report. The income for the past year was £2,172, including the annual grants of £1,000 from H.M. Treasury and £400 from the Fishmongers' Company. In connection with our Editorial note in the last number, it is interesting to read that the rent of tables produced only £36 16s. 3d. We are glad to hear that the trouble about bad sea-water supply has been detected and satisfactorily settled.

THE *American Naturalist* states that the American Association for the Advancement of Science has again subscribed $100 for a table at the Marine Biological Laboratory at Woods Holl. Any member or applicant for membership of the Association may apply for the table through the permanent secretary, Professor F. W. Putnam, Cambridge, Mass. The award is made by a committee of five.

A "BOTANICAL Society of America" has been founded in Brooklyn, N.Y., and Professor W. Trelease, of St. Louis, has been chosen first president.

THE A.W.P.L. and P.N.E.U. bid fair to become as firmly established among us as the Y.M.C.A. and the M.A.B.Y.S. The A.W.P.L. however has, in consequence possibly of the rivalry of the Pioneer Club, changed its name to the "Women Lecturers' Association," and will no doubt be symbolised by W.L.A. Under the auspices of the W.L.A. a course of six lectures for children on "Our Animal Friends" is being delivered by Mrs. Rose (Certified Student, Royal College of Science, London). The time is 3 p.m. on Wednesdays, and the place is, as heretofore, one of the geological galleries in the Natural History Museum. Tables of strata and cases of rocks and fossil shells are so very appropriate to talks on the Cat, Dog, Horse, Rabbit, and Mouse. The kindness of the trustees also permits the Museum specimens to be drawn upon for the illustration of the lectures. Tickets for the course are 10s. Our representative informed us that the first two lectures were poorly attended. We hope the rest will be more successful.

Meanwhile the Belgravia branch of the P.N.E.U. announces certain free lectures, one of which, on December 4, should at least attract those interested in natural science. It is "On the study of Natural History as a means to Moral and Mental Development," and will be delivered by Miss Shackleton, at 35 Cranley Gardens, S.W., at 4.30 p.m. The Secretary of the P.N.E.U. is Miss Jennie Paterson, 28 Victoria Street, S.W. The Secretary of the W.L.A. is Miss Edith Bradley, 4 Caroline Place, W.C.

A COURSE of twenty-five lectures on the History of Geography and of Geographical Discovery, by Mr. H. J. Mackinder, is now in progress at Gresham College on Mondays at 6 p.m. These lectures are under the auspices of the Royal Geographical Society and the London University Extension.

THE interesting series of Cantor Lectures, which were delivered by Mr. Hugh Stanners on "Artificial Foliage in Architecture," are now being printed in the *Journal of the Society of Arts*. The stem and the flower are dealt with in the *Journal* for November 2. They are accompanied by some good illustrations.

WE learn from the *Pall Mall Gazette* that Mr. Hovey, in the course of a paper read before the American Forestry Society, stated that the remains of silicified trees in Arizona, known as the "Arizona fossil forest," are being ground into powder by a commercial company as a substitute for emery.

CORRESPONDENCE.

"ANLAGEN."

THE word "blast," proposed (NATURAL SCIENCE, v., p. 368) as an equivalent for the German "Anlage," has the certainly great merit of shortness, but has it not likewise some shocking demerits in the way of earlier associations? We are accustomed indeed to the blast of a bugle or a trumpet; but what is to be thought about "the blast of an organ"? I venture to suggest "primule" as an irreproachable term.

Salt Hill, Galway. W. E. HART.

THE MUSEUMS' ASSOCIATION.

As a member of the Museums' Association I read the note entitled "Science at a Picnic," in your August number, with much interest, not unmixed with astonishment at your boldness. Now that I have returned from a distant land where even NATURAL SCIENCE did not penetrate, I have read the replies from the Secretaries of the Association, and from a member of the Dublin Local Committee, to which your strictures not unnaturally gave rise. Now it would be affectation on my part to overlook the fact that I am the member of the Association alluded to by Messrs. Platnauer, Howarth, and Johnson, who appeared at the last moment with "some notes," "of an extremely discursive nature," "which he wished to descant upon," etc., etc. If this paper of mine—which certainly was discursive in that it dealt with fifteen museums in different parts of the world—is one of those to which you alluded as having been burked, I beg leave to state that you are in error. I had absolutely no *locus standi*. Not only did I send no intimation of my paper till after the programme was printed, but though I had received most courteous permission by telegraph to read the paper on the first day, I did not appear with it till the last day; and in spite of this I met with the utmost consideration at the hands of the officers of the Association, of various members of the local committee, and especially of Professor Johnson himself. I may also add that, although my paper is one of great length, it has been most cordially accepted for publication in the Report of the Association.

After saying this, I do not think I can possibly be misunderstood if I venture to express my agreement with your main thesis. Without referring to what was privately said by other unofficial members of the Association, I need only state that I, at least, was one of those who "could ill spare their time," and to whom, consequently, "the insertion of a day's excursion between the two days of meeting proved vexatious." This, indeed, was the very reason why I was unable to intimate my intention of attending the meeting; and after all I only managed to be present by travelling two nights in succession. It is perfectly possible, as Professor Johnson says, that discussion "was invited once or twice, and was not forthcoming." People who have the most to say are not likely to speak when the allotted time has already been exceeded Silence is especially golden when the luncheon hour has struck and much business has yet to be got through. It would be invidious to mention special papers, and obviously one cannot give the names of members who undoubtedly would have spoken on them had time permitted. It is enough to contrast the statement of the Secretaries that "the volume of *Proceedings* of the Dublin Meeting will be the largest that has ever been published by the Association," with the other statement, also by the Secretaries, that only seven hours "were devoted to papers and discussions. F. A. BATHER.

[Our Comments on this subject were written with full knowledge of the circumstances attending Mr. Bather's oration. We did not count it among the papers of which the reading and discussion were unduly curtailed.—ED.]

I HOPE you will permit me a word in this discussion. As a member of the Association who was present throughout the Dublin meeting, although I personally feel extremely grateful to the able Secretaries and to the local committee, whose indefatigable exertions made the meeting the success it was, I must nevertheless express sympathy with the feeling which actuated your editorial note in NATURAL SCIENCE for August. The object of our Association is undoubtedly purely practical; that is, we meet mainly for the purpose of seeing and learning of new and efficient mechanical appliances and new methods, for the display and arrangement of the objects in Museums, considered entirely apart from the scientific or artistic details of the objects themselves. This we aim at accomplishing by the inspection of the Museums within reach of the place of meeting, by the reading of papers which describe the arrangements, actual or proposed, in these or other Museums, and by the discussion and explanation of the same. Now, on the last day of the meeting I did feel that the reading of papers and the discussion were considerably restricted owing to want of time; and one kept silence in regard to many points on which one would have liked more information, because this want was evident.

The excursion to Bray—which all those who joined in it described (so far as I heard) as charming (for I personally employed the day in re-visiting the Museums in Dublin, gathering details of the many excellent devices introduced there by Dr. Ball and his officers, some of which I am now trying to give effect to here)—was a part of the programme that some others besides myself felt might have been postponed, and the day given instead to the discussion of the many important appliances described, and other matters raised (but *scarcely* settled) in papers read before the Association. Lord Powerscourt's collection, though of the highest interest *per se*, did not pretend to any arrangement which could afford new hints to the *Museum Curator*. Such luxuries as this excursion and visits to other places of great interest in themselves, but only indirectly beneficial to us as *Curators*, might, on future occasions, I think, be advantageously deferred till the true work of the Association has been accomplished, when those who are able to remain would enjoy them with greater zest and a clear conscience.

The Museums, Liverpool. HENRY O. FORBES.

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