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NOTES AND COMMENTS.

Scientific Dinners.

NOW that the festive season is upon us, a few remarks on Scientific Dinners may not be out of place.

Most of the learned Societies meet once a year to celebrate their "anniversary," in a more or less convivial fashion. This they do, not in the style of the "good old days" of the "three-bottle" men—the process is in a measure reversed. Instead of a plain dinner and a large amount of wine, the custom is to have a very elaborate dinner and a moderate amount of wine. The cost may be much about the same.

In the meanwhile the composition of the societies has undergone considerable alteration. In old days, Science was the pursuit, or the pastime, for the most part of those well-to-do—of leading professional men, of clergy, and men of independent means. Nowadays Science owes its progress as much, perhaps more, to poor men than to the comparatively rich. Yet the annual dinners are practically restricted to the latter, on account of the guinea (or more) that is charged. This is not as it should be. To the younger workers, as a rule, the cost is prohibitory. These social gatherings should be representative: whereas, under present circumstances, it seems as if we had a Christmas dinner, and denied a place to the children and to our poor but hard-working relations. A change is needed, so that rich and poor, old and young, may meet, and equally find a welcome.

We read in the *Life of Edward Forbes* that during the meeting of the British Association held in Birmingham in 1839, "He and other young naturalists, disliking the irksomeness and expense of the ordinary, adjourned to a small tavern, adorned with the sign of the Red Lion. There they dined daily at small expense, on beef cooked

in various fashions, moistened with sundry potations of beer, and enlivened by joke and song—in contradistinction to the endless dishes and wines and formality of the 'big wigs.'"

"And in after years, when he had arrived at the zenith of his reputation, and British geology had conferred upon him the highest honours it had to bestow, his antipathy to men of buckram remained as strong as ever. He could see no reason why a President of the Geological Society should cease to be a 'Red Lion,' and so he chanted his songs and cracked his jokes as merrily as he had done in the little Birmingham tavern when he was only beginning to be known."

The Red Lion dinner, alas! is not what it used to be—with the formalities and expenses that seem to attend the introduction of "war-paint."

In making these remarks we have no desire to advocate a return to the customs of the past, nor to impair in any degree the true dignity of Science; but we fully believe that if the festive gatherings of our learned Societies were arranged on more economical lines, they would much more adequately fulfil the purpose for which they are instituted. In reference to the customs of the past, it will be of interest to append the following account of a Royal Society Club Dinner a century ago, taken from a translation of Faujas St. Fond's *Travels in England, Scotland, and the Hebrides*, 1799 (vol. i., pp. 48–51):—

"About forty members of the Royal Society have been, for more than twenty-five years, in the habit of dining annually in one of the taverns of London. Each member has the right of bringing to this club two visitors, whom he chooses, among foreigners or the friends of the Royal Society of his own acquaintance. The president may bring a greater number, and can select whoever he pleases for guests.

"We sat down to table about five o'clock. Sir Joseph Banks presided, and filled the place of honour. No napkins were laid before us; indeed there were none used; the dinner was quite in the English style.

"A member of the club, who is a clergyman (I believe it was the astronomer Maskelyne), made a short prayer, and blessed the company and the food. The dishes were of the solid kind, such as roast beef, boiled beef and mutton prepared in various manners, with abundance of potatoes and other vegetables, which each person seasoned as he pleased with the different sauces which were placed on the table.

"The beef-steaks and the roast beef were at first sufficiently drenched by large quantities of strong beer, called porter; it was drank out of cylindrical pewter pots, which are, by some, thought preferable to glasses, perhaps because they enable one to swallow a whole pint at a draught.

"This prelude being finished, the cloth was removed, and a handsome and well-polished table was covered, as if it were by magic, with a number of fine crystal decanters, filled with the best port, madeira and claret; this last is the wine of Bordeaux. Several glasses were distributed to each person, and the libations commenced on a grand scale, in the midst of different kinds of cheeses, which

rolling in mahogany cases from one end of the table to the other, provoked the thirst of the drinkers.

"To give more liveliness to the scene, the president announced the health of the prince of Wales; this was his birth-day. We then drank to the elector palatine, who was that day to be admitted a member of the Royal Society. The same compliment was next paid to us foreigners, of whom there were five present.

"The members of the club afterwards saluted each other, one by one, with a glass of wine. According to this custom, one must drink as many times as there are guests, for it would be thought a want of politeness in England to drink to the health of more persons than one at a time.

"A few bottles of champaign soon put all the company in good humour. The tea came next, with butter, marmalade, and all its usual accompaniments; coffee followed, humbly yielding precedence to the tea, though it be the better of the two. In France, we commonly drink only one cup of good coffee after dinner; in England they drink five or six times that quantity of the most detestable kind.

"Brandy, rum, and some other strong liquors closed this philosophic banquet, which terminated at half-past seven, as there was to be a meeting of the Royal Society at eight o'clock. Before we left the club-room, the names of all the guests were written on a large sheet of paper, and each of us paid seven livres four sols French money: this was not dear.

"I repaired to the Society along with Sir Joseph Banks,—Cavendish, Dr. Maskelyne,—Aubert, and Sir [Henry] Englefield; we were all pretty much enlivened, but our gaiety was decorous.

"Doubtless, I should not wish to partake of similar dinners if they were to be followed by settling the interests of a great nation, or discussing the best form of government; such a conduct would neither be wise nor prudent; but to meet, to celebrate the admission of an elector palatine, who has, besides, much merit, to a learned Society, is not a circumstance from which any inconvenience can result."

Circumstances, as we have said, have changed much since the time of St. Fond's travels, and we can only hope that the conservatism of the older learned Societies will ere long be compelled to yield to the spirit of the age.

National Museums.

An article of some importance "On our National Art Museums and Galleries" is contributed by Sir Charles Robinson to the *Nineteenth Century* for December, 1892 (p. 1025), and many of his remarks will apply equally well to the National Scientific Collections. While fully appreciating the zeal with which the officers in charge of the various museums perform their duties, and while duly acknowledging the gratifying progress that is being made, Sir Charles laments the want of a central controlling organisation to direct the whole and prevent unnecessary duplication of specimens:—"It is little to say that in our museum system everything is in a chaotic state, everything drifts fortuitously; there is no central overruling and directive power, no bond of union, and scarcely any intercommunion between one

establishment and another—briefly, no definitely established system for the general governance of these institutions; hence tacit rivalries, which sometimes develop into flagrant antagonisms. The bounds of jurisdiction or the several provinces of these institutions overlap in all directions, and their respective interests clash."

Everyone acquainted with the Natural History Departments of the British Museum and the allied public institutions of more recent foundation, will recognise how appropriately these remarks apply to them. The originally economic aims of Kew Gardens, for example, have expanded to such an extent that the Herbarium is in constant rivalry with that of the British Museum, separating collections which ought to be united, and duplicating collections in other instances. The Museum of Practical Geology, also, continually acquires fossils which are of little or no value for stratigraphical or economic purposes, and which, from their unique interest in an anatomical sense, ought to be placed with the purely scientific collections in Cromwell Road. When the Art Museums are re-organised, by all means let the Science Museums be similarly controlled, and the reformation will be not merely economical, but conducive to efficiency.

The Botanist in Winter.

Though so many of our native plants lie dormant in the winter, the time is not lost even for them. We perhaps go into the lanes or fields and see only withered stalks, or ripe fruit waiting to be picked or shaken off. We say to ourselves that the year is finished, and nothing more is to be done till life revives with the first warm days of spring. This idea, however, is quite mistaken, for the botanist who has the good fortune to be in the field in winter sees things that are quite unknown to the summer rambler. He soon learns that though life may be dormant, or even extinct, in the dead stalks, yet they are not therefore merely cumbering the ground and waiting for decay. In many of our native plants the adaptation of the means to the end does not cease with life, and the dying or dead stem is often modified to help to protect or disperse the seeds.

Winter botany has been little studied, but there is much to be learnt. For instance, the dead, unsightly umbellifers, so common in every hedge, are still playing their part. Instead of the green, flexible stem found in the summer, we see a stiff, elastic stalk, with thin whip-like tips, from which the fruit, separating from the axis, dangle by one corner. Anyone who has seen boys throw pellets of clay by means of a switch, and has afterwards forced his way through dead umbelliferous stems, like those of the Wild Chervil, will at once realise the great use of the elastic stalk, the whip-like tip, and the divided carpophore with its dangling fruit. Each passing animal and every breeze bends the stalk, which, springing back, tends to fling the fruit well beyond the limit of the ground exhausted by the

patent plant. Thus the elasticity of the dead stem may be as important to the species as any character in the living plant.

Many annual plants, however, have burrs, and in these species the dead stem is more flexible and tougher, so that it bends and rubs the fruit against the fur of an animal, or against our clothing. The different character of the dead stem will at once be recognised in the Wild Carrot, the ripe burr-like fruit of which curl inward, so that they cannot be shaken off, but come away a few at a time when the plant is stroked by anything rough.

This is merely one example of what can be observed in late autumn and winter; but anyone who has visited a beech or oak wood during a gale in the fruiting season, will understand why such fruit grow on the tips of long flexible branches, instead of on thick stems. The stinging blow that can be given by an acorn under these circumstances will soon convince the naturalist of the important part played by the flexible branch. Even the gale that tears off large arms may be of great use to the species, though ruinous to the individual tree.

A Naturalist in the West Indies.

Mr. W. R. Elliott, who has been collecting Cellular Cryptogams during the last year in St. Vincent, Anguilla, and Dominica for the West India (Natural History) Exploration Committee, has met with considerable success. In his last expedition, he has made a prolonged examination of the highest peaks of Dominica, the most densely wooded and primitive of all the Antilles, except, perhaps, Hayti, "the black republic," of which very little is known. He has obtained a very large series of Hepaticæ which Mr. Spruce has undertaken to work out, while he has added to the large number of Fungi already collected and partly described by Mr. George Massee in the *Journal of Botany*.

Among other things of which Mr. Elliott has been in quest is the petrel, the Diablotin, supposed to occur only in Trinidad. He has found the holes frequented by the birds, and at the time of his last letter was awaiting the possible re-appearance of a stray specimen or two, since the season (November) had arrived at which this might be expected. He visited the Carib reservation of Salybia on the windward coast of the island, but his account adds little of note to that given of the expiring remnant of this people by Mr. Ober in his interesting *Camps in the Caribbees*, except that the race of true Caribs is now in much the same case as the Diablotin. It would be of interest, and even a negative result would be of some value, if a local naturalist of St. Vincent or Grenada were to examine, at the proper season, some of the higher, less accessible, and seldom visited island peaks among the Grenadines whence reports have come at different times of the appearance of a bird resembling the Diablotin.

THE SQUARE-MOUTHED RHINOCEROS.

IT has been considered that the square-mouthed, or so-called White Rhinoceros (*R. simus*) is already extinct. Recently, however, Mr. F. C. Selous has written to our contemporary, the *Field*, to say that a few of these magnificent animals still survive in a remote corner of Mashonaland. One of these last survivors has already been killed, with the view that its remains should find their way to the British Museum. We trust, however, that the authorities in Mashonaland will take care that the others are not molested, and may be allowed a chance of propagating their species.

We may take this opportunity of mentioning that although most zoologists have recognised the extreme specialisation of the molar teeth of this species, as exemplified by their very tall crowns, and the flat plane of wear of their grinding-surfaces, yet it does not appear to have been noticed that they differ from those of all other living Rhinoceroses by the presence of a thick investing coat of cement. Not only does this cover the outer surface of the tooth, but it likewise fills up both the main and the posterior valleys. Indeed, the molars of this species bear almost the same relationship to those of the Sumatran Rhinoceros as is presented by the molars of the Horse to those of the Anchithere.

In the extinct Woolly Rhinoceros (*R. antiquitatis*) the cement is present to a less degree in the molar teeth, which are of the same general structure; while in the allied *R. platyrhinus*, of the Siwalik Hills of India, there was, probably, also a certain amount of this constituent. In both these extinct species the teeth do not, however, appear to attain the extreme specialisation of the square-mouthed Rhinoceros, and it is accordingly difficult to regard the latter as representing a genus apart from the one including all the other existing species, which we should otherwise have been disposed to do.

THE SCIENCE OF GROWTH.

READERS of a recent article in NATURAL SCIENCE on the Anatomy and Development of the Brachiopoda, who were appalled by certain strange and fearful words, such as "nepionic," "ephebolic," and "nostologic," will be interested to learn that those terms have just been elucidated and improved by S. S. Buckman and F. A. Bather in a paper entitled "The Terms of Auxology" (*Zoologischer Anzeiger*, November 14 and 28, 1892, pp. 420 and 429). Growth and change, as the authors observe, do not stop in an animal when the embryonic stage has been passed; nor is the study of later stages of less importance than that of the earlier. Thus has arisen a new science, of which embryology is only a part; and, as was inevitable, definite names have been given to the various post-embryonic stages. Unfortunately, the names used were not only open to serious objection

on etymological grounds, but were used with too great laxity. As now revised, the technical terms are as follows:—*embryonic*, *brephic* (= infantine or larval), *neanic* (= adolescent), *ephebic* (= adult), and *gerontic* (= senile), the last being subdivided into *catabatic* (= declining) and *hypostrophic* (= atavic). These terms are restricted to stages in the growth of an individual. By prefixing the syllable *phyl-* they may be made to express stages in the history of a race; while the successive stages in the evolution of a character may be designated by the same terms with the addition of the prefix *morpho-*. Confusion of terms leads most surely to confusion of thought, and if the latter be in any way dispelled by the labours of Messrs. Buckman and Bather, we are bound to be grateful.

The Growth of Shells.

A most peculiar, and, as far as is at present known, unique mode of increase in a Gastropod shell, is described by Mr. B. B. Woodward in the last number of the *Proc. Zool. Soc. London* (1892, pp. 528—540, pls. 31, 32). Broadly speaking, the Gastropod shell may be looked upon as a more or less elongate conical tube, at the apex of which is the young shell, while increase takes place by the addition of fresh material at the other, open, extremity—the mouth. This tube is usually spirally coiled in the direction of a screw, the successive whorls touching one another. In those forms where the spire is low and the whorls are close together, the portion of the tube next the previously-formed whorl is sometimes largely, sometimes completely, dispensed with, and what was in the first instance the outermost wall of the tube becomes in the course of growth the dividing partition between the last whorl and its predecessor. When the shell is a very thick one, the presence of an equally stout internal partition (*paries*) between the whorls becomes unnecessary, and even inconvenient; hence it is very frequently to a great extent re-absorbed and reduced in thickness by the animal, as, for example, in *Conus*.

The Neritidæ advance a step further, and usually remove the greater part of the paries altogether, converting the shell into a single chamber, the only remnant of the dividing wall left being a portion near the mouth, to which one of the retractor muscles is attached. On the other hand, this remaining fragment of paries is usually strengthened, sometimes considerably, by an extra layer of shelly material. In extreme cases this deposit forms an independent projecting shelf (*septum*), standing out from the paries into the general cavity, between the former and the mouth of the shell. In these instances the septum forms the point of attachment for the retractor muscle, and the remnant of the paries is reduced to insignificant proportions or disappears entirely. The different species of *Neritina* and *Nerita* exhibit the various stages in the process.

Another feature of the Neritidæ is the presence of a thick shelly deposit (the *callus*) just without the mouth, on the surface of the last whorl. This deposit is thickest at the margin next the mouth, and generally at this point forms a ridge or even a thin shelf, or septum, which stretches partly across the mouth, reducing it in size. The full edge of this shelf is often serrate, sometimes coarsely so. As the animal grows and adds to its shell, still winding round in a spiral direction, these various deposits advance with it by the simple process of the addition of fresh material on their outer sides (using the word "outer" in its relation to the mouth and the direction of growth), and the removal of a corresponding amount on their inner sides.

Having prefaced so much, it is easy to understand the interest of what took place in the most eccentric Eocene member of the family, *Velates conoideus*, Lamk., as described by Mr. Woodward. In this species the growth of the young shell was perfectly normal, but when it had completed about 4½ whorls a remarkable change ensued; it ceased to grow spirally, and increased by the addition of fresh layers of shell over the whole surface of the callus as well as round the margins of its mouth, and gained the additional internal space the animal required by the removal of a corresponding amount of shell on the inner side of the callus. It thus changed both its direction and axis of growth—and like the Irishman, it raised its roof by digging out the floor of its tenement. In a full-grown specimen, therefore, nearly half the shell and the internal septum were carved out of former callus, the layers of which can be seen in sections cutting across the walls and running round on the inner surface. A still more remarkable feature is presented in the disposition of the component plates which, in the callus, are so arranged as to provide, in anticipation, the strongest possible structure for the muscle-carrying septum, ultimately to be sculptured out from it. The presence of silica in the outer layer of the shell is also noteworthy, and it is interesting in this connection to remark that one of the earliest describers of this fossil gastropod, and the author after whom it is sometimes called—C. C. Schmidel—observed, in 1780, that this outer layer did not appear to consist entirely of lime.

The Classification of Bivalved Shells.

After the embryologist has made a certain advance in the study of development, and the comparative anatomist has progressed in the determination of homologous parts, the services of the systematic zoologist are required to express the results in orderly sequence. Classifications are thus merely temporary expedients—generalised retrospects, so to speak—and every advance demands some reconsideration. At the present time Professor Carl Grobben (of Vienna) is of opinion that some reform is necessary in the classification of the

bivalved mollusca; and he publishes the following scheme in the *Zoologischer Anzeiger* for October 24 (vol xv., pp. 373—375) as most in accordance with the existing state of knowledge :—

Class: LAMELLIBRANCHIATA (PELECYPODA).

Sub-Class I.: PROTOBRANCHIATA.—With double comb-shaped gills; hinge toothless or with interlocking denticulations of the hinge-border or taxodont. Families: Vlastidæ, Cardiolidæ, Antipleuridæ, Lunulicardiidæ, Præcardiidæ, Silurinidæ, Protomyidæ [*Solenomya*], Solenopsidæ, Grammysiidæ, Posidonomyidæ, Daonellidæ, Nuculidæ.

Sub-Class II.: DESMODONTA.—With double lamellar gills; hinge-teeth wanting or irregular, arising in intimate connection with the ligament supports. Families: Pholadomyidæ, Myidæ, Anatinidæ, Panopæidæ, Septibranchia, Mactridæ, Pholadidæ, Gastrochænidæ.

Sub-Class III.—AMBONODONTA. With double lamellar gills; hinge-teeth inclined backwards on the hinge-plate of the shell, variable in position, may be wanting.

Order 1. *Eutaxodonta*. With taxodont hinge. Family: Arcidæ.

Order 2. *Heterodonta*. With heterodont hinge. Families: Astartidæ, Crassatellidæ, Chamidæ, Lucinidæ, Cardiidæ, Tridacnidæ, Cyrenidæ, Cyprinidæ, Veneridæ, Solenidæ, Tellinidæ, Donacidæ.

Order 3. *Schizodonta*. With schizodont hinge. Families: Trigoniidæ, Najadæ.

Order 4. *Anisomyaria*. Hinge-teeth wanting, or if present, isodont or irregular; with two very dissimilar hinge-muscles or merely a single hinge-muscle. Families: Aviculidæ, Mytilidæ, Pinnidæ, Pectinidæ, Spondylidæ, Ostreidæ, Anomiidæ.

THE CURVATURE OF PLANTS.

MR. FRANCIS DARWIN and Miss D. Pertz have lately been studying the artificial production of rhythm in plants, and the results appear in the last number of the *Annals of Botany*. The observations were made with the help of a new machine devised by the authors, which they call an intermittent klinostat. It differs from the ordinary klinostat in that movement is allowed only at regular intervals. The spindle is connected with a clock in such a way that a half rotation, *i.e.*, of 180 degrees, occurs at the end of every half-hour. Experiments were made with relation both to geotropic and heliotropic curvatures. In the former series, shoots of valerian and stalks of dandelion were used; these were fixed through a bored cork in test-tubes of water, which were attached horizontally to the spindle. The machine was placed opposite a window to cancel the effect of light in causing curvatures. When the clock was working, the influence of gravity on the growing shoot or stalk was, of course, reversed every half-hour, and it therefore received a stimulus of equal duration to curve towards opposite sides.

After the first few half-hourly intervals, the curvatures in either direction continued regularly for about thirty minutes. If the clock

were stopped, the rhythm was found to persist for a short time, the shoot or stalk actually curving in opposition to gravity for the half-hourly interval before finally obeying the impulse to grow downwards.

A similar effect was observed with heliotropic curvatures, for which seedlings of canary grass were used, the rotation being, of course, in a horizontal plane.

In one case, after the clock was stopped, the seedlings curved away from the light, for *two* half-hourly intervals, separated by one of curvature towards the light, so strongly were they imbued with the artificially-induced rhythm. The authors were evidently much struck with the result of their experiment; "to watch the movement of the free end of the shoot," they say, "and to see it reversed exactly at the expiration of half an hour is an experience so impressive as to compel belief. When a shoot is in a thoroughly rhythmic state, it is possible to prophesy to a minute at what time the reversal will take place." They compare the persistence of the rhythm after cessation of the stimulus to habit, like that acquired by a man who, after being regularly called for a time at 6 a.m. will awaken of his own accord at that hour. In the case, however, of the man, something in his surroundings, such as duration of light, may unconsciously affect him, but in the above experiments with plants, no such factor can have intervened, and the rhythm must be the result of an "internal chronometry," associated probably with the course of nutrition.

Recent Progress in the Study of Algæ.

The subject of the diseases of seaweeds is beginning to attract attention. So far, all that is known relates to the familiar *Vaucheria* galls and to the occurrence of Chytridieæ in *Sphacelaria*, *Cladostephus*, *Ceramium*, &c., as recorded by Magnus in the German North Sea Commission's Reports; while there are two papers by Miss Barton, one on galls in *Rhodymenia* caused by a copepod (*Journ. Botany*), and another on the malformations of *Ascophyllum* and *Desmarestia* (*Phycological Memoirs*). The thread-worm (*Tylenchus fucicola*), which causes the malformation of *Ascophyllum*, has been minutely described and beautifully figured by Dr. de Man in the recently issued *Festschrift* in honour of Professor Leuckart. Dr. Schmitz has described, at the British Association, Edinburgh, and in the *Botanische Zeitung*, certain tubercles of Florideæ, caused by the inevitable Bacteria; and a further contribution of great interest will shortly be made to the subject by Miss Frances Whitting, who is studying remarkable malformations of *Sarcophycus*, a Fucaceous Alga of the southern seas. This note of the literature of the subject may serve to call further attention to a very promising field of study.

A very admirable example of true methods in the study of Cryptogamic Botany is to be found in a memoir on the Algæ of P. K. A. Schousboe, contributed by M. Ed. Bornet to the *Société*

nationale des Sciences naturelles et mathématiques de Cherbourg (vol. xxviii., 1892). These Algæ were collected so long ago as 1815–29 by Peter Schousboe, Danish Consul at Morocco, and a part of them were bought by the King of Denmark, who placed them in the Botanic Garden of Copenhagen. The rest, together with a herbarium of Phanerogams, came into the hands of M. E. Cosson, and the late M. Thuret undertook and began the working out of the Algæ. He did not live to complete his labour, but sets were distributed by M. Kralik, under the name of *Algæ Schousbœanæ*, and the British Museum possesses a very excellent one. M. Bornet has set to work on the complete series in Herb. Thuret, and the result is the admirable treatise just issued. The numerous phycologists who have found trouble in dealing with the *Algæ Schousbœanæ* will be able to appreciate this authoritative publication, and especially the valuable critical notes on the species, as excellent and useful as any systematic work yet done by the great French botanist. A good deal of it will have particular interest to those who study seriously the distribution of our Channel Algæ.

Mr. James Ellis Humphrey, at present attached to the Agricultural Station at Amherst, Mass., is about to visit Jamaica for the purpose of working specially at the Algæ of that island. He intends to pay attention to the development of the multinucleate forms and in so rich a field is sure to meet with rewards.

Recent Progress in the Study of Fungi.

Among new mycological literature, the *Vegetable Wasps and Plant Worms*, by M. C. Cooke, furnishes an account of the Fungi that prey upon insects. His book is based on the privately-printed *Insect Bases of Fungoid Parasites*, by G. R. Gray (1858). Dr. Cooke brings it up to date in his own way, which is not remarkable for severe accuracy, but the result is a guide, at all events, to the literature of the subject.

Messrs. Ellis & Everhart's *North American Pyrenomycetes* has at last appeared and in a ponderous form. It is an excellent piece of work, and, so far as we have tested it, errs only in the references not being sufficiently exhaustive—especially the earlier historical references, if these may be so distinguished from the more modern.

Mr. Massee, whose activity in book production threatens Dr. Cooke's supremacy in this respect, has produced the first volume of his *British Fungus Flora* (Bell & Sons). Mixed with much that is both acutely and shrewdly critical, there is considerable credulity in the matter of taking references and statements for granted as correct. This kind of credulity may be beautiful as a matter of sentiment, and convenient in cases of haste, but it makes a vast difference to the value of a book.

An interesting and very comprehensive book on the truffle, by M. A. Chatin ("La Truffe," Baillière et Fils, Paris), has just been

published. It is a good example of the varying "form" displayed by a scientific author, his present effort being as successful as his *Anatomie Comparée* was the reverse.

IT is interesting to remark how the skeletons of animals that we have always hitherto regarded as typically Old World forms of life are still being discovered in the Tertiary formations of North America. The latest announcement is that of the discovery of a hyæna by Professor Cope in the Pliocene formation of Texas. It is an animal about as large as the common spotted hyæna, and is only known to differ from *Hyæna*-proper in the possession of a fourth premolar in the lower jaw. It is named *Borophagus diversidens* (*American Naturalist*, 1892, p. 1028). With the hyæna were also found two tortoises, one bird, one sloth, three mastodons, one peccary, three horses, one camel, a weasel, and a large cat. One of the horses—a true *Equus*—is remarkable for its small size, the teeth being no larger than those of a sheep.

SIR HERBERT MAXWELL and Mr. J. E. Harting, the Chairman and Secretary respectively of the Committee appointed by the Board of Agriculture to investigate the plague of field voles in Scotland, will shortly proceed to Thessaly to examine the results of Professor Loeffler's work in that region. As is well-known, the Professor has attempted to exterminate the Thessalian voles by placing in their food the germs of mouse typhus, and he claims to have been successful.

THE important monograph on the development of the macrurous Crustacea, by Professor W. K. Brooks and Mr. F. H. Herrick, to which we referred some time ago (vol. i., p. 243), has lately been issued in vol. v. of the *Memoirs of the National Academy of Sciences*. The other memoirs in the same volume relate to physical subjects.

WE have received from Mr. Hugh Fulton a photograph of the beautiful little land-shell, *Opisthostoma mirabile*, discovered a short time ago by Mr. A. H. Everett in North Borneo. In ornamentation this minute species rivals the *O. grandispinosa*, and several specimens have lately been obtained by Mr. Fulton.

IN the recently-issued number of the *Annals of Botany*, Dr. Schunck gives a short account of the work done on the chemistry of the green colouring matter of plants (chlorophyll) since the publication of his former paper on the subject in the volume for 1889.—In the same journal Mr. R. A. Rolfe describes a natural hybrid

between the frog orchis (*Habenaria viridis*) and the spotted orchis (*Orchis maculata*). It was found at Longwitton, in Northumberland, and shows, in a marked manner, a blending of characters derived from its two very distinct parents. The spur, for instance, is neither long nor short, but intermediate between the forms characterising the *Habenaria* and *Orchis* respectively. The hybrid receives the somewhat formidable name of *Habenari-orchis viridi-maculata*.

Attention has been directed to the subject of Lunar Volcanoes by Mr. J. B. Hannay (*Nature*, vol. xlvii., p. 7). He thinks that many of the craters look more like some disturbance in a semi-liquid surface than an accumulation of volcanic *débris*; and remarks that the rise and fall of a fused slag through holes in its solidifying crust, form features exactly like the craters in the moon. He suggests that, in the case of the moon, the rise and fall would be caused by the tidal motion of its still liquid interior. Mr. S. E. Peal also enquires (*Geological Magazine*, Nov., 1892, p. 501) whether the moon "could retain from the igneous-molten era, to its present *intensely* cold, airless and waterless condition its pristine surfacing, the very poles themselves being covered (it is urged) with large and small volcanoes, untouched by the hand of time." He has, however, written a book, which he speaks of as "my Theory of Lunar Surfacing by Glaciation."

The supply of natural gas and petroleum in Canada does not yet approach in importance that of the United States; we are glad to see, however, by the recently-issued First Report of the Bureau of Mines, 1891 [Ontario], that the industry is making steady progress. An Act has wisely been passed to prohibit the wanton waste of the by no means inexhaustible supply, and to provide for the compulsory plugging of unused bore-holes. The same report shows that nickel mining, of which so much was expected, progresses but slowly, the new alloys of nickel-steel not having yet come into use to any great extent. The ore is a mixture of copper-pyrites and nickeliferous pyrrhotite, usually associated with greenstone.

An interesting discovery of Lithographic Stone is recorded in the seventh volume of the *Mineral Resources of the United States*, lately issued by the U.S. Geological Survey. Material of excellent quality, comparing very favourably with Bavarian stone, has been found near Little Rock, Arkansas; and stone that has been tested and found serviceable, occurs at Fincastle, Virginia, and also in Blanco County, Texas. Preparations have been made for working the stone at the three localities. In England, trials for Lithographic

Stone have been made of slabs from the Lower Lias and White Lias (Rhætic) Beds, but not, so far as we know, with satisfactory results.

THE new part of the *Proceedings of the Geologists' Association* contains an interesting account, by Mr. W. J. L. Abbott, of the deposits and fossils found in excavations made for the foundation of the new Admiralty buildings at Whitehall. Full lists of the fossils are given, the most interesting finds being an undetermined species of water-tortoise (*Emys*), and leaves of the Arctic birch (*Betula nana*). This is the first time any Arctic plant has been found fossil in the Thames valley, and the water-tortoise in Britain was known only from Norfolk.

GEOLOGISTS will be glad to learn that a series of unpublished papers and notes by the late Professor Carvill Lewis, on the Glacial Geology of Great Britain and Ireland, will shortly be published as a small volume by Messrs. Longmans & Co. Dr. H. W. Crosskey is preparing an introduction.

A BIOGRAPHY of the late Sir Andrew Ramsay, who was for nine years Director of the Geological Survey of Great Britain, is being prepared by his successor, Sir Archibald Geikie.

A NEW monthly journal of Ornithology, the *Ornithologischen Monatsberichte*, is announced by Messrs. Friedländer & Son, of Berlin. The first number is to appear this month, and the annual subscription is 6 marks. The editor is Dr. Ant. Reichenow, and the main object of the journal appears to be the publication of brief preliminary notes on new researches, with a detailed index to current ornithological literature.

THE World's Fair at Chicago possesses an unusual interest from a Hygienic point of view. The probability is that cholera will break out again in Europe next spring. Whatever may be the vehicle of the cholera germs, it certainly tends to spread along the route where those who are affected have passed. It would certainly more or less affect the success of the Fair if intending visitors had any suspicion that dangers arising from this source were not entirely under control. There is also some reason to suppose that Chicago is well suited to become a sort of headquarter for this disease. In 1891 the typhoid fever death-rate of Chicago was twelve times as great as that of London. This state of the public health points to a water supply contaminated by germs, and it is natural to expect that, if the cholera germ by any means reaches Chicago, the water supply would be one of the most efficient means of spreading it.

One Jacob Horner has written a book called "Did a Hen or an Egg exist first? or My Talks with a Sceptic." One Mr. James Crompton has edited it, and the Religious Tract Society have published it in London in 1892. Mr. Jacob Horner says in the preface that he has found many of the artisans intelligent, that they are fond of an argument, that they want to understand the grounds of religion. "They like to get hold of a smart book with some dash and 'go' in it." Therefore has this book been written. It is a series of dialogues between an uncle and his nephew Tom Rod, "who has taken it into his head to be a Freethinker." In each round the stuffing is very agreeably knocked out of Tom till, at the end of the book, he writes a letter, the purport of which is that he can't come up to time again until he has been refilled. The stuffing is the very worst and oldest we have ever seen. It is, in fact, so bad, that it is hardly worth while pointing out that the nimble Uncle's blows are nearly all fouls. Take the first round as a sample. For brevity, we take the blows only and leave out the feints:—Uncle: Did a Hen or an Egg exist first? Tom objects to question. Uncle explains, and Tom admits that every egg comes from a hen, every hen comes from a chicken, and every chicken from an egg. Uncle puts in that Sir Charles Lyell says "You must explain the past by the present." Tom admits. Uncle: This must have held in the past. Tom admits. Therefore, first egg must have come from first hen, and so the first hen must have existed before the egg, and could not have come from an egg; and so Lyell's principle breaks down; you can't explain the past from the present. Stuffing badly out. There are 96 pages of this sort of thing. The last chapter is very appropriately entitled "Where are you going?"

Our first article this month relates to the life and works of Sir Richard Owen, who died on December 18. At the moment of going to press we regret to learn that the conspiracy of silence so long prevailing in certain quarters as to the great anatomist's writings, extended even to the arrangements for his burial. We have to record that a public funeral was not even offered, and Sir Richard Owen's remains were interred in Ham Churchyard in the presence of a small gathering of personal friends and official representatives of the scientific institutions of London.

I.

Owen.

I.—BIOGRAPHICAL NOTICE.

IT is with deep regret that we record this month the death of Sir Richard Owen. The ablest comparative anatomist of his time, Owen ranks with Darwin and Lyell as one of the greatest naturalists that England has yet produced.

Born at Lancaster eighty-eight years ago, Richard Owen received his early education at the local grammar school, and became, at the age of twenty, a student at Edinburgh University. His activity and talents enabled him to reap the fullest advantages offered by his teachers, among whom were Wm. Pulteney Alison, T. C. Hope, Robert Jameson, Alexander Monro (the third), and Alexander Barclay; and in 1825 he had already been elected President of the Hunterian Society, which he and others had formed a short time before. Leaving Edinburgh in 1826, Owen joined the medical school of St. Bartholomew's Hospital, where he came into contact with the celebrated John Abernethy. It was, it is interesting to note, to the Medical Society of this hospital that he presented his first published paper, "An Account of the Dissection of the parts concerned in the Aneurism, for the cure of which Dr. Stevens tied the Internal Iliac Artery," which appeared in the *Medico-Chirurgical Transactions* for 1830.

In the year 1826, he became a member of the Royal College of Surgeons, and contemplated entering the Naval Service. Fortunately, however, for Science, Abernethy, recognising the material in the man, obtained for him a post at the College of Surgeons, as assistant to William Clift. From this moment, the brilliancy of Owen's genius commenced to shine, and for fifty years, until age overclouded it, remained undimmed. His first work at the College of Surgeons was to assist Clift in the preparation of Catalogues of the Museum, which had been mainly formed by the anatomist, John Hunter, a man Owen greatly revered, and whose works he later on edited from the manuscripts rescued by Clift from the vandalism of Everard Home.

The publication of a detailed and exhaustive paper on the pearly nautilus (*Nautilus pompilius*), in 1832, at once raised Owen to the foremost rank of working naturalists. Not only was this memoir

complete in a zoological sense, but the views on structure and affinities therein expressed were considerably in advance of his contemporaries.

Continuing the work on the Hunterian specimens after the death of Clift (1849), to whose position as curator he succeeded, Owen issued many special catalogues of the collections, completing the series in 1854. During his twenty years' curatorship of the College of Surgeons, he so rearranged and added to the Hunterian Collections that in 1856 they filled ten times the original space allotted to them, and were suitably housed and displayed in three large galleries specially erected for the purpose.

Owen became public lecturer in comparative anatomy at St. Bartholomew's Hospital in 1834, and in 1836 succeeded Sir Charles Bell as Professor of Anatomy and Physiology in the College of Surgeons. He was also appointed in the same year to be the first "Hunterian Professor" at that institution. He became a Fellow of the Royal Society in 1836, in 1839 was made LL.D. of Cambridge, and in 1843 was elected F.R.C.S. Three years previously he founded the Royal Microscopical Society, and became its first President. In 1844 he was appointed to the Commission of Enquiry on the Health of Towns, and reported on his birthplace, Lancaster. He served also on commissions on the Health of the Metropolis and on Smithfield Market, was president of one of the juries at the Great Exhibition in 1851, and in 1855 filled the same position at the Exposition Universelle, Paris. In 1852, Owen was made D.C.L. of Oxford, and received the same year "Le Prix Cuvier" from the Institute of France, an honour peculiarly acceptable to one who had so industriously and brilliantly followed in the footsteps of that master.

Owen's connection with the College of Surgeons ceased in 1856, when he accepted the Superintendentship of the Natural History Department of the British Museum. Once in this position, he ceaselessly urged the necessity of more space for the growing collections, and in 1859 his "Report, with plans, of a National Museum of Natural History" was ordered by the House of Commons to be printed. Building operations eventually commenced in 1873, and the Museum in Cromwell Road was finally completed in 1880. It is, however, only fair to state that the building as finished but little represents Owen's ideas; one has only to look at his report and plans to recognise his knowledge of what was required, and to see how much the arrangement of the building has been sacrificed to architectural peculiarities. In recognition of his great services to science, Owen received the honour of knighthood on his retirement from the Natural History Museum in 1884.

Richard Owen was a giant, both in stature and in intellect, and the best monument that can be raised to his memory already exists in the Palæontological Galleries of the British Museum. Label

after label calls to mind that Owen first studied and described the remains exhibited, and though he was not free from the errors of the early investigators, and was very jealous of his contemporaries, his triumphs will linger in our memories longer than his weaknesses or mistakes.

Few students realise the magnitude of Owen's work, and it is only those who search in many fields that can comprehend the genius of the man who has now passed to his rest.

C. DAVIES SHERBORN.

II.—SIR RICHARD OWEN'S HYPOTHESES.

IT is now more than sixty years since the Zoological Society published the earliest of those anatomical papers on the Anthropoid Apes, among the later of which was the first thorough and detailed description of the skulls of the Chimpanzee and Gorilla. These, together with skilful restorations of the extinct birds of New Zealand and many other anatomical papers well-known to zoologists, will cause the name of Owen long to live in the grateful memory of all men who have the cause of Natural Science at heart. It seems, indeed, that the fame of our veteran Comparative Anatomist is likely rather to augment than decline. As time goes on and the disputes which formerly arose about the precise definition of "corpus callosum," and the presence of the "hippocampus minor" fade from memory, the many merits of the greatest English Zoologist of the first half of the nineteenth century will, we think, be more and more generally recognised. The esteem in which he has continued to be held during recent years is clearly shown by the award to him by the Council of the Linnean Society of their first Zoological Medal.

However Sir Richard Owen may in some respects have laid himself open to criticism, he has certainly been the victim of some injustice. It may be that a sense of such unfairness had its influence in that award to him of the Linnean Medal to which we have just referred.

This injustice mainly concerns some of his fascinating hypotheses relating to the essential nature of the processes of generation and repair, and concerning certain archetypal principles of our own bodily structure which he first made familiar in England.

We well recollect a brilliant lecture given at the College of Surgeons concerning "virgin reproduction," to which process, so far as we are aware, Professor Owen first applied the term Parthenogenesis. This lecture, revised and enlarged, was subsequently published.[1] It is with respect to this that a great injustice has been committed. It has been, no doubt, unwittingly committed by men who, while lauding or criticising Professor Weismann for his more

[1] On Parthenogenesis. London: John Van Voorst, 1849.

recent contributions to science, have failed to refer to the work and the hypothesis of their aged and illustrious compatriot. Yet in many respects he most certainly anticipated the ideas of the Freiburg Professor,[2] as we have before been careful to point out. It is, however, not so much Owen's hypothesis with respect to virgin reproduction that we desire here to refer to, as to that concerning his ideal generalisations with respect to the structure of vertebrate animals.

At the British Association meeting of 1846, and in his work on the Anatomy of Fishes, published in the same year[3], he put forward views which were fully developed in his work entitled "On the Archetype and Homologies of the Vertebrate Skeleton," published[4] in 1848, and further illustrated in his book, "On the Nature of Limbs,"[4] on the frontispiece of which the human and bovine structures are represented in juxtaposition, much as the skeletons of man and horse have been recently placed in juxtaposition by Sir Richard Owen's most able and indefatigable successor in the directorship of the British Museum of Natural History.

Owen's views were essentially those of Oken, as he lets his readers very plainly know. At page 73 of his work on "The Archetype," speaking of the vertebrate theory of the skull, he says:—"The gifted and deep-thinking naturalist, OKEN, obtained the first clue to this discovery by the idea of the arrangement of the cranial bones of the skull into segments, like the vertebræ of the trunk. He informs us that walking one day in the Hartz forest, he stumbled upon the bleached skull of a deer, picked up the partially dislocated bones, and, contemplating them for a while, the truth flashed across his mind, and he exclaimed, 'It is a vertebral column!'"

Sir Richard Owen's notion was that the skull consists of four vertebræ, with sense organs at the anterior part of each of its four osseous rings. The ear-capsule, in front of the first of the four (the occipital vertebra); the nerve ministering to taste in front of the second, or hinder sphenoidal vertebra; the organ of sight at the front of the third cranial (anterior sphenoidal) vertebra; and that of smell at the anterior part of the fourth or ethmoidal vertebra. He clearly shows (p. 74) how in all this he had been anticipated by Oken, and, on the next page, adds: "This will serve as an example of the close observation of facts, the philosophical association of their relations and analogies, and, in a word, of the spirit in which Oken determines the vertebral relations of the cranial bones. . . . . ." Reverting to the petrosal, Oken thus beautifully and clearly enunciates its essential nature and homology:—'You will say I have forgotten the part petrosa. No! it seems not to belong to a vertebra, as such; but to be a 'sense-organ,' in which

[2] In the number of *Nature* of November 14, 1889.

[3] By Longman, Brown, Green and Longman.

[4] By John Van Voorst.

[5] Also published by John Van Voorst.

the ear-nerve loses itself; and therefore as distinct an organ from a vertebral element as is any other viscus, or as is the eye-hole itself.' Although Oken does not in this essay formally admit a fourth vertebra anterior to the eye vertebra, he recognises the vertebral structure as being carried out rudimentarily or evanescently by the vomer . . . or the nasal bones."

Now we need hardly say that we do not give this citation with any intention of implying that the views of either Oken or Owen are true; but merely to show that the English naturalist did justice to his German predecessor—even to his suggestions with respect to the vomer and nasals, which are just those which Sir Richard Owen advocated.

In the present day both Oken's and Owen's archetypes have become obsolete, and we suppose no one now maintains them. Nevertheless, when they were first promulgated here, they produced no slight effect, they drew many thoughtful minds towards questions of biology, and they roused an antagonism which has also led to much valuable work. We believe them to have been, in these different ways, very serviceable to science, but we also think that they embodied, or were the mistaken outcome of, some deep and very significant truths which are, in general, far too little appreciated, a wave of sentiment, and the influence of a party (which could do much to make or mar a young man's progress), having combined to indispose many minds towards a dispassionate appreciation of them.

Personally, well disposed as we have always been to Owen, the Hunterian Professor, on account of much courtesy and kindness shown to us by him, we never accepted his representations in any detail, and were quickly convinced that he was quite mistaken as to his "Petrosal" in the lower Vertebrates. Although adopted by not a few persons, some of his views invited hostile criticism, and were soon attacked as being inconsistent, as they obviously were, with the facts of development. It was urged that the skull was made up of modified vertebræ. Its vertebrate character should be plainest in its earliest and least modified stages, and yet such stages had no resemblance to vertebræ at all. It was also pointed out that basilar cartilage and trabeculæ cranii were unlike anything to be found in the incipient vertebral column, the segmentation of which was sufficiently accounted for by the necessary action of pressures and strains on any frequently flexed cylindrical body. A flood of ridicule and sarcasm was poured on Owen's hypothesis, and the doctrine of archetypal ideas was supposed to have received its *coup de grace* from the theory of "Evolution," and above all, from that of "Natural Selection."

But Sir Richard Owen did much more than propound a modified Okenian theory of the skeleton, and indeed it would be a great injustice (an injustice which, nevertheless, has been ruthlessly perpetrated) to represent his theory as *mere* Okenism; for Owen gives no

sign of ever having entertained the fanciful notion that the head was a repetition of the whole body. By the clear and emphatic distinction he made between *Analogy* and *Homology*, by his account of special and general homologues, and by his exposition of serial and lateral homologies, he spread abroad in England the perception that a deep significance underlies the structure of animals—a significance for which no stress and strain, and no influence of heredity, and certainly no mere practical utility, can account.

The temporary overclouding of this perception through the retrograde influence of Darwin's hypothesis of "Natural Selection" is now slowly, but surely, beginning to pass away; for which no small thanks are due to the efforts of his zealous disciples, Professors Weismann and Romanes. It would be out of place to trouble our readers with a restatement[6] of simple facts of lateral and serial homology; we will confine ourselves to once more repeating the assertion that homologies for which neither heredity nor utility will account, reveal themselves in the limbs of chelonians, birds, beasts, and most notably in those of man.

It was, in all probability, a clear perception of this fundamental fact of animal organisation which led Sir Richard Owen at once to reject with disdain the teaching inculcated in the Origin of Species, a teaching eagerly welcomed by that Teutonic "wearer of motley" by whom, as Professor Cunningham well says,[7] *Halophysema* and *Gastrophysema* were "specially created," by the creator of *Bathybius*, and by that prolific Professor at Freiburg whom the destruction by facts of some utterly gratuitous hypotheses, does not for a moment deter from troubling us with others no whit less gratuitous than were their predecessors.

But to return to archetypal ideas and theories as to the nature of the skull. In the first place exception must be taken to embryology as the test. For, if we carry back such researches to the incipient germ, we shall find no available characters for discrimination at all, while diversities of interpretation of nascent structures are almost always possible, and the meaning of initial changes must be elucidated by the study of subsequent changes.

Thus, Professor Huxley tells us,[8] as to *Menobranchus*, that "no definite answer can be given" to the question whether the trabeculæ "grow into adjacent tissues as a tree pushes its roots into the soil," or whether their apparent distinction does not "arise rather from a chondrification of the pre-existing tissue in the immediate neighbourhood of the trabecular cartilage?"

Then, when ossification sets in, the meaning of the several ossific centres, as they arise, must be interpreted by their later stages. How

[6] Readers interested in such questions may be referred to Proc. Zool. Soc., 1884, p. 462.

[7] NATURAL SCIENCE, September, 1892, p. 544.

[8] Proc. Zool. Soc., 1874, p. 199.

else could epiphyses ever be discriminated? Again, the circumstance of a bone or cartilage making its appearance as a single element may in any case be due to the junction of its incipiently distinct parts at a period anterior to possible observation; it may be *connate*, *i.e.*, never distinct to observation, though judged from analogy to be essentially multiple. Of such rationally-inferred but invisible distinctness, we have examples in botany. The *results* of development are surely as much to be considered as are its earlier stages.

As to the vertebral or non-vertebral nature of the skull, it seems to us that such vertebral nature may be affirmed in one sense and denied in another, according to the line of thought which is followed.

It is undeniable that it is within the *laminæ dorsales* of the embryo that the cartilages and bones of both the brain-case and of the dorsal portion of the spinal cord are alike formed. Certainly also the bones of the skull—especially in the higher animals—present a singular reminiscence of vertebræ in the three serially successive arches they form; and if the essence of vertebræ consists in their being a series of bony rings fitted together, and serially enclosing the nervous centres, then, in that sense, the skull is in part composed of three vertebræ. In some fishes (*e.g.*, the sturgeon) the transition from the spinal column to the skull is so gradual that the two are difficult to distinguish. In the Silurian fish *Bagrus* (as Owen pointed out) the vertebræ next the head are greatly expanded, and join each other by suture, and this shows how undoubted vertebræ may simulate osseous cranial walls.

It is true that, unlike the nascent spinal column, the primitive skull presents no serial segmentation, and that the cartilaginous precursors of the cranial bones may even be divided medianly and antero-posteriorly. Nevertheless, in the young axolotl, before the base of the skull has become cartilaginous, an index of transverse segmentation is to be traced in the soft tissue of that region, in spite of the subsequent continuous chondrification of the base of the skull.

But most striking of all is the return made[3] of late years by Sir Richard Owen's main opponent to the conception that serial segmentation, however latent and disguised, extended primitively and fundamentally to quite the anterior end of the head, and that, judging from *Amphioxus*, the skull of the higher vertebrates may be represented as made up of something less than twenty serial segments, each of which is equivalent to a spinal vertebra with its annexed parts.

The conception of cranial vertebræ, then, like the conceptions of serial, lateral, and general homology, are subjective apprehensions of relations having an objective existence in nature. They are like conception "types," which are not, of course, real objective entities, *as types*, though they are conceptions which have none the less an objective basis. The acceptance of the theory of evolution is no bar to the reception of the view which represents all organic forms

[3] Proc. Roy. Soc., no. 157, p. 127.

as having been specially created according to certain fixed and pre-ordained ideal types. The two beliefs, far from being reciprocally exclusive, can and do exist in perfect harmony.

Immediately underlying the question here touched upon reposes one of the deepest and most important of all philosophical questions, but we have no space to enter upon its consideration here. Profoundly impressed as we are, however, with the significance of questions touching philosophical anatomy, we cannot refrain from expressing our sense of the deep gratitude the world owes to Sir Richard Owen, who, years ago, forced questions such as these upon willing and unwilling ears. The details of his system are erroneous, much of it is fanciful and baseless. But he has nevertheless enriched zoology with a knowledge of a multitude of valuable facts, and many of his memoirs are models for those who come after him. To not a few kindnesses, and to many worthy actions, unknown to the world, we can testify. Of him, at least, it can never be said that he sought eminence through the degradation of his species, or that with selfish recklessness he threw broadcast seeds producing fruits fatally poisonous to all that is highest and best in man, or that gives any virtue, beauty, and dignity to human life. His work is done, his troubles, his labours, and disappointments are over. May his end bring him peace.

ST. GEORGE MIVART.

III.—SIR RICHARD OWEN'S RESEARCHES ON THE INVERTEBRATA.

IN the course of more than half a century's active devotion to scientific research, Sir Richard Owen has investigated the structure of almost every group of living and fossil organisms from man to the internal parasites affecting his frame. His contributions to knowledge of the invertebrated animals are not so numerous as his publications on the living and extinct forms of the vertebrata, but his attention was nevertheless directed successfully in early life to this interesting division of the animal kingdom, and his original memoir on the Pearly Nautilus, published in 1832, when he was only twenty-seven years of age, laid the foundation of his well-deserved fame as a physiological anatomist. Just twelve years later he was awarded the Copley Medal of the Royal Society, in recognition of the merits of a memoir on the fossil "Belemnites" published in the Philosophical Transactions.[10]

The general results of Professor Owen's investigations of the Invertebrata were epitomised in his Catalogues of the Fossil Invertebrata in the Museum of the Royal College of Surgeons, in which more than three hundred and fifty species of fossil cephalopoda collected in the previous century by his famous predecessor in office, John Hunter,

[10] In conjunction with the Memoir on the Monotreme Mammalia.

were described; in Lectures on the Comparative Anatomy of the Invertebrata, largely based on discourses delivered during his occupancy of the "Hunterian Chair," in that great institution, the foster-parent of so many distinguished reputations; in an article on Cephalopoda contributed to the "Encyclopædia of Anatomy and Physiology," and in that on the Mollusca, including the Molluscoidea, to the eighth edition of the "Encyclopedia Britannica." A remarkable article, entitled "Palæontology" was first published in the latter work, and subsequently enlarged and re-issued as a "Systematic Summary of Extinct Animals and their Geological Relations" in 1861 as a separate publication. (Part I., relating to Invertebrata.) This is one of the best examples of Professor Owen's literary powers of popular exposition of technical details. The Memoir on "Parthenogenesis," treating of the phenomena of asexual generation among certain groups of Invertebrates, belongs more properly to the category of his speculative and philosophical inquiries.

We will now refer briefly to some of the most important of Professor Owen's publications on invertebrates, grouping them for convenience more in zoological order than chronological sequence. Observations on the structure of a new genus of "well-woven" sponge *Euplectella aspergillum*, Owen, appeared in 1841; and in 1857 a new species of the same interesting genus was described. "The threads of the *Euplectella* were not first spun and then interwoven," he wrote in his second memoir on the genus, "but were formed as interwoven, the two processes going on simultaneously; or *pari passu*, in the living state, the exquisite structure of the flinty framework may be veiled by the delicate, gelatinous, enveloping, organic tissue."

No less than five memoirs on the Entozoa appeared in the first volume of the Transactions of the Zoological Society issued in 1836, and his comprehension of "the structural differences existing among them" induced him to propose a more natural classification of this difficult group. His descriptions and figures of a new endo-parasite, first discovered by Mr. (now Sir) James Paget, his fellow student, at St. Bartholomew's Hospital, infesting the voluntary muscles of a human subject, and the subsequent recognition by Zenkrer of *Trichina spiralis*, Owen, as the cause of a troublesome and often fatal disease now known as Trichinosis, led to results of practical utility to mankind.

An interesting paper, entitled *Protichnites*, was published in 1852, wherein the fossil footprints of ancient crustaceans were tracked in the sands of time. "Old Nature speaks as plainly as she can do by these distinct symbols, and if we do not fully thereby read her meaning, the fault is in our powers of interpretation. The creatures which have left their impressions on the most ancient of known sea shores belonged to an articulate and probably crustaceous genus, and *Limulus* comes the nearest to my idea of the kind of animal which has left the impressions on the Potsdam Sandstones."

Twenty-one years later Professor Owen published an elaborate and well-illustrated memoir on that interesting, abnormal, but mis-named arthropod, the King-Crab (*Limulus polyphæmus*), which is no crab, and has since been definitely promoted out of the class of water-breathing Crustacea into that of the air-breathing Arachnida and associated with the Spiders and Scorpions.[11] He detailed the muscular, nervous, digestive, and generative systems, and proposed the term "Cephaletron" for the anterior division of the body, and "Thorocetron" for the second; while he declared "the chief fossorial agent, as indicated by the size and disposition of the principal muscular masses, to be the cephaletral digging-shield." While recognising some of the Arachnid affinities of the King-Crab as then maintained by Strauss-Durckeim and Latreille, Owen did not admit the necessity for removing the genus from the order of Merostomatous Crustaceans.

Professor Owen's numerous memoirs on the anatomy of the Brachiopoda were second only in importance to his researches on the Cephalopoda. In both classes he founded orders which have been generally[12] accepted by biologists, and still maintain their position as recognised additions to zoological nomenclature and classification. In the article on "Mollusca," published in the eighth edition of the "Encyclopedia Britannica," the sub-kingdom Mollusca was divided into the *Heterogangliata*, with one nerve ganglion, and the superior *Homogangliata* of Owen. The group was compared to a tree, with the Cephalopoda at the summit, one of the roots appeared to be lost in the Turbellarian and the Trematode families of Abranchiate Vermes; the Brachiopoda conducted to the Bryozoa, and both, with the Tunicata, were considered as "mollusc-like" rather than molluscan. The Brachiopoda were regarded as a class equivalent in value to the Lamellibranchiata, and were placed between those acephalous molluscs and the sea-squirts, that is to say, between the "Acéphales testacées," and the "Acéphales sans coquilles" of Cuvier. In 1833 the anatomy of the "Cuvierian genera of Brachiopoda," more especially of *Terebratella* and *Orbicula* (now known as *Discina*), were described. The circulatory, muscular, and nervous systems of other genera were more fully investigated twenty years later, and the results published, accompanied by beautiful drawings by the author, in a "Memoir on the Anatomy of Terebratula," in the Introduction to Davidson's classical Monograph of the "British Fossil Brachiopoda," elucidating some points in the structure of *Waldheimia* and *Lingula*. Owen's brachiopodal hearts are now known

[11] "Limulus an Arachnid," by E. Ray Lankester, F.R.S. *Quart. Journ. Micros. Sci.*, 1881.

[12] Waagen and F. A. Bather excepted. Mr. A. H. Foord, in his exhaustive "Catalogue of the Fossil Cephalopoda in the British Museum," 1888 (p. viii., Int., part i., Order Nautiloidea), states that "no evidence has as yet been brought forward that the fossil forms included by Owen in the Tetrabranchiata were other than four-gilled."

to be oviducts, and it has since been made evident that he did not recognise the closure of the intestine in certain genera—a physiological feature of ordinal value. But he divided the class into two orders, so well differentiated by other characters, that the names of *Lyopomata* (or loose valves) and *Arthropomata* (or jointed valves), which he applied to them respectively, should retain priority over the *Articulata* and *Inarticulata*, the *Tretenterata* and *Clistenterata* of other investigators, of which subdivisions they are the exact equivalents. Owen's researches on the internal structure of the Brachiopoda marked a distinct advance in knowledge of this difficult and obscure group of organisms.

In a memoir on the anatomy of *Clavagella*, Professor Owen noted the enormous development of the mantle in the "club shells" as one of the instruments in the work of excavation for which these bivalves are remarkable. Some observations on the camerated structure in the valves of the water-clam (*Spondylus varius*) were first printed in the *Proceedings of the Zoological Society* for 1837, when attention was directed to the chambered and septated condition of the shell in that genus, and to the connection existing between the mode of growth in *Nautilus* and that of other molluscs which partition off a part of their shell not required for the convenience of the soft parts of the animal inhabiting it. It was then distinctly stated that the septal divisions of the *Nautilus* shell are analogous to the septa of the "water-spondylus," and many other shells which partition off the disused portion of their houses both among univalves and bivalves. An investigation of the anatomy of the gasteropod family of the "bonnet-shells" (Calyptreidæ) proved of interest "as manifesting some of the successive stages of complexity in the passage from the simple *Patella* to the spiral valves." Professor Owen's studies of the univalve mollusca in general, and of the recent *Nautilus* in particular, led him to the conclusion that the Pteropoda had less affinities with the Dibranchiate, or "two-gilled" cephalopoda, than the recent *Nautilus*. This form he considered to be the type of an inferior order of tetrabranchiate, or "four-gilled" cephalopoda, represented by numerous fossil species, and as an osculant form between the cephalopoda and gasteropoda. In consequence of this conviction, he transferred the encephalous floating Pteropoda from above to below the Gasteropoda[15] in his classification of the Mollusca, which is based chiefly on the conditions of the respiratory organs, as a physiological character intimately connected with activity of locomotive functions.

The subject of the Pearly Nautilus had been strongly recommended to his attention by Baron Cuvier, in whose dissecting rooms young Owen enjoyed the privilege of working. The famous French zoologist had never seen a specimen of the animal of this genus,

[15] The Pteropoda are now considered to belong to the inferior branch of the class Cephalopoda by those who place the class Gasteropoda at the summit of the Molluscan series.

which was known to Aristotle, and had been briefly described by Rumphius. It was, therefore, with peculiar satisfaction that Mr. Owen received an example in alcohol which had been captured, in a dying condition, by his friend and former fellow-student at the College of Surgeons, Dr. George Bennett of Sydney, while cruising in the Polynesian seas. The results of his investigations appeared in the "Memoir on the Pearly Nautilus, with illustrations of its external form and internal structure," which, to the keen regret of the author, was not issued from the press until three days after the death of his illustrious master. The animal was shown to be characterised by the presence of pedunculated eyes, calcareous mandibles, like a bird's beak reversed, a crop, gizzard, and liver, four gills, or breathing organs, and by the absence of a branchial heart and ink-bag. It occupied the last and largest chamber of a nacro-porcellanous, many-chambered shell, into the last cell of which it could retract itself and its numerous simple non-aceptabulated arms closing the mouth of the shell by the dorsal arms and by a horny hood-shaped structure, the analogue of the aptychi or double operculum of the fossil Ammonite. A central siphuncular tube pierced all the septa running through all the chambers or so-called "air-cells" which had been occupied by the animal in succession. This siphuncle "subserved a hydrostatic function," enabling the animal to rise to the surface and sink to the bottom, possibly by means of a gas secreted from the mantle. But the author's remark that "much remains to be done before the theory of the chambers and siphuncle can rest on the sound basis of experiment and observation" is still applicable.[14] The order Tetrabranchiata was proposed to receive the recent *Nautilus* characterised by the presence of four gills, a many-chambered external shell, and the absence of an ink-bag; and the fossil Ammonites and their allies were included therein. The Dibranchiata comprised all the two-gilled naked Cephalopods with an internal shell, or pen, and an ink-bag such as the living octopoda, the squids, cuttles, and the fossil Belemnitidæ.

In 1844 Professor Owen resumed the study of fossil cephalopoda, and communicated descriptions and figures of "certain Belemnites preserved with a great proportion of their soft parts in the Oxford Clay at Christian Malford, Wilts." The guard sheath or rostrum, known as the dart or javelin, whence the name Belemnite was originally applied, was described; the chambered or vertically septated portion was named the phragmacone; while the presence of an ink-bag, internal shell, muscular mantle, and aceptabulated arms were duly noted, and the fossil molluscs were classed with the recent

[14] There appears to be no doubt that the deserted chambers of the *Nautilus*-shell contain, in the healthy living animal, a gas which serves to lessen the specific gravity of the whole organism. . . . "A certain stage is reached when no new chamber is formed; with regard to its origin we can only conjecture; the whole matter is involved in obscurity." Ray Lankester, ninth ed., Ency. Brit., 1883.

Spirula, in the order Dibranchiata. The examples of *Belemnites oweni* upon which this memoir was founded, were subsequently shown by later investigators to belong to the animal of *Belemnoteuthis*, an extinct member of the existing squid family, and the animal of the true Belemnite proved to be characterised by the presence of an internal pen or pro-ostracum not found associated with the fossil remains of *Belemnoteuthis*.

In the same year Professor Owen communicated to the Zoological section of the British Association two papers detailing the "Further Experiments and Observations on the *Argonauta argo*," by Madame Jeannette Power, the originator of marine aquaria. He prefaced them with some remarks on the relation of the animal of the Paper Nautilus to its shell. The experiments which proved that the dorsal arms were the fabricants of the shell instead of the mantle, as in the Pearly Nautilus, had been suggested to Madame Power by Professor Owen. He has also described *Rossia*, a sub-genus of *Sepiola* (*R. palpebrosa*, Owen). In 1848 he enjoyed an opportunity of dissecting an unique but fragmentary specimen of the animal of *Spirula peronii*, and a portion of *Spirula reticulata*, and contributed the results to the "Zoology" of the "Voyage of H.M.S. 'Samarang.'" Thirty years later he returned to the subject of the structure of this interesting genus.

In 1878 Professor Owen discussed with much vigour "The Relative Positions to their Constructors of the Chambered Shells of Cephalopods." In this paper he showed that the shells of *Nautilus* and ammonites are revolutely spiral, or coiled over the back of the animal, not involute like the *Spirula*, and maintained the opercular character of the aptychi, which had been doubted by Keferstein and Waagen. He reiterated the opinion that they correspond to the fibrous hood of *Nautilus pompilius*, and are the calcifications of an ammonite hood. The fact that no trace of the ink-bag has ever been found with any fossil ammonite although that organ occurs abundantly in the fossil Belemnitidæ, is cited as conclusive evidence that the Ammonites were tetrabranchiates protected by an external shell, and he points out that the occurrence of the protective ink-bag in the *Spirula* proves that mollusc to belong to the more active dibranchiate order, with an internal chambered shell—the homologue of the phragmacone of the Belemnite.

The ink-bag does not occupy the last chamber of *Spirula*, as had been maintained in Woodward's "Manual of the Mollusca." The situation of the small pyriform ink-bag in that genus being "rectal," as Owen first demonstrated in 1848. It is denied that the septated condition of the many-chambered shells of the *Nautilus* was related conditionally to the generative function and periodical increase, as had been asserted by Professor Seeley and by Dr. Henry Woodward; for the reason that the formation of such chambers commences from the embryonal cup ("protoconch") and continues through an early

period of growth antecedent to the acquisition of the procreative functions, or the adult stage of existence, and that these early chambers are relatively deeper than the succeeding ones.

In this comprehensive review of the subject Professor Owen states that he "holds by the opinion expressed in his original memoir and in the 'Catalogue of the Fossil Cephalopoda in the Hunterian Museum' (4th ed., 1856, p. 29), that they so affect the specific gravity of the active, highly-organised, cephalopodous mollusc, as to enable it with little effort to rise, in the case of the *Nautilus*, from its habitual position at the bottom of the sea, and in the case of the *Spirula*, to sink from its more usual zone at or near the surface, by means of the hydrostatic mechanism worked by the muscular forces of the mantle and funnel. The constancy of the siphuncular connection running through all the chambers of the largest and most complex of the polythalamous shells, with the great size and singular complexity of the siphuncle in several extinct species, form the grounds on which I still hold to my original belief in the function of the siphuncle as related to a maintenance of the vitality of the shell."

In the following year, Professor Owen published the results of his dissections of a perfect specimen of *Spirula australis* received from Mr. Cuming. The animal was described as almost as devoid of external organs of natation as *Nautilus*. In both, the direction in which such forces act is retrograde. *Nautilus* exercises them mainly by virtue of the muscular funnel, through which it forcibly ejects into the surrounding water the respiratory current. *Spirula* superadds to this the ejection of that volume of water upon which the cephalic arms and their basal webs contract after the fashion in which the other Dibranchiata, especially the Octopods, propel themselves backwards. *Spirula* is superior to *Nautilus* in the cephalic mechanism. In both instances of multilocular cephalopods, the natatory power is inferior to that of existing Dibranchiata. The distinction, therefore, between *Nautilus* and *Spirula* in regard to the shell in its protective relation is relative not absolute: in the one a small proportion of the shell is occasionally "internal," in the other a small proportion is always "external," in both the multilocular shells correspond with the phragmacone of the Belemnite. The tetrabranchiate *Orthoceras* may be called a representative analogue of the dibranchiate Belemnite, as the tetrabranchiate Ammonite is of the dibranchiate *Spirula*. The siphon is "ventral" and marginal in both kinds of coiled shells, but it runs along opposite sides of the coil. In *Spirula*, its position is internal, or ento-marginal; in ammonites, it is external or "ecto-marginal."

In 1883 the veteran anatomist published a paper on the "Aspect of the body in Vertebrates and Invertebrates," which formed, we believe, his last contribution to the anatomy of the invertebrated animals. His researches cover a period of fifty years and afford

constant proof of the patience, industry, and acumen of the author. "The progress of Palæontology since 1830," he wrote in 1866, "has brought to light many missing links unknown to the founder of the science. My own share in the labour led me, after a few years of research, to discern what I believed, and still hold to be, a tendency to a more generalised or less specialised organisation as species recede in date of existence from the present time."

AGNES CRANE.

(To be continued next month by an account of Sir Richard Owen's Researches on the Vertebrata.)

II.

Artificial Protoplasm.[1]

LET it be said at once, for the benefit of the timid, that Professor Bütschli does not claim to have made protoplasm. His sumptuous quarto contains the results of ten years' special work on the physical side of the phenomena of life. The author is very strongly opposed to that school of Biologists who attempt to surround the phenomena of life with unnecessary mystery, and who clamour at once timidly and virulently against all physical and chemical interpretations. He has now been able to make microscopic foams which in structure and behaviour, under various conditions, marvellously resemble the structure and behaviour of protoplasm. These foams have enabled him to bring together the observations of himself and others on the actual structure of protoplasm, and to furnish a striking theory of its physical nature.

He tells us that he began his attempts at the making of foams with the forlorn feelings of a nineteenth century alchemist. The starting point was this: If fine particles of a substance soluble in water be mixed with a fatty oil, and the mixture be placed in water, the water diffusing into the oil becomes arranged in minute droplets round the particles of soluble material, and a foam is formed. Owing to the difficulty of breaking up the soluble substance into sufficiently minute particles, the particles, viewed under the microscope, are very coarse. The drops are placed on the under side of a coverslip with wax feet, and the coverslip is placed in a drop of water on a slide. When such a slide is put in a damp chamber for about 24 hours, the drop becomes quite milky and opaque. When it is flattened out by pressing on the coverslip, or made transparent by allowing glycerine to diffuse through it, this drop shows that it is composed of a very fine froth. It seems probable that the froth is caused by the presence in the oil of a small quantity of soap. The soap is more soluble in water, and the solution of soap and water has gradually separated out in very fine droplets. Bütschli empirically found the best way to produce these froths. Fresh olive

[1] UNTERSUCHUNGEN UEBER MIKROSKOPISCHE SCHAEUME UND DAS PROTOPLASMA. By O. Bütschli. (With 6 lithographic tables, and 23 figures in the text.) Leipzig: Engelmann, 1892. Price 24 marks.

oil must be kept in a watch glass, at a temperature of from 50–60° Centigrade, for twelve days, or at 80° C. for about three days. It becomes nearly colourless, and rather thicker. This is placed in a mortar, and powdered, but not anhydrous, carbonate of potash is rubbed into it. Although the desired result is the formation of a soap in the oil, good froths are not obtained by adding fatty acids or soaps to the oil.

The prepared froths, then, consist of a multitude of tiny droplets of a solution of soap in water, each droplet being surrounded by a film of liquid oil. The films, when they are in contact, naturally run together, and so a structure of polyhedral figures is built up, just as when one blows into soap solution in a saucer, the mass of bubbles can be seen to consist of polygonal figures. Minute examination of the froth-structure shows that at the nodes where the films meet, owing to their liquid condition, slight expansions occur (*op. cit.*, p. 20, Fig. 2). These expansions, when the bubbles are very small, give a granular appearance to the whole structure—an appearance which is more striking when a part of the froth more than one layer of bubbles thick is examined. The general optical effect is that of a meshwork, with thickenings at

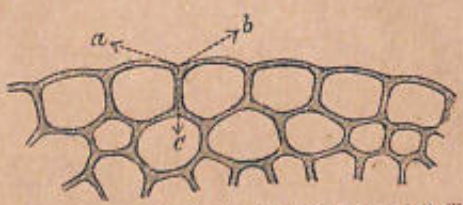

FIG. 1.—Foam under high magnification showing reticulate and fibrillar appearance and nodes.

the nodes. Bütschli found that when adventitious particles as, for instance, of lamp-black, are mixed with froth, these particles tend to collect at the nodes; but a careful study of the froth shows that node thickenings alone are enough to account for the granular appearance of protoplasm. He also shows how the familiar fibrillar, reticulate, and lamellar appearances, on careful focussing, are resolved into arrangements of bubbles.

He shows and explains, theoretically, how surface tensions make drops of such a froth, suspended in a liquid, assume a globular form, save under the influence of special internal or external forces. For some not very clear reason, the larger bubbles come to group themselves in the interior of each drop. Round the periphery, where the surface tensions are similar, a single row of bubbles of equal size becomes arranged (Fig. 1). The films separating these bubbles are radially arranged, and so there is formed a definite alveolar border. When the bubbles are small, this looks like a striated border. A similar border is naturally formed round any internal large vacuole. The alveolar layer, spite its definite appearance, remains as liquid as the rest of the foam, and persists while the drop is flowing.

These drops show very remarkable streaming movements. While they are in a weak solution of carbonate of potash they remain at rest. But this solution may be removed by drawing pure water under the coverslip on its wax feet by means of pieces of filter paper. Thereupon the drops, if they are not pressed on by the coverslip, move about actively. During this movement they may change in shape, the change consisting in the extrusion of processes of the foam in the direction of the movement. The internal streaming movements can be seen when the opaque drops are cleared by glycerine. It consists of an axial stream from the centre of the drop to the edge in the direction of the movement through the liquid of the whole foam. At the edge the stream turns back over the surface of the drop, and the backward streams so formed join the axial stream posteriorly. All this corresponds very closely with the streams and motion of a simple amœba. After a time the streaming from one centre ceases, a new set of currents form from a new centre,

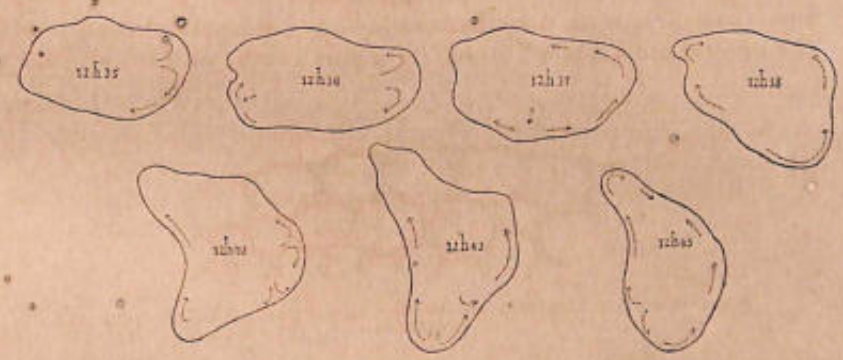

FIG. 2.—Figures of streaming movements observed in a drop.

and movement in a different direction occurs. Sometimes several centres of streaming are present together, and the whole drop excessively resembles an amœba. The streaming goes on from 24 hours to at most six days (Fig. 2).

The theoretical explanation of these movements depends on surface tensions. When a liquid is brought towards a drop of a second liquid suspended in a third, if the surface tension is less between the first and second than between the second and third, as the first liquid approaches the second the drop of the second stretches out towards the first, and a set of streaming movements occurs. Thus, in the figure some soap solution (first liquid) is brought up towards a drop of oil (second liquid) suspended in water (third liquid). Then the drop of oil moves gradually into the soap solution, and the currents shown by arrows in the figure appear (Fig. 3). The surface tension between the oil and soap solution is less than the surface tension between the oil and water. The equilibrium is destroyed and the soap solution spreads round on the drop from the

point of contact. As it passes round it gets more and more diluted, and the equilibrium continues to be disturbed. The surrounding fluid presses in to replace the soap solution, which is carried backwards from the point of contact, and this general pressure, acting alike on oil, soap, and water, moves up the oil drop slowly towards the point of contact with the soap solution.

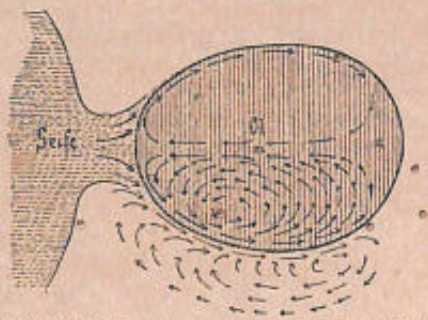

FIG. 3.—Soap solution (*seife*) in contact with drop of oil (*öl*) suspended in water, showing streaming movements.

In the actual case of the froths, the soap solution is contained in the cells of which the froth is composed. Centres of streaming movements are produced by the soap solution diffusing out into the surrounding liquid or by the bursting of superficial cells.

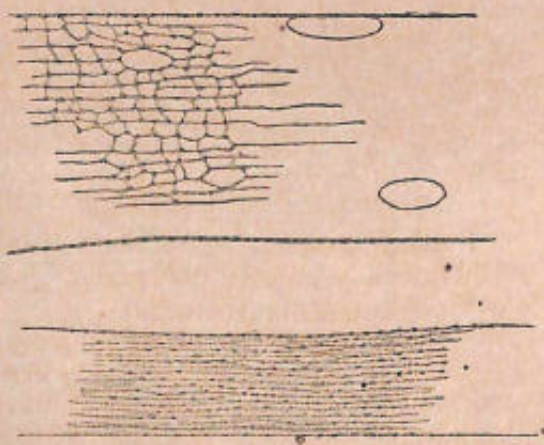

FIG. 4.—Foam of olive oil and carbonate of potassium, very viscous, much magnified, and pressed into fibrils by the cover glass. The upper figure is a higher magnification of part of the lower figure.

From a consideration of these froths Professor Bütschli passes to the observed appearances of actual protoplasm in the animal and vegetable kingdom, and from a large number of instances collected from all manner of cells, he shows how readily these appearances can be interpreted if we suppose protoplasm to have a frothy structure like the structure of his artificial drops. Of these two instances are reproduced opposite.

A large part of the remainder of the book is occupied by a discussion of various existing theories of the structure of protoplasm, and it concludes by foreshadowing possible explanations of various phenomena like the streaming movements in plant cells, and muscular contraction, upon the basis of his interpretation of the structure of protoplasm.

Put generally, Professor Bütschli's results are these: Protoplasm is itself a special chemical substance, the seat of many chemical changes. But it is also a physical substance, with definite physical texture, and obeying definite physical laws. It is possible to construct foams of different chemical nature but with similar physical properties. These foams show that protoplasm is composed not of a network containing sap and granules, but of a series of delicate bubbles obeying the physical conditions that rule bubble structures. The contents of the bubbles are some kind of fluid—a fluid which can be replaced by glycerine and other reagents. Many of the familiar features in

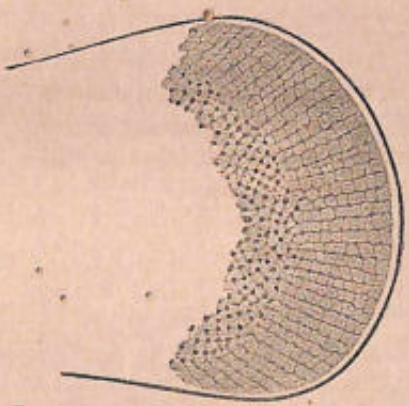

FIG. 5.—Protoplasm of *Amœba* (*Cochliopodium*?) *actinophora*.

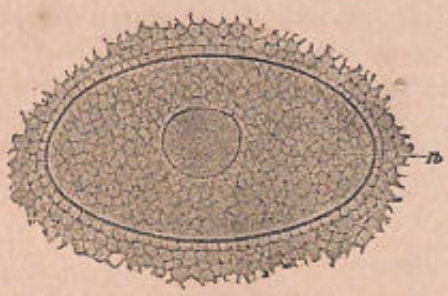

FIG. 6.—Nucleus with part of surrounding plasma from ganglia cell of ox.

the microscopic appearance of protoplasm—features like the network, the granular and fibrillar appearances, the striated alveolar layers, are the optical expression of this frothy structure.

Streaming movements, changes of shape, progression through fluids, are physical conditions. In artificial froths they can be referred easily to differences in surface tension inside the froth and between the froth and the surrounding fluid. The more complex chemical conditions and the active chemical changes in protoplasm render it highly probable that complicated changes of tensions within and without the cell are constantly being produced, and so there is to be expected a more lifely manifestation of the movements of streaming, of change of position and change of shape, which are actually present in the relatively simple artificial froths. A combination of fuller knowledge of the chemistry of protoplasm, combined with this fuller knowledge of its physical structure, would go far, if not the whole way, to an understanding of life.

This must suffice for a short account of the nature of Professor Bütschli's investigations. I am glad to be able to say that my friend Mr. E. A. Minchin has arranged with the author and publisher for the production of an English translation of this work. Until the appearance of this, everyone who can spare time to cope with an intricate and rather perplexing German style should go to the original. Vitalism, the empirical study of living organisms as living, may be the most presently fertile working hypothesis. But only those of little hope look for no final synthesis of the facts of Biology, Physics, and Chemistry, and even now they will have much ado when they try to discount Professor Bütschli's brilliant work.

P. CHALMERS MITCHELL.

IV.

Phases of Evolution in the Guiana Forest.

THE great forest region of South America, which extends almost to the shores of British Guiana, is one of the most interesting in the world to the naturalist. An ordinary observer may, perhaps, think its dark arches tame and dull, but even he is delighted with the beauty and variety conspicuous at every bend of a creek or on the edges of the savannah. Unlike the woods of Europe, so uniform in character, we have here trees of a hundred species struggling with each other for position, or straining their utmost to hold their own in the great battle for life. It is hard to find two individuals of the same species growing together; each appears to be fighting for himself, regardless of the others, whether akin or not.

As a result of this, specialisation is a most prominent characteristic, not only of genera and species, but even of individuals. The oaks and pines of temperate climates are undoubtedly variable to a considerable extent, but will not compare with the great family of Leguminosæ in the tropical forest, where a thousand contrivances are manifest in trunk, leaf, flower, and fruit. The different forms of the legumen alone are particularly interesting. Here we have the trysil (*Pentaclethra filamentosa*) with pods a foot long, and, farther on, the wallaba (*Eperua falcata*), which hangs its scimitar-shaped fruit on long strings. Then the tonka-bean with plum-like fruit, the arisaro and others with winged seed-vessels, and a host of shrubs and climbers with pods so curious and wonderful as to baffle the naturalist when he attempts to find reasons for their existence. Other natural orders, such as the Lecythideæ and Lauraceæ are also particularly interesting.

At first the wealth of species is almost bewildering; the forest alone would give work for a lifetime. It is so grand and magnificent —so crowded with immense trees, bush ropes, epiphytes, and parasites—so varied in species, as first to excite enthusiasm and then depress with the thought that life is too short to investigate it thoroughly.

Coming from a temperate climate, where the number of species in a wood can be counted on his fingers, the European sees above him a canopy of intermingled branches and foliage of a hundred different kinds which he cannot even reach, much less identify.

Nothing but an interminable assemblage of columns and a green roof above is seen, and even these but indistinctly, as not a ray of sunlight can penetrate the canopy.

On the ground the soil is covered with fallen leaves in various stages of decomposition, some already sunk into the rich brown humus so characteristic of the forest, others still retaining their shapes, and a few, newly fallen, mingled with petals and perhaps monkey-pots, pods, or other fruits. In one place the ground is covered with petals and the botanist turns to look for the tree from whence they have fallen, or even takes out his glass—but all in vain, for no particular tree can be separated from the throng; the flowers are outside in the light, and quite invisible from below.

Only on the edge of the forest, where it borders on the river or savannah, can the trees be identified. Here a mass of yellow shows that an etabally (*Vochysia*) or some leguminous tree is flowering, but it is almost impossible to procure a specimen. When the tree rises sixty or eighty feet without a branch, nothing less than cutting it down will serve. A few broken spikes may be brought down by a shot, or, perhaps, if the traveller is fortunate, one of his Indian boatmen may cut off a branch with an arrow, but to do either he must be fortunate indeed. True, many a specimen may be gathered alongside the river or creek, but some of the great forest giants are not found in such a situation. Even the mora (*Mora excelsa*), which is so common, hangs its flower spikes far overhead in a most tantalising manner, as if defying the botanist to gather them. With so few outside enemies, the trees of the forest have only to contend with each other for their shares of the light and moisture. Hardly a single ray can reach the ground. On the banks of the rivers where the continuity is broken, hosts of epiphytes crowd the great limbs and branches, utilising the diffused light. Seated on the upper sides, they extend outwards and downwards until they almost form curtains. Even those which grow upright often push their flower-spikes from the side or base so that they may gather more light away from the crowd. Ferns and orchids have developed this faculty to its greatest extent, hanging their long leaves straight downwards until they sometimes touch the points of other plants on the branch below.

If these do not take up every ray, a jungle of palms, tree-ferns, marantas, and arums crowd together underneath, along the rive banks, and prevent all access to the forest except by cutting a way through them. If the river-current is strong, no water plants can secure a footing, but in every creek there are bays where, given a little sunlight, patches of *Cabomba aquatica*, or water lilies, manage to exist.

The atmosphere inside the forest is hot, dense, and steamy. To the old colonist it suggests intermittent fever, but to the naturalist its peculiar odour recalls many a pleasant journey through creek and wallaba swamp, or, perhaps, remembrances of anxious hours when

he found himself lost in the forest. The perspiration streams from the new-comer, and unless he is enthusiastic in pursuit of his hobby he is glad to find himself again on the river. Where the land is low, pools of ruddy-brown water occupy the hollows between the immense buttresses of every tree, while here and there small streams wind in and out among them. The fallen leaves and other *débris* form an oozy kind of peat, obviously giving colour to the water, which is the same in almost every creek and river in Guiana.

It is generally considered that the forest is unhealthy for man. Residents along river banks suffer more or less from intermittent fevers, which often leave them after a change to the coast. In opening up new land by clearing, it is notorious that strangers suffer much while the work is going on. Swamps are also unhealthy for strange plants. Few cultivated fruit trees can endure inundations for even a day or two. Yet, under these conditions, the trees of the forest thrive. They have obviously become accustomed to environment such as would be dangerous to nearly all others. Is it not possible that what is dangerous to a plant is just as injurious to man, unless he has protected himself in some way? Also that there may be lessons to learn from the way trees have warded off the danger?

It is an undoubted fact that the forest giants of Guiana have evolved contrivances to prevent the development of low fungi. The most common of these is a thick bark containing tannin. This tannin is Nature's great antiseptic. It is found in all the waters of the forest and swamp, and accounts for that great difference which exists between the clear brown bog water and that seat of infection the stagnant pool. In towns and villages ordinary rain-water, if allowed to remain in open trenches, never resembles in its antiseptic properties that of the forest. The Indian has gone to the forest and learnt a lesson. When suffering from ulcers he scrapes the inner bark of some tree and applies it to the sore with most beneficial results.

Besides this, many other secretions have been developed. One of the most extraordinary is, perhaps, the milky juice which dries into india-rubber and gutta-percha. It is well-known that the Hevea grows only in places that are periodically inundated, and this characteristic is also strikingly exemplified in the tree which produces balatta, the gutta-percha of Guiana. It may be objected that some of the species of *Ficus*, which have similar juices, do not necessarily belong to the swamp, but perhaps their ancestors lived under different conditions. These secretions are so evidently fitted for protection against water that we can hardly conceive of them in any other connection.

Then there is another large class, the gum-resins and oleo-resins, also common in Guiana. Gum animi, the incense gum from the hyawa *(Icica)*, and balsam copaiba are the best known examples. The species of *Copaifera* are characterised by their thick bark, from which the Indian wood-skin canoes are made, and which are preserved

for a long period by their impregnation with the balsam. The tree which, perhaps, above all others delights in the inundations of the forest is the wallaba. In this we have not only a secretion of tannin, but also an oleo-resin which impregnates the tree to its very heart. Others have similar balsams, while some are so impregnated with essential oils as to smell quite strong, although not always pleasant. These secretions make the Guiana timbers so valuable, as they endure longer under water than those from any other part of the world.

Bitter extractives are found in the barks and seeds of many trees. Simaruba and greenheart are well-known, as they may be found in the British Pharmacopœa. No doubt they are also protective, perhaps chemically. Is it not possible that there is some truth in the old belief that medicines grow in every country suited to the disorders endemic to the place? The true explanation of this would be that plants protect themselves against evils which also affect man, and he, going to nature for a lesson, learns to follow their example.

Protected against floods and excessive dampness by so many contrivances, the forest trees are able to secure moisture at all times without suffering from drought or flood. With such a wealth of rotting leaves about their roots, there is never any lack of plant food, and as for light, they have to struggle hard for that. In doing this we cannot help seeing that they are intensely selfish. We can hardly conceive of selfishness without a self—an individual—and that every tree, and even the humblest plant, is such the naturalist is bound to admit. No matter that he cannot recognise one from another except in but few instances, the individuality is there and has been working for ages to raise the species to its present stage. According to the amount of individuality so will progress be greater or less. Heredity and environment may account for a great deal, but there is always something more than inherited traits and force of circumstances. Evolutionists have apparently thought too much of species and too little of individuals. At some time or other the individual was father to the species, and a few of the present generation may be progenitors of new forms, the careful observation of which will give the naturalist of the future the strongest proofs of evolution.

The tropical forest is undoubtedly made up of individuals. There is little of that apparent unison so common in temperate climates, where the trees open their leaf- and flower-buds together and at regular seasons. Here the ordinary observer says there are no seasons, but that trees flower and fruit all the year round. This, however, is an exaggerated statement not borne out by facts. There are two cycles in a year, the first in British Guiana commencing in February, and the second in August. As might naturally be expected, some species and varieties are earlier or later, and not only is this the case, but hardly two individuals flower, fruit, or drop their leaves at exactly the same time. This is most strikingly exemplified in the mango and other cultivated fruit trees. At the

present time (September) the mango has been flowering for two months, and trees may be seen in different stages from unopened flower buds to half-grown fruit. The fruit will ripen from December to February, when the great mango-season will be over, and only a fruit here and there be obtainable until June and July, when a second crop matures. The result of the two cycles and the difference in flowering and fruiting of individual trees is that most tropical fruits are obtainable more or less for six to eight months of the year, being, however, far more plentiful during the few weeks of each season when the majority ripen.

It is hardly necessary to bring evidence in favour of individuality among cultivated plants. Every gardener and florist knows that it is impossible to predict absolute uniformity in the offspring of a single plant, even when the greatest care is taken to prevent crossing. Everything goes to prove that the same individuality can be found, more or less, throughout the plant world, but it is especially recognisable in tropical plants. The analyst notes differences in the amount of active principle in samples of drugs, the tanner in barks, while the wood-cutter accounts for one log being more durable than another by false notions of the moon's influence.

Leaving the trees of the forest, which are so difficult to investigate particularly, and coming to the epiphytes which grow upon them, we find specialisation carried to extremes in the great orchid family. The collector, the dealer, and the grower will all admit individuality here. The varieties are so numerous as to be almost impossible to define. Not only are there differences between plants of the same species from several localities, but even among those collected within a small area. Any grower who knows his plants will recognise one from another, and be continually on the look-out when each flowers for the first time. On account of these differences, orchid collecting is particularly fascinating. A dozen plants of one species are bought at auction, and, until they flower, no one can tell how they will turn out. We often hear of unique specimens, but the fact of the matter is every one is unique.

In the case of the orchids, there can be no suspicion of man's interference. Artificial selection may have been concerned in the development of other families, but not of this. The wonderful variations in the leaves of crotons and flowers of the hibiscus family may, perhaps, be due, to some extent, to their appreciation by the Polynesians, but the Indians of Guiana wear no garlands, nor do they cultivate a single flower for ornament. It is not only among the more showy genera, such as *Cattleya* and *Odontoglossum*, that individuality is conspicuous, but brassias, gongoras, and even some of the more insignificant families, are equally variable.

It is only in our gardens that we can observe the many variations of large trees, and here we have, unfortunately, but few species from the forest. By studying the vegetation on every trip to the bush, we

see that, although there are differences between wild specimens and those under cultivation, these are generally unimportant. As we stated above, they have their two seasons every year, when they drop their leaves, flower, and later ripen their fruits. Flocks of parrots and other frugivorous birds indicate by their congregating that the fruit season has arrived, while a great increase of fish in the creeks confirms the fact. Haws and other berries of temperate climates often remain on the bushes for months after they are ripe, but most tropical fruits fall quickly. Birds and monkeys would suffer greatly if the season lasted for only a month or two. There appears to be little migration in search of food, although this takes place to some extent. Individuality among the trees is, therefore, of advantage to animals, as it lengthens the season and prevents anything like a dearth for a long period. Birds and monkeys hardly ever alight on the ground, so that once the seed or fruit has fallen it is lost to them and eaten by rodents, or carried away to feed the fish which congregate in every little stream.

Young trees are at first very erratic, opening their flowers and shedding their leaves regardless of the proper season. Some, like the flamboyant (*Poinciana regia*), are never bare, although the older trees lose their leaves entirely and remain so for several weeks. As they become mature their habits are more and more regular until individuality is lessened and they become part of the crowd. In studying the flowering and fruiting seasons, therefore, the naturalist has to generalise from trees of full age, and avoid all deductions from erratic youth. It is almost as difficult to find out their characters as those of young animals at a similar stage.

Among the difficulties of the naturalist are changes produced by removal. He cannot study the epiphytal orchids in the forest, but must bring them to his garden, and thus change their environment. Heredity and individuality will, of course, be the same under all circumstances, but the other great factor in evolution must be interfered with. Few orchids are fertilised when growing on the coast, probably because the insects of the forest are wanting, but these go to prove what a wide field for observation there is among this family alone. Almost hidden among the foliage of a tropical garden, however, some orchids will attract a crowd of insects as soon as their flowers are open. Some of these can hardly be found at other times, but let the flowers open in the early morning, and they call attention to the fact by humming and buzzing around them.

The student of nature in the tropics should be something more than a specialist. Flowers must be studied in connection with insects, and fruits together with birds, bats, and even fishes. The interdependence of animals and plants is so close, that they seem to be engaged in a race where each tries to outstrip the other for his own advantage. The tree invites the perfect insect to fertilise its flowers, but warns off the larva by nauseous secretions in its leaves. Some

caterpillar or other is, however, almost always found to digest the most repellent juices and make all such efforts fruitless. A few have attained a large measure of success on account of the toughness of their leaves and their covering of stiff bristles or thorns; but the most curious provision is, perhaps, that by which a garrison of carnivorous ants is accommodated. Either in swollen petioles or bases of thorns the ants take up their abode and fall upon and destroy every invader.

It is generally found that the trees most commonly attacked by larvæ are those the flowers of which are nectariferous, and therefore provide food for the perfect insect. The fiddle-wood tree (*Citharexylon*) is a striking example of this. Its spikes of white flowers perfume the air at night for long distances, and attract large numbers of moths. These same insects lay their eggs upon the tree, with the result that myriads of larvæ are produced, and the tree is often denuded of every leaf. In the ordinary course of nature it drops its leaves and is bare for a few days, after which the flowers are produced. The result of the ravages of the larvæ, like that of a ruthless pruning, is similar, so that by the time the perfect moth emerges from its pupa-case the flowers are ready to provide nectar for it. Other trees are attacked in the same way, and careful observation will no doubt show that there is something so remarkable in the actions of both plant and insect that nothing short of design can account for it.

Plants are undoubtedly improved by having to contend with so many difficulties. The survivor is always the fittest and most suited to the environment. One contrivance after another is developed, with good, bad or indifferent results, but generally somewhat to its advantage in over-reaching its neighbours. Meanwhile, however, its rivals have not been sleeping, but as fast as the one gains an advantage others will have put on some defensive armour, or perhaps become more fitted to assume the aggressive. The study of the innumerable contrivances by which the plants of Guiana have attained their present stage is a never-ending source of interest. Probably there is more to be learnt here in a few miles of forest, river, and swamp, than in the whole of Europe, while the difficulties of a thorough investigation of even one square mile are enormous.

In Europe a large number of trees are anemophilous, but here all are fertilised by insect agency. This is one of the reasons why insects are so plentiful. At some seasons great moths fly into our open rooms at night, often to fall a prey to the gas lights. Sometimes a table below the chandelier becomes littered with hundreds of small moths and other insects in an hour or two. If a tree is flowering outside, and happens to be one of those that are fertilised at night, the increase is most striking. The moths fly towards the white flowers and are apparently attracted farther by the intenser light of lamps. In the streets of Georgetown the arc electric lamps also attract great numbers of insects, including some of the largest moths, which may be seen wheeling round them almost like

swarms of bees. By far the greater number of insects are nocturnal in their habits, and, as might be expected, white flowers are very plentiful. This makes observation so very difficult, as few of the insects can be seen at work. Now and again, however, a belated moth may be seen taking a last sip in the early morning, but such cases are rare.

After the contrivances for fertilisation, those by which fruits are protected and disseminated are most interesting. Nearly all those of our tables have thick skins, often charged with acrid secretions. These have acted so well that fruit-eating flies and wasps are almost wanting in Guiana. Birds, however, peel some of them, while bats succeed in utilising all except perhaps those of the orange family. In the forest the variety of pods, capsules, berries, and other seed-vessels is almost overwhelming. Perhaps the most useful developments which are common to almost all are those by means of which they float and are carried long distances by water. This object is generally attained by means of spongy pericarps, but many other devices are common, all helping to spread the seeds over very wide areas. In the Lecythideæ we have apparently protective contrivances against monkeys. The Brazil nut (*Bertholettia*) has a hard, woody capsule without an opening, which falls to the ground entire. In the genus *Lecythis* there is a cover which falls off when the nuts are ripe, and allows them to scatter in every direction. It is probable that monkeys try to get at these, and by loosening the covers let them fall out without being able to secure more than one or two. The cannon-ball tree (*Couroupita*) has a softer covering, but this smells so very disagreeably that neither monkeys nor rodents will touch it. Other natural orders repel animals in different ways. The short brittle hairs on the cowhage (*Mucuna*) pods would undoubtedly prove more than distasteful to bird or beast, as would also the prickles on the seed-vessel of the *Allamanda*. Sometimes the seed itself is protected, while the covering or pulp surrounding it is obviously intended to attract. In such cases birds assist the tree by scattering its seeds, and are therefore invited. In the bignonias, so common along the banks of the rivers, the seed has a delicate membranous expansion, on which it floats in the air like a butterfly. Other climbers have feathery appendages, while the stately *Triplaris* lets its seeds sail down like shuttlecocks.

Fibrous coverings to the stems are common among the tropical weeds. Exposed as these are to vicissitudes unknown in the forest, they have developed contrivances distinct from those of the trees. The most common mode of overcoming the difficulty of a dry season when the roadsides can no longer supply sufficient moisture is the faculty of ripening seeds before this takes place. In the forest, every plant is a perennial, but annuals are common on the dry savannahs, as well as on waste land in the towns and villages. Many weeds are not only repellent to man, but even to domestic animals

and other plants. They are so aggressive in their very nature that they overpower the more delicate plants. All along the coast of British Guiana, where a narrow fringe is under cultivation, hardly a native plant is to be seen, but a host of tropical weeds covers the parapets of the roads and every piece of ground otherwise unoccupied. They even penetrate long distances into the interior where there is a clearing, ousting the more delicate ferns and grasses natural to the locality. These latter may occupy the provision ground of the Indian without interference, but where the white man or negro makes his home the weeds follow him. One of the most common in pastures is the black sage (*Varronia curassavica*), which stands up defiantly, as if it knew that not even a goat or donkey will touch it. But of all these pests the sour grass (*Paspalum conjugatum*) is, perhaps, the worst. It is so rampant in the wet seasons that it allows nothing else to exist where the ground is moist. Disliked by every animal, it covers all the road sides, and is a perpetual eye-sore to the agriculturist. Other weeds affect dry places, holding their own by pushing thick roots or tubers far below the parched surface. Like the forest trees, weeds struggle with each other, and, according to the season, either come to the front or give place. Where there is not much traffic on the roads, the grasses and other weeds cover them entirely, and even in the city they encroach so far that periodical cleaning is necessary. The dams of plantations are often almost impassable on account of tall grasses, which rise to a height of five or six feet. Many of these have serrated leaves, and the pedestrian, in pushing his way, gets his hands and face disfigured with scratches which in some cases may prove serious cuts.

As in the forest the trees take care not to allow the sunlight to penetrate and raise up a host of rivals, so on the plantation the agriculturist must keep the soil occupied to prevent weeds from spreading. The labour of weeding, when this is not done, is most arduous. In Europe, most of the pests of arable land are annuals which can be easily destroyed by the hoe. Here, however, the tufted grasses and sedges, deep-rooting shrubs, and tangled creepers are exceedingly difficult to eradicate. Only the dense jungle of a cane-field, or the shade of thick-growing trees, can keep them under without man's interference. Not a single weed shows itself among the sugar-canes after they have got beyond their earlier stage, and when ripe no large beast, much less man, can penetrate them. Here, instead of individuality, we have something like combination for an object, examples of which are more common in temperate climates than in the tropics.

JAMES RODWAY.

V.

The Distribution and Generic Evolution of Some Recent Brachiopoda.

WE had recently occasion to summarise the salient features of some modern publications on the anatomy and development of the Brachiopoda.[1] The past year was certainly remarkable for the production of excellent memoirs on this group of organisms. The great work, modestly entitled by Professor James Hall "An introduction to the study of the Genera of Palæozoic Brachiopoda," noticed in the October number of NATURAL SCIENCE (p. 628) is truly of an epoch-marking character, and a distinct revelation that the problem of the origin of species, so far as the Brachiopoda are concerned, is becoming merged in that of the evolution of genera.

It may be as well to recall the fact that the results of three American dredging expeditions and several French marine explorations have been worked out since Davidson's Report on the "Challenger" Brachiopoda was issued in 1880. It is from a review of the material obtained by the "Travailleur," the "Talisman" (2, 3), the scientific mission to Cape Horn of the "Romanche" (4), and three of Prince Albert 1st of Monaco's "Hirondelle" voyages (5) that the well-known French conchologists, Dr. Paul Fischer and D.-P. Œhlert have jointly derived the conclusions to which we are about to refer. It is fortunate that the rich harvests of results gathered in such distant regions should have been all entrusted to the experienced hands of the authors of the "Manuel de Conchyliologie et de Palæontologie conchyliologique." They have thus been enabled to formulate generalisations of value concerning the transition of genera, the distribution of some recent species, and their relations to the fossil forms of the Tertiary epoch. It is, perhaps, advisable to premise that Bayle's name *Magellania* is employed throughout to replace that of the far more familiar generic appellation *Waldheimia* of King, the use of which has been abandoned by Messrs. Œhlert, Fischer, Dall and others, because it had been previously given a few years before to a genus of hymenoptera; although, as M. Eugène Deslongschamps has observed, it would seem impossible that anyone could confuse a

[1] NATURAL SCIENCE, vol. i., p. 603.

Brachiopod with that most ill-met insect. We adopt the new name for the sake of uniformity, and in order to avoid the undesirable repetition of the bracketed tri-nomen which is making too frequent appearances in modern scientific literature. The substitute possesses the unusual merits of commemorating the exploits of a celebrated navigator, and of indicating at the same time the region of the maximum development of the genus it distinguishes; for the Magellanian province, as Darwin first pointed out, is remarkable for the great size of the mollusca inhabiting it, and to this rule the Brachiopoda form no exception.

First in interest and importance is the report of Messrs. Fischer and Œhlert on the Brachiopoda of the North Atlantic Ocean, which forms the third fasciculus of the fine series of Monographs embodying the results of the scientific explorations of Prince Albert 1st of Monaco on board his famous yacht "L'Hirondelle" (5). It gives a critical revision of the recent *Terebratulinæ*, accompanied by exquisite illustrations, and reveals some surprising facts in the general distribution of North Atlantic Brachiopoda, more especially in the Gulf of Gascogny, the archipelago of the Azores, and off the shores of the Soudan (5). The Gulf of Gascogny appears to be one of the richest localities, no less than sixteen species occurring there, including abyssal forms.[2] By far the most remarkable among them is the beautiful species founded on a single example first dredged by the "Challenger" Expedition from 390 fathoms off Culebra, one of the West Indian islands, and originally described by Davidson as the largest *Terebratulina* known (1).

This is undoubtedly a very interesting type. Fischer and Œhlert had several examples for examination, and were able to study the animal in detail, which was described by Willemöes-Suhm, in his MSS. notes, as relatively very small. It is now considered to be the type of a new genus, and of a new family (3), "characterised by its non-annulated loop and the arrangement of the arms, which, instead of forming free lobes, are re-united, and, rolled up in the pallial cavity, present a short sub-rectangular disc, a unique feature among the recent Brachiopoda. This disc can be schematically considered as composed of two rudimentary arms, joined laterally, and furnished with a large brachial sinus" (5). The figures now given show great incurvation of the beak area, and thickened margins of the valves—features which are also apparent in Davidson's drawings of the species. Hence we are inclined to consider *Dyscolia wyvillii* to be one of those atavistic types which Dr. Beecher states occur generally in families after the maximum of genera and species is attained.

This is a widely-distributed species, ranging from the West

[2] Twice that number of species are recorded, from Japanese and Corean waters, by Davidson in his "Monograph of the Recent Brachiopoda," *Trans. Linn. Soc. Zool.*, vol. iv. (1886–1888), p. 33.

Indies to the Gulf of Gascogny. It occurs off the East African coast, on the shores of the Soudan, and also in the latitudes of the Azores, another prolific region, yielding twelve species of Brachiopoda, of which four are also common to the West Indies, and eleven to European seas. A species dredged off the Portuguese coast, and named *Terebratula sub-quadrata* by Gwyn Jeffreys, is now regarded by Fischer and Œhlert as very closely allied to *Dyscolia wyvillii*, and the same authorities recognise *Terebratula (D.) guiscardiana* from the Sicilian Pliocene as an ancestral form of this interesting family Dyscoliidæ. The authors concur with Davidson in regarding the *T. septentrionalis couthouy*, which is larger and more rounded than the elongated and sub-pentagonal *T. caput-serpentis*, as a distinct species. They note, however, the occurrence of the varietal form they have named *T. germana*, from the Cape de Verde islands, and suspect it to be allied to the examples of so-called *T. septentrionalis*, recorded from the South Atlantic (Cape of Good Hope), the Indian Ocean, and Prince Edward's Island (5).

Sixteen deep-water species were obtained during the "Talisman" and "Travailleur" expeditions, three of which, *Rhynchonella cornea*, *Eucalathis tuberata*, and *E. ergastica*, were previously little known (2). Thirteen of them, according to Messrs. Fischer and Œhlert, occur in a fossil condition in the Pliocene marine deposits of Sicily and Calabria. These species are either absolutely identical, or at least exhibit such slender differences as permit their describers, in calling them by different names, to indicate a common origin. The Zanclean Sands of South Italy yield an assemblage of fossil forms which recalls the abyssal fauna of the marine Lusitanian province. We learn (2) that three species, *R. sicula*, *Dyscolia guiscardiana*, and *Muhlfeldtia (Megerlia) granulata*, have become extinct in the Mediterranean, while such closely allied forms as *R. cornea*, *D. wyvillii*, and *M. echinata* continue to be perpetuated in the Atlantic Ocean. Three other species found in the Pliocene of South Italy, *Magellania peloritana*, *M. euthyra*, and that widespread and interesting species *Liothyris sphenoidea*, which was long known as a fossil before it was dredged in the living state, appear to be absolutely on the point of extinction in the Mediterranean (2), although they are still represented in the broad Atlantic under the designations of *Magellania septigera*, *M. cranium*, and *Terebratula cubensis*.

Hence it is evident, the French conchologists conclude, that the Mediterranean has lost some of its deep-water species since the Pliocene period. This tendency is still manifested, and it is held to be associated with the gradual rise in the temperature of the Mediterranean waters which is about + 13° C., from below 183 metres down to the bottom. The Mediterranean now resembles an inland sea as compared with the Atlantic, where the temperature decreases as depth increases. The fact of the general uniformity of deep-water temperature, first established by the "Challenger" explorations,

accounts for the wide distribution of the abyssal fauna. During the Pliocene period, the cold currents penetrated into the Mediterranean, which then received a number of hardy boreal species, the remains of which have been preserved in the fossiliferous deposits of Ficcarazzi, in Sicily. The elevation of the sea-bottom at the Straits of Gibraltar checked the inflow of the cooler currents of the ocean depths, with the consequent result of a gradual rise of temperature in the then nearly enclosed sea. The abyssal forms, being unable to accommodate themselves to the changed conditions, died out in the thermal waters, but their collaterals continued to flourish and multiply in the favourable and unchanged conditions of the Atlantic Ocean (2).

These considerations, Dr. Fischer and D.-P. Œhlert maintain, confirm the hypothesis that the distribution of marine species is principally regulated by the temperature of the waters. Hence it arises that the Mediterranean Sea now possesses a rich surface fauna and a poor abyssal one; while the Lusitanian province beyond is characterised by a poor surface fauna and a deep-water fauna of extraordinary richness and vitality.

The results derived from a careful study of the specimens obtained by the "Romanche" during the French scientific mission to Cape Horn (4) were of a different character, and, while affording some distributional data of interest, which serve to emphasise the great dissimilarity existing between the boreal and the austral brachiopodal fauna, yielded more important evidence concerning the transitional stages of development of the larger southern species of *Terebratella* and *Magellania*. These successive stages are shown to be of a more complicated character and to differ somewhat from those undergone by the northern forms as first described by Herman Friele, who based thereon his simple division of the great family of Terebratulidæ into two groups of the short loops and the long loops since generally adopted. It is now quite clear that the genus so long known as *Waldheimia* is a closed type, the ultimate phase of a long line of development through successive pre-magadiform, magadiform, magaselliform, and terebratelliform stages culminating in the southern seas in *Magellania venosa*, *M. lenticularis*, and *M. grayii*. It is remarkable that the species of the Northern Ocean should reach the same goal by a shorter line and somewhat different stages, which Œhlert and Fischer define as centronelliform, ismeniform and terebratelliform, ultimately attaining the Magellanian grade as represented by *Wald. septigera* and *Macandrewia cranium* (4).

Davidson's classification of the long-looped species of *Waldheimia* with the short-looped species of *Terebratula*, *Liothyris* and *Terebratulina*, which assume their stable generic loop characters without undergoing any metamorphoses, cannot possibly be longer sustained. It is quite evident that *Magellania* has absolutely no affinities with the sub-family Terebratulinæ, but must be associated with the Terebratellinæ and should rank above that genus in a natural scheme of classification, as

Dall suggested in 1891.[3] The following subdivisions of the great family Terebratulidæ, which we should place in the following order, were proposed by Œhlert in 1887:—Terebratulinæ, Centronellinæ, Magasinæ, Muhlfeldtinæ, Terebratellinæ and Magellaninæ. The present investigations confirm this grouping. The genus *Terebratula* of the first-named and *Centronella* of the second are the more ancient terebratuloid types, and were represented in the Silurian seas.

Messrs. Œhlert and Fischer received the materials for their research on the Brachiopoda of the Magellanian province too late to permit of the publication of their conclusions in the series of memoirs relating to the French scientific mission to Cape Horn, during which the "Romanche" dredged often in many localities. Few species were obtained, but the number of individual specimens of different ages was large, and afforded them much desired opportunities for studying the metamorphoses of the loop and gradational development of *Terebratella dorsata* and *Magellania venosa*. The knowledge thus gained, it is distinctly stated, should tend to the suppression of many so-termed species and some assumed generic forms, for "the more the Brachiopoda are studied the more the number of legitimate species will decrease." It is demonstrated that the same individual in the course of its post-embryonic development assumes successive stages which are identical with both specific and generic forms of various authors. Thus it is fully proven that the genera *Waltonia* (Davidson) and *Magasella* (Dall) are really immature *Terebratellæ* in which the brachial apparatus has not completed its development. Gould's *Terebratella pulvinata* is shown to be merely a stage of *Magellania venosa*, which is not permanently arrested in the terebratelliform grade like its congener *Terebratella dorsata*, but assumes ultimately the characteristics of *Magellania venosa*. This is a fine but variable species with a large number of synonyms which have been well worked out by Davidson. Messrs. Œhlert and Fischer state that it attains maturity early, and its complete evolution was observed by them in specimens 27 mm. long in which all traces of the jugal band had disappeared. The largest known specimen measures 3 inches 2 lines in length and 2 inches 8 lines in breadth. *Magasella gouldi* becomes *Terebratella gouldi*, but it is not yet known whether it stops there or eventually assumes the generic characters of some species of *Magellania*. It is certain, however, that *Magasella evansi* is but a phase of *Terebratella cruenta*. *Magasella patagonica*, *M. flexuosa*, and *M. suffusa* name different ages of *Terebratella dorsata*, *M. lævis* another stage of *Magellania venosa*, while *M. adamsi* first becomes terebratelliform and eventually passes into *Magellania grayii* (4).

In the course of these investigations the important physiological facts of accelerated growth were recognised. Owing to favourable conditions of the environment, some adolescent individuals were

[3] The name *Terebratulinæ* was accidentally printed for *Terebratellinæ* on line 19, p. 603, of my paper "Recent Researches on the Brachiopoda," in NATURAL SCIENCE, October, 1892. The error escaped notice.

found to be obviously capable of reproduction during the Magasiform and the Magaselliform phases, which precede that of *Terebratella*, and thus an inferior race of *Magasellæ* is perpetuated which never attains the characteristics of the adult *Terebratellæ*. The authors deem it advisable, therefore, to retain Dall's subgeneric name of *Magasella* for such permanently arrested types, although it is quite clear that four-fifths of the so-called species will prove, as the American authority admits, to be the immature forms of different species of *Terebratellæ*, with which genus, it is well to remember, they invariably occur associated. Some *Terebratellæ* also remain so throughout life, but with others the terebratelliform stage is transitory, and they pass up into true Magellanæ. Many such grades were arrested and became stable during geological periods, and should therefore retain generic value. Among these the authors cite *Centronella*, *Magas*, *Terebratella*, and possibly *Ismenia* of King.

This is a distinct advance in our knowledge of the Brachiopoda, and throws some light on the origin of genera. It illustrates at the same time the difficulties under which palæontologists labour in describing and naming obscure fossil forms, deprived of the assistance to be derived from a study of the developmental history of any member of the group in the living condition. The simultaneous occurrence, so often noticed, of a smooth series and of ribbed examples of the same species, indicate the futility of relying on these mutable external features as indicators of species. The most divergent views are taken of generic values; for instance, Mr. H. Douvillé proposed his genus *Neothyris* for *Waldheimia lenticularis*, a well-known form occurring in the New Zealand province, both in the living and fossil condition, allied to the *W. kerguelenensis*, and so closely related to the giant *W. venosa* of the Magellanian province, that Dall even considers it as a varietal form or Novo-Zealandian race of the S. American species. "The difference in colour seems the main distinction" (4). Œhlert's sectional employment of the term Neothyrine as differentiating the smooth *Magellanæ* like *venosa*, and *lenticularis*, from the pleated forms, such as *M. flavescens*, is far less objectionable.

Now that *Terebratulina wyvillii* has been definitely promoted, and with good reason, to be the type of a new family, the Dyscoliidæ, in which the short united "arms" resemble a freemason's apron, the fine *T. crossii* from Japanese waters is the largest known species of that sub-genus. It is remarkable that it should have been also dredged by the "Romanche" off the coast of Patagonia. *Liothyris moseleyi*, first obtained by the "Challenger" off Kerguelen islands, and subsequently by Dall in the West Indies (Blake Expedition), also occurs off Tierra del Fuego, and proves to be a well-defined, far-ranging species of the widely-distributed Liothyrine group of *Terebratula*.

The Magellanian province, to which the Marion isles, the Crozets and Prince Edward's Island are now attached by Œhlert and

Fischer, is poor in species but rich in individuals. Favoured by abundant marine vegetation, the species so numerously represented attain relatively gigantic dimensions. Seven species occur, one of which, *Magellania fontaineana*, Davidson held to be only a synonym of *M. venosa*. The French conchologists consider it distinct, and as still inhabiting the littoral. It is found fossil in Chili, and *Terebratella crenata* occurs in far distant New Zealand. A close similarity, it is evident, exists between the brachiopodal fauna of the Magellanian province, where the species are practically colourless, and the bright-hued forms of the Novo-Zealandian. The rich colouring is regarded as an adaptation to the brilliant marine vegetation. The Kerguelen islands occupy an intermediate position, both as regards geographical situation and specific distribution, although the temperature of the bottom currents is identical with that of Tierra del Fuego. The fauna of the Magellanian province differs radically, alike from the Boreal and from that of the Tasmanian region, which is linked by its characteristic Kraussinoid types with the Cape of Good Hope, and connected with the Kerguelen islands by the occurrence of the same genus, a species of which abounds in the latitudes of St. Paul's islands. Fishes seem to be ardent collectors of Brachiopoda. Two specimens of *Magellania venosa* were found in the stomach of a cod in New Year's Sound. *Terebratulina septentrionalis* occurs frequently in the same situation off the banks of Newfoundland, and a much rarer species of the genus was originally described from a single specimen taken under similar conditions from a fish captured at a depth of 80 fathoms off the Mauritius.

A detailed comparison of the Austral species leads Messrs. Œhlert and Fischer to confirm the hypothesis "of the existence of an extended but hitherto little-known Arctic and circumpolar fauna." In a short paper, just issued, the authors summarise the results of their observations "On the evolution of the brachial apparatus of certain Brachiopoda" (6), and present a tabular view of the affiliation of the *Magellanæ*. They suggest the subdivision of the genus into two sections, differentiated by distinct lines of development and by the characters of the adult species. By their original and well-executed researches they have added considerably to our knowledge of the distribution and generic evolution of the recent Brachiopoda.

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AGNES CRANE.

V.

Aggressive Mimicry Among the Flies of the Genus *Volucella*.

MUCH attention has recently been drawn to the resemblances which exist between the forms of *Volucella* and certain species of Hymenoptera. Not only are theoretical explanations of the resemblances in dispute, but there is disagreement as to the exact species of *Bombus* which are mimicked. It may therefore be opportune to describe briefly the characters both of the Diptera and Hymenoptera concerned.

In England, there occur four species of the genus *Volucella*; of these, *V. inanis*, *V. pellucens*, and *V. inflata* possess a smooth or nearly smooth body, while *V. bombylans* alone possesses a markedly hairy body.

V. inanis is parasitic in the larval condition on the larvæ of wasps and hornets; the adult in no small degree resembles a wasp. *V. pellucens* and *V. inflata* are similarly parasitic on wasps, but neither can be considered to bear a resemblance to its hosts in the adult stages. The larva of *V. bombylans* is, in the same way, parasitic on certain species of the genus *Bombus*. The adult of this last occurs in two varieties; the dimorphism affects both sexes, and intermediate forms are occasionally found.

The one variety is black, with the apical portion of the abdomen red. It thus closely resembles, both in size and colour, a small worker of *Bombus lapidarius*, or of *B. derhamellus*, or of *B. sylvarum* (in some of its variations). The other, var. *mystacea*, has a yellow border to the otherwise black thorax, yellow hairs on the basal portion of the abdomen, which is itself in this region yellow and semi-transparent, and a grey or greyish-yellow apex to the abdomen. The exact extent of the yellow colouration is liable to variation both on the thorax and abdomen. Here the resemblance is to a worker of such species of *Bombus* as *B. pratorum*, *B. hortorum*, *B. schrimshiranus*, *B. terrestris* (*lucorum*), or *B. sylvarum* (in other variations). The two varieties present also a superficial resemblance, when on the wing, to two other bees, viz., *Anthophora retusa* and *A. pilipes*. The males of these two bees are clothed with brownish hairs, which fade to a yellowish grey in weather-beaten specimens, with a considerable mixture of black in certain lights; the females are entirely black

But it is rather in size, and especially in mode of flight, that *V. bombylans* resembles the *Anthophoræ*. In both cases the insects have a dashing impetuous habit, interrupted by periods of hovering. During such rapid movements exact markings fail to catch the eye, and the general impression produced by the two is by no means dissimilar.

It is further maintained by some observers that var. *mystacea* resembles *B. muscorum*. This bee has tawny yellow hairs on the thorax, while on the abdomen the hairs are yellow, more or less mixed with black, or even entirely black. Whether mimicry can be said to exist in this case is a matter of opinion, but it should be borne in mind that both *B. muscorum* and the *mystacea* variety of *V. bombylans* vary in appearance and colour-effect, according to the direction from which they are observed. A freshly-emerged *mystacea*, if viewed from in front, and from a low elevation, would probably be considered by very many persons to resemble *B. muscorum*, while, if viewed either from directly above or from behind, the resemblance would probably be denied. Moreover, a faded specimen of *muscorum* loses much of the tawny colouring and appears of a yellowish grey.

Both varieties of *V. bombylans* have been observed to emerge from nests of the three species of *Bombus* which are chiefly attacked, viz., *B. muscorum*, *B. lapidarius*, and *B. hortorum*. The first-named of these is apparently the most infested. In this connection the habits and demeanour of the bees are of interest. *B. muscorum* constructs a nest on or near the surface of the ground, and is of a comparatively mild and pacific disposition. The other two construct their nests at some depth from the surface, and are, especially *lapidarius*, fiercely irritable. Thus the nest of *muscorum* is clearly more accessible to *Volucella* on account both of the weakness of its strategic position and the faint heart of the defenders.

Be this as it may, it is exceedingly difficult to comprehend how the colour of *Volucella* can in any way impose upon any of the bees, for so soon as the fly has entered the approach to the nest it must be in more or less total darkness, and from that moment the value of mere colour and markings is reduced to *nil*. Any *Bombus* which encountered a *Volucella* in the darkness could not by any possibility discriminate by the sense of sight between friend or foe. It seems far more probable that the hairiness of *V. bombylans* (in both varieties) is the feature which should be spoken of as an example of "aggressive mimicry." It is the sense of touch which alone can enable the *Bombus* to detect *V. bombylans* in the semi-darkness, and surely the hairy body is less likely to arouse suspicion when touched than would a smooth body, for the latter could in no case enter the nest of *Bombus* as belonging to one of the natural members of the colony.

This contention derives much support from the fact that the other three species of *Volucella* mentioned above have hairless bodies and are parasitic upon hairless Hymenoptera. Our knowledge at

present is insufficient for us to be able to determine whether the hairiness of *V. bombylans* is as the "skins of the kids of the goats," or if the Esaus of the genus have practised the converse deception.

If this theory be correct, the colouration of *Volucella* must be removed from the category of "aggressive mimicry"; unless, indeed, the general bee-like appearance disarms suspicion when the *Volucellæ* are flying in the neighbourhood of the nests of the *Bombi*. It seems more probable that the dimorphic mimicry of *V. bombylans* is of "warning" value, and this is far more in accordance with the mimicry occurring in dimorphic forms among other groups of insects. During the past summer I made a few experiments to test this point. Specimens of *V. inanis* and of both varieties of *V. bombylans* were offered to six lizards whose ordinary diet consisted of worms, blue-bottle flies, and caterpillars. In each instance the *Volucellæ* were inspected from a distance and watched attentively, but not one was tasted or even licked. When the *Volucellæ* flew round the cages the lizards exhibited the utmost alarm.

These lizards were brought from Italy in the spring, and had not previously, while in my keeping, been alarmed by any bees or other insects capable of inflicting injury upon them. It was interesting to observe that the wasp-like *V. inanis* was evidently regarded with greater fear than *V. bombylans*. The same dread of any insect banded with black and yellow, *e.g.*, larva of *Euchelia jacobeæ*, was frequently exhibited.

It is evident that as yet we are not in a position to dogmatise concerning the mimicry exhibited by these (and many other) insects. Now that attention has been called to our previous somewhat hasty conclusions, it is to be hoped that the work will be taken in hand by many independent observers during next season. The *Volucellæ* abound in nearly all country places, and are easy to keep under observation in captivity. An interesting point to record, among others, would be the relative proportions of the sexes in the two varieties of *V. bombylans* and the *mystacea* variety. In a series of fourteen taken last summer, consisting of eight *V. bombylans* and six var. *mystacea*, all the former are males, but only one of the latter. Theories to explain mimicry we have in abundance; let us hope for more facts in the future.

OSWALD H. LATTER.

VI.

The Rothschild Museum, Tring.

SO considerable a part of the richness of our public collections in Natural History is due to the interest and munificence of private individuals, that every true student will welcome with gratitude each new recruit in the ranks of those who devote their energies and resources to the amassing of materials for investigation. Of these disinterested workers, who not merely collect but also pursue scientific research, there are happily many still rising in Britain to replace the older generation now quickly passing away: and conspicuous among those in the forefront of progress must be placed the Hon. L. Walter Rothschild, whose interest in the cause of Natural History has of late attracted so much attention. Since his undergraduate days at Cambridge, when he came under the influence of Professor Alfred Newton, Mr. Rothschild has devoted himself with enthusiasm to the pursuit of Ornithology and Entomology; and while following these studies he has not remained satisfied with the resources of the University and National Museums, but has amassed great collections which in some respects rival, and even surpass, those of the British Museum itself. His endeavours have met with so much success, that he has recently erected a commodious museum at the town of Tring, his place of residence, to contain the collections; and he has now added to his private cabinets of Ornithology and Entomology a fine series of typical animals of other groups, arranged for the instruction of the general public, who are admitted on four days of the week. Few naturalists have hitherto visited the museum, the study-cabinets being still incomplete; but, by the courtesy of Mr. Rothschild, we have lately enjoyed the privilege of seeing the arrangements so far as advanced, and it will be of interest to those engaged in research to learn the present position and prospects of the institution.

The Museum and "Cottage," of which we give a side-view in Fig. 1, is a red brick structure, about 100 ft. long by 50 ft. broad. Attached to one of the outer walls is a taxidermist's and general workshop; while, a short distance away, a space of ground, occupied by cages and a paddock, is arranged for the accommodation of living animals, kept for the observation of their habits and life-history. The museum itself consists of one large room, 65 ft. by 32 ft., with a gallery all round, some 15 ft. from the floor. Two large glass cases, about 50 ft. long, occupy the central floor, and the whole of the wall-space, both on the ground floor and in the gallery, is one continuous

glass case of moderate depth. There is, therefore, scarcely a square inch of waste room anywhere. The cases themselves are beautifully fitted with mahogany, somewhat similar to those at the Natural History Branch of the British Museum, though considerably improved. The tops of the central cases are solidly constructed, and by this neat device form, as it were, a central floor to the gallery above, occupied by large specimens too bulky for the glass cases themselves. Additional specimens, chiefly large fishes, are suspended from the roof at a level with the eye of the visitor in the gallery, as shown in Fig. 2.

Entering the museum on the ground floor, one finds the whole of the right-hand portion devoted to birds, and the whole of the left-

FIG. 1.—THE ROTHSCHILD MUSEUM, TRING.

hand half to mammals; while upstairs in the gallery the typical series of sponges, corals, molluscs, fishes, and reptiles are in the wall case, and the insects, with crustaceans, occupy small boxes with loose wooden doors (to exclude light), fixed to the hand-rail of the gallery balustrade, as shown in the foreground of our illustration. On the side galleries these boxes are hinged to permit of their being lowered to hang parallel with the balustrade; such an arrangement not only allowing an uninterrupted view of the specimens placed on the top of the central cases, but permitting the top-light to fall freely on the ground floor.

The whole of the exhibits in the Museum cases, with some exceptions among the mammals and birds, form what Mr. Rothschild

terms his "Public Collection"; and this is regularly open to the inspection of visitors. The private cabinets for study are arranged in the "cottage" at the entrance, and in the "bird rooms" at the opposite end of the museum. The Bird Room on the ground floor is already fitted with cabinets and provisionally arranged. The collection comprises about 30,000 skins of from 5,000 to 6,000 species, and is especially rich in "types." The specimens are preserved, the larger ones in glass-topped drawers, and the smaller ones in light glass-topped drawer-trays, the birds lying in rows in cardboard grooves. This arrangement is found peculiarly convenient, permitting, as it does, the whole of the individuals of a species to be

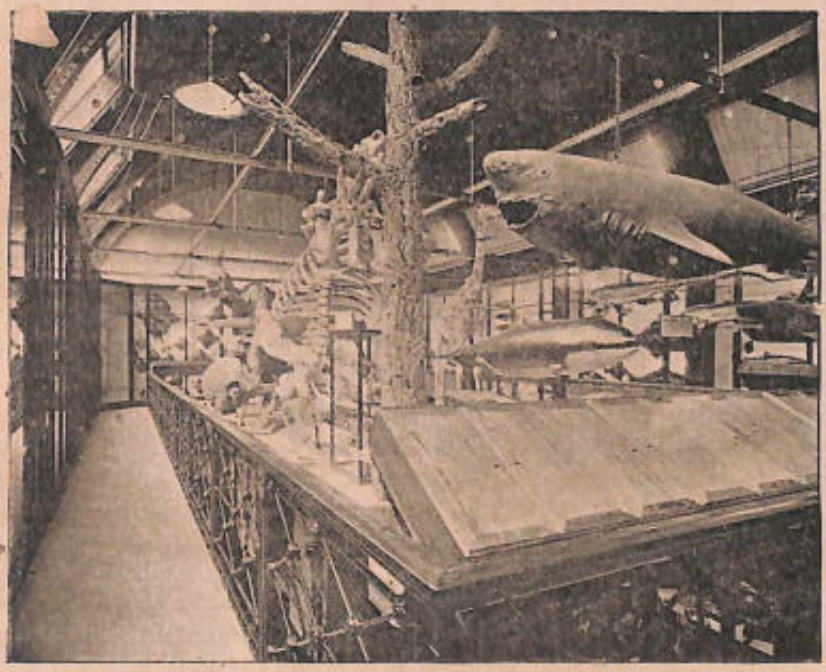

FIG. 2.—THE UPPER PUBLIC GALLERY OF THE ROTHSCHILD MUSEUM, TRING.

lifted out at once and removed in their own tray to the student's work-room and library. The upper bird room is as yet merely a store-room, and above this is another small store-room in the roof, reached by a spiral staircase.

The "cottage" is separated from the Museum by a wide stone staircase, which forms the public means of access to the gallery. It contains Mr. Rothschild's own study, the curators' room, the library, and the entomological cabinets. The library is certainly the most useful and extensive collection of Natural History serials we have seen in a private institution; the special works on Ornithology and Entomology are mostly represented, and are being constantly added

to. The Coleoptera are arranged in glass-topped boxes in bookshelves on the staircase and in the upper room, while the Lepidoptera are in course of arrangement in cabinets, both in the upper and lower rooms, the collection containing altogether about 200,000 Lepidoptera and 300,000 Coleoptera, and especially noteworthy for the large and complete series of most species. All the specimens placed in the public collection are, of course, duplicates.

Passing to a more detailed examination of the public museum, the first striking feature is the wonderfully life-like aspect of the stuffed birds and mammals. This is due to the fact that Mr. Rothschild employs a specialist in each group, and for each kind of work. For instance, all birds of the duck family, ostriches, &c., are stuffed by Doggett, of Cambridge; the eagles in Switzerland; and the same care and discretion are exercised in reference to the mammals. The fishes, also, in the gallery are, many of them, remarkably good, the work for the most part of the museum attendant. Chief among the mammals may be noticed the large (probably the largest known) gibbon, *Hylobates syndactylus*, brought by Dr. Hartert from Sumatra; while close by, on a small board, there sits in most life-like posture the late chimpanzee, "Sally," from the London Zoological Gardens. The lower monkeys are also numerously represented. Among the Carnivora, a particularly interesting specimen is a hybrid between the lion and tiger; and there is a bear's head from Central Siberia of unusual size, the skull being larger than that even of the largest Cave Bear in the Paris Museum, and said by Milne Edwards to be almost identical in characters with the latter. There is a fine example of the Caspian seal, the only specimen in England; and albinos are numerous, the most interesting being one of the sable. Among the unique specimens of Ungulata, we may refer to a fine quagga—an animal so rare that it is unrepresented even in the British Museum—and the head of a rhinoceros from Central Somali-land, with an enormous straight horn, and possibly of a new species. Among smaller mammals, the recently-described opossum-mouse from New Guinea (*Acrobates*) is conspicuous; and there is also an example of the rare squirrel, *Rhinosciurus tupaioides*, from Borneo. The collection of marsupials is very extensive, comprising a fine series of kangaroos, wallabies, and koalas, &c.; while the monotremes comprise the unique *Echidna nigroaculeata*, lately described by Mr. Rothschild, from New Guinea—a very large animal that lived for some time in Mr. Rothschild's possession, and of which he has carefully preserved the skeleton and the soft parts in spirit.

Among birds, the most remarkable series are those of hybrid pheasants, chiefly bred at Tring Park; of the Tetraonidæ (capercaïzies, &c.) from Russia, comprising about 280 specimens; and of the ostriches and other struthious birds. The latter include a fine series of cassowaries, stuffed by Mr. F. Doggett, of Cambridge,

and coloured from life by Mr. Frohawke: the preservation and colouring of the carunculated skin of the neck form one of the greatest triumphs in the taxidermist's art. Young emus, showing the striped character of the early plumage, are also interesting; and to illustrate the development of the struthious birds in the past, Mr. Rothschild has added a model of the skeleton of the New Zealand moa, other moa bones, and a complete egg of the great extinct Madagascar bird, *Æpyornis*. In an adjoining case the display of eagles also deserves special notice.

Passing to the gallery, there are a few striking fossils occupying the walls of the staircase, notably *Ichthyosaurus* and *Steneosaurus* from the Lias, and a large palm-leaf from the Eocene of Monte Bolca, near Verona. A striking object here also is an enormous stuffed python, from Western Africa. The large specimens to be seen from the gallery scarcely require detailed enumeration, the general arrangement being shown in the accompanying Fig. 2. Mr. Rothschild, like a true naturalist, does not regard extinct animals as belonging to a totally distinct domain from those now living; and he thus places, for comparison with the sloths and armadillos, models of the skeletons of *Megatherium* and *Glyptodon*. There is also a noteworthy extinct giant tortoise, *Testudo grandidieri*, from Madagascar, represented only by the shell; the limbs of this animal (it is said) having, by accident, been offered to and purchased by the British Museum. Around the walls are the fishes and reptiles, only a small space being reserved for the other groups, which, as we mentioned above, are confined merely to typical examples. Of the fishes, the best things to be seen are a saw-fish, some sturgeons, some excellent specimens of European species, and a few in spirits. In the hanging boxes on the balustrade the insects are remarkable for their perfection, and here the same rule is carried out, that of showing only typical examples of the various orders.

All these collections are open to the public during certain hours on Mondays, Tuesdays, Wednesdays, and Fridays, and it is gratifying to find that Mr. Rothschild's liberality is appreciated. Since the formal opening early in September, no less than 16,000 visitors have been admitted, and the largest attendance is on the Wednesday evening from 4 to 7, when as many as 300 persons frequently pass the door. The Museum is lighted throughout by electricity, and the rows of small incandescent lamps are most conveniently and admirably arranged.

Referring next to the purely scientific collections, it must suffice merely to enumerate the various series of specimens they comprise. Among birds may be mentioned the following:—

(i.) The Bartlett Collection of weaver birds and finches.

(ii.) The Holdsworth Collection from Ceylon.

(iii.) The Bruijn and Beccari Collection both of birds and mammals from New Guinea, containing many type-specimens of birds described by Count Salvadori.

(iv.) The Whitehead Collection from Palawan and Borneo, with numerous types.
(v.) The Von Hügel Collection from New Zealand.
(vi.) Sir Walter Buller's Collection from New Zealand, containing the types described in the second edition of his classic work.
(vii.) The Palmer and Scott Wilson Collection from the Sandwich Islands and Chatham Island.
(viii.) An extensive collection made by Dr. Ernst Hartert in the Dutch West Indies.

Among insects may be mentioned :—

(i.) The Felder Collection of Lepidoptera and Coleoptera.
(ii.) The Boucard Collection of Coleoptera.
(iii.) Part of the Doherty Collection of Lepidoptera from the Malay Archipelago.
(iv.) The Rev. W. Hamilton's Collection (in part) of Lepidoptera from the Khasia Hills.

Mr. Rothschild, it may be added, is now employing special collectors in the Sandwich Islands and Madagascar, and his series of birds from the first-mentioned islands is already unique.

Enough has now been said to show how vast are the collections already acquired for the Tring Museum, and it only remains to observe how thoroughly Mr. Rothschild is carrying out the undertaking he has begun. Up to the present most of the material has been stored and comparatively inaccessible; but by the appointment of Dr. Ernst Hartert, the well-known ornithologist, as curator, the initial step towards order and arrangement has been taken. The cabinets of bird-skins have already been provisionally arranged; and early next April Dr. Jordan, of Hildesheim, will enter upon his duties as assistant-curator in charge of the entomological collections. The general attendant, caretaker, and taxidermist will also shortly be provided with two additional men. In short, there will be an adequate staff to preserve and render accessible every part of the collections.

It is the custom of certain official naturalists to speak disparagingly of those who, from personal taste, begin to form private collections, afterwards to find themselves deeply engrossed in the subject, and competent to speak with authority on special groups of animals. They declare that private collections are useless, being inaccessible and neglected. We have no sympathy with such expressions in any case; but more especially would we protest against the injustice of those who, even in public meetings, give utterance to these sentiments in reference to the Tring Museum. All the collections are preserved with the greatest care and arranged in the most approved and convenient fashion. As soon as Dr. Hartert and Dr. Jordan have had a brief time for work, everything will be as readily accessible as in the British Museum; and we are permitted to add that, when this arrangement is accomplished,

Mr. Rothschild's collection will not merely be for his own private use and that of his curators, but will be at the disposal of every competent specialist who desires to make use of it for reference. While arranging the collection, Mr. Rothschild and his curators will prepare a catalogue and describe the forms that appear to be new; and the systematic manner in which the collecting is being undertaken in little-explored regions abroad is certain eventually to yield most important results.

In conclusion, we would advise all our readers who are interested in mammals and birds, in the art of taxidermy, or in the arrangement of exhibition cases, to visit the Tring Museum. It is a remarkable and beautiful collection, of interest alike to the wandering amateur and the professed naturalist.

A. Smith Woodward.
C. Davies Sherborn.

SOME NEW BOOKS.

THE VISIBLE UNIVERSE: Chapters on the Origin and Construction of the Heavens. By J. Ellard Gore, F.R.A.S. Demy 8vo. Pp. 346, with 6 stellar photographs and 12 lithographic plates. London: Crosby Lockwood & Son, 1892. Price 16s.

MR. GORE is well known as an enthusiastic observer, an able computer, an authority on variable stars, and the author of successful works which appeal to the general reader and the amateur astronomer. The present volume, like its predecessors, is well written, well printed, and well illustrated. Its main object is to give an account of various hypotheses as to the origin and construction of the heavens, and of the arguments for and against them. In discussing various astronomical fictions, as these hypotheses may be termed, many facts are necessarily described and much interesting information is communicated. Thus, in reviewing the "Meteoritic Hypothesis," the author gives an account of the leading characteristics of stellar spectra and of the recent work by Professor Lockyer, Dr. Huggins, and others, on such subjects as the coincidence of the chief nebular line with the brightest termination of the magnesium fluting.

Mr. Gore has paid special attention to the distribution of stars in space, and this subject is accordingly treated at considerable length. The theories of Wright, Lambert, Herschel, Struve, Proctor, and others, as to the construction of the visible universe and the laws governing the distribution of stars, are described and critically examined. There is a very interesting chapter on stellar parallax and stellar motions in which the latest information on these important subjects is given. Speaking of the sun's motion in space, the author asks, What was the position of our system in past geologic time? If the motion has been rectilinear we must have come from that part of space in which Sirius is at present situated. "Now, with a parallax of 0·39" the distance of Sirius from the earth would be about 49 billions of miles. With a velocity of 14 miles a second the sun's annual motion would be nearly 442 millions of miles. Therefore in 200,000 years the distance traversed would be about 88 billions of miles, which, carried back, would place it out in space far beyond the distance of Sirius." It is thus safe to say that the Ichthyosauri of the Lias must have looked upon a system of stellar distribution very different from that which we now see.

The book appeals to many classes of readers. Those who like to revel in metaphysical subtleties will find portions adapted to their peculiar taste; those who prefer to trace the work of individuals in the present state of our knowledge will turn to the historical parts; while those who feel that such subjects as the above are apt to become tedious, will find plenty to occupy their attention in the descriptive portions of the volume. Too many tastes are catered for to make the book uniformly palatable to any one individual.

Among the illustrations we may select for special commendation those which represent some of the recent results of the application of photography. The magnificent photograph of the nebula in Andromeda, by Mr. Isaac Roberts, forms an appropriate frontispiece.

SHORT STALKS; or, Hunting Camps North, South, East, and West. By E. N. Buxton. 8vo. Pp. vii., 405. Illustrated. London: Stanford, 1892. Price 21s.

FROM the main title, which alone appears on the cover, it might be thought that the volume before us was a treatise on some branch of botany or horticulture, if not a novel, whereas, as a matter of fact, it is a very interesting book on "big game" shooting. It must not however, be thought that it comes merely under the designation of an ordinary book of sport, since the author gives us some very valuable information as to the habits and mode of life of the various animals treated. Moreover, Mr. Buxton has been fortunate in the localities he has selected for many of his hunting trips, whereby he has been brought into contact with animals as to whose habits there has hitherto been a dearth of information in the works of recent English writers. Sporting works on the larger mammals of Southern Africa, India, and North America are so numerous as to afford an almost superabundance of information; whereas, in regard to Eastern Asia, Europe, and North Africa there is a marked dearth of accessible and reliable observations by English sportsmen. We are, therefore, especially glad to welcome Mr. Buxton's accounts of the Sardinian Mouflon (*Ovis musimon*), of the Spanish or Pyrenean Wild Goat (*Capra pyrenaica*), of the Arui (*Ovis tragelaphus*) of North Africa, as well as of the Pasang or Persian Wild Goat (*Capra ægagrus*), and the Chamois (*Rupicapra tragus*). The majority of the twelve chapters into which the work is divided have already appeared as separate articles in various serials and newspapers, but they are now so much embellished by the addition of the excellent illustrations with which the book is adorned, that it is difficult to recognise them as the same. By the courtesy of Mr. Stanford we are enabled to reproduce one of these illustrations as a sample. In addition to these excellent portraits of the animals he describes so graphically, Mr. Buxton also incidentally introduces some charming little sketches of bird-life and scenery.

The first chapter of the book is devoted to the Mouflon, which the author describes as one of the most difficult animals to stalk which he has ever come across. The second deals with the Chamois of the Alps, while the variety inhabiting the Pyrenees is described in the ninth chapter under its Iberian name, Izzard; and here we may point out to the author that he is quite behind modern zoology in referring to this animal under the title of *Antilope rupicapra*. The third chapter is devoted to American game, where some interesting observations are recorded as to the habits of the Bighorn Sheep (*Ovis montana*); while in the fifth Mr. Buxton treats of the Arui and Mountain Gazelle of Algeria. The author appears to have been the first Englishman who has hunted these animals, and the results of his observations have already appeared in the Zoological Society's Proceedings. We are told that the name "Aoudad," so commonly applied to the Arui, is quite unknown in its native land, and the author also speaks as to the remarkable powers of concealment possessed by these animals. The Elk of Norway claims the whole of the fifth chapter; while the sixth (which originally appeared in the *Nineteenth Century*) treats of the Pasang, under the legend of "The Father of all the Goats," in allusion

MALE CHAMOIS (*Rupicapra tragus*).—*From* Buxton's "Short Stalks."

to the probability of its being the ancestor of the domestic breeds. The most interesting of all is, however, the seventh chapter, which treats of the Pyrenean wild goat. Mr. Buxton says that, in contradiction to the true Ibex, this animal largely frequents scrub-jungle, and he points out—we believe for the first time—that the inward inclination of the tips of its horns is evidently adapted to passing with ease among bushes, the true Ibex, which inhabit open country, having the tips of their horns divergent.

Other chapters, relating to Reindeer and Bear, do not call for special notice, and we accordingly conclude by congratulating both author and publisher on the production of a work which, while written professedly for the sportsman, contains much that is deserving of the best attention of the naturalist. R. L.

Our Country's Birds, and How to Know Them. By W. J. Gordon. Crown 8vo. Pp. viii., 152. Illustrated. London: Day & Son, 1892. Price 6s.

Mr. W. J. Gordon is already well known as the author of "Our Country's Flowers," which has attained a well-merited reputation as a handy and convenient guide to the British flora. In the work before us he has treated the birds of Britain in a somewhat similar fashion, the result being a volume which contains, perhaps, more information in a small compass than any other of equal size which has come under our notice. The great feature of Mr. Gordon's work is that every species is illustrated, and that, too, by a coloured figure, and since these figures have been executed by Messrs. Willis and Holding, any comment as to the excellence of their execution would be superfluous. Mr. Gordon, we are glad to see, rather ridicules the idea of including such birds as the Flamingo under the title of British, but as the species has occurred in the British islands, he is perforce compelled to include it in his list.

The great object of the volume is to enable any person to identify any bird he may happen to come across in the British Islands, and for this purpose Mr. Gordon provides us with an elaborately worked-out system of "keys." He is, however, careful to add that this is merely an empirical method, and has nothing to do with classification. Other chapters deal, however, fully with the classificatory portion of the subject, in the course of which the peculiarities of the bones of the skull in the different groups, as well as the subject of pterylosis, are briefly but carefully treated. An unique feature in the book is the series of tables of dimensions of all the British species.

We may point out that in including *Pandion* among the Falconidæ the author is not up to date, we ourselves being persuaded of the correctness of the view that this genus represents an order connecting the Accipitres with the Striges. We cannot, moreover, accept the inclusion of all the Passerines in a single family "Passeridæ."

These, however, are comparatively small defects, and the author is to be congratulated on having given such a large amount of information on British birds in such an exceedingly small compass.

R. L.

The Building of the British Isles: A Study in Geographical Evolution. By A. J. Jukes-Browne, B.A., F.G.S. Second edition. Pp. xiv., 465. 8vo. London: George Bell & Sons, 1892. Price 7s. 6d.

One object of geological enquiry is to picture the physical conditions that attended the deposition of the many geological formations. In its most interesting aspect, the subject is inseparably connected with

the natural history of successive periods; but from this broad point of view all sorts and conditions of life and of land and water would have to be considered.

In the present work the author endeavours to picture the changes in the distribution of land and water, over the area now occupied by the British Islands, during each of the great epochs of time which make up our geological sequence. His task is a most difficult one, for few geologists have been tempted to draw boundary-lines to mark the former limits of formations, however well they may be acquainted with them. It is one thing to picture in the imagination former general geological conditions; it is quite another thing to draw the outlines of land and water and to fill in conjectural rivers and lakes. The subject, like discussions on theology, is apt to lead to almost endless diversity of opinion, for (as the author admits) the materials for forming a definite judgment are so imperfect.

That his work is appreciated, is shown by the issue of a second edition, in which the author has added over a hundred pages of further information. Altogether there are fifteen plates drawn to show the geography of different epochs. Three of these are new, six remain the same as in the first edition, and six have been subject to alteration. Several other small maps and sections are printed with the text. One of the principal alterations is in the map of the Ordovician period (Arenig Epoch); we notice also that a small lake is introduced into the Devonshire and Cornish area, in the map that represents the Permian period.

Whatever value the maps may possess, there can be no doubt that the author has brought together, in a most painstaking way, a large amount of material bearing on the physical geography of our bygone ages. He concludes with a chapter on the Supposed Permanence of Continents and Oceans, and with an appendix on the Sub-oceanic Crust.

LES ALPES FRANÇAIS, LES MONTAGNES, LES EAUX, LES GLACIERS, LES PHÉNOMÈNES DE L'ATMOSPHÈRE. [Bibliothèque Scientifique Contemporaine.] By Albert Falsan. Pp. 288. Illustrated. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

IN a little volume of 280 pages, Monsieur Falsan has given a readable account of the French Alps, their geology, physical geography, and meteorology. The author's extensive experience and well-known writings will attest his competence to speak on the subject, and we need only add that the condensation from larger works has been carefully and judiciously made. The nature of Monsieur Falsan's special studies leads to marked prominence being given to the glaciers and ancient glacial phenomena, and to the Tertiary geology of the Alps. The chapters on these subjects are particularly good, and to them the geologist will naturally turn.

A CATALOGUE OF BRITISH JURASSIC GASTEROPODA. By W. H. Hudleston, F.R.S., and Edward Wilson, F.G.S. Pp. xxxiv., 148. London: Dulau and Co., 1892. Price 7s. 6d.

THIS work, although it embraces a comparatively small section of our British Fossils, is a most important addition to that series which we hope will eventually replace the famous Morris' Catalogue. Nearly forty years have elapsed since the second edition of Morris' work was published. In that volume only five species of Gasteropoda are

quoted from the Lias; now 319 species are enumerated. Altogether, the authors of the present Catalogue record 1,015 species, and they have reduced the lists by something like 150 names of doubtfully identified foreign or imperfectly-constituted British species.

This new volume is the second contribution to Catalogues of British Fossils, Messrs. Woodward and Sherborn's Vertebrata appearing in 1890. We constantly use the old classic now, and considering the vast amount of work that has been done in the past forty years, it will readily be understood how useful must be a recent book of a similar kind that fully arranges our present knowledge. It is gratifying to find that the labour has so far been undertaken by those specially qualified to do it.

We should like, however, to make some remarks on a few points which might, in our opinion, have been more carefully studied. In the first place, the authors do not distinguish between James Sowerby and James de Carle Sowerby, a careless practice of some English palæontologists which has several times been pointed out to us by continental workers. Secondly, there is a great inconsistency in giving dates; sometimes a separate work is quoted with and sometimes without date, and the same may be said of serials. Considering the authors do not attempt exact dating in their bibliography, the reader is left hopelessly in the lurch, and has after all to seek his reference for himself in some public library.

We are glad to see that when quoting pre-Linnean genera, the first Linnean authority is also given, though from the method of quotation one is almost led to believe that the pre-Linnean author's name is accepted. The entry *Cerithium*, for instance, would be more accurately expressed thus: *Cerithium*, Bruguière, 1792 (*ex* Adanson, 1757). It is also gratifying to note that the 10th and not the 12th edition of Linnæus' "Systema" is used.

With regard to the generic determination of some of the mollusca, we think that a more detailed study of recent forms and recent continental work upon them would have enabled the authors to more exactly arrange the various Cerithiform, Naticiform, and Trochiform groups; and although the authors have some remarks (pp. 18—23) on several genera, we are loth to believe that all the "species" quoted under *Cerithium*, *Chemnitzia*, *Natica*, *Trochus* and *Turbo* properly belong to these genera. We are especially dubious as regards the first, third, and fourth genus, in which the characteristic features can only be seen in perfectly-preserved or completely disentombed specimens.

The sundry new names used in the volume might have been listed on p. xxxiv., now a blank; as it is we are told to "find six faces" in the style of the puzzle-pictures.

The general get-up is good, but the paper is too thick and clumsy, and the book takes up one inch, instead of half-an-inch, of already seriously overcrowded shelves.

Fossil Plants as Tests of Climate. By A. C. Seward, M.A., F.G.S. [Sedgwick Prize Essay for 1892.] Pp. 151. London: C. J. Clay & Sons, 1892. Price 5s.

This Sedgwick Prize Essay is a useful compilation, the author having brought together all the botanical evidence of former climatic conditions. Most of the opinions quoted are, however, extremely speculative, and of little value, and, wisely, Mr. Seward seldom ventures an opinion of his own on so difficult a subject as bygone climatic changes.

As far back as plants are known, even in the problematical *Nematophycus* of the Silurian, rings of growth are visible in certain orders, but whether these are true "annual rings" we have no means of telling. When we study the evidence derived from fossil floras found within the Arctic regions, we are on safer ground, for the Devonian, Carboniferous, Jurassic, Cretaceous, and Lower Tertiary plants could not possibly have lived there under present climatic conditions. Still clearer is the evidence when we come to more recent times, and find alternating deposits containing northern and southern plants of existing species.

LES LICHENS. By A. Acloque. [Bibliothèque Scientifique Contemporaine.] Pp. viii., 376. Illustrated. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

THE French people are happy in the possession of a popular scientific literature of excellent quality and moderate price, of which they are justly to be envied. They produce books now and then, however, of which no nation envies them, and this is one. In the matter of bad popular books on fungi and lichens this country might be thoughtlessly considered to hold the record, but it is refreshing to know that while our neighbours can beat us easily in serious research on these subjects, they can also show us the way in the matter of poor productions. It is almost enough to say of this little popular book on the study of lichens that its benighted author rejects Schwendener's great discovery of the dual nature of lichens, and the symbiotic phenomena they illustrate. It is hard to do more than prove a matter, and after the establishment of this great fact by the researches of De Bary, Schwendener, Stahl, Bornet, and others, it was confidently expected that the old lichenological school would die out. It is dying, however, like a vested interest, and the patent rights in "homæogonidisme" have been leased afresh by M. Acloque. His little book shows a most superficial knowledge of morphology in general, and the text is not much better than the figures, which are all that is not to be desired.

NAKED-EYE BOTANY. By F. E. Kitchener, M.A., F.L.S. [Beginners' Text-Books of Science.] Small 8vo. Pp. xii. and 182. London: Percival & Co., 1892. Price 2s. 6d. net.

THE series of elementary science text-books of which this is the first, has been projected to meet the want, long-felt, of something which will cover the ground of a few terms' lectures and serve the pupils as a supplement to those lectures. The volume comprises 48 lessons, which may "be got up by the pupil by himself as prepared work," or "taken in school unprepared with the help of a running commentary from the master in the form of a catechetical lecture." These two objects are widely different, and demand very different treatment; with the help of a "running commentary" from a competent master the work before us may be of some use, but it is not one to put into the hands of a beginner to "get up" alone. As lecture-skeletons on various types of flowers, the book may be a help to busy teachers, but they must be careful to explain crude statements and correct erroneous ones, such, for instance, as "the leaf-stalk may be traced as carried on through the leaf to its apex, and sends out branches (or veins) on each side arranged like a feather," the definition

of stipules as "two small leaves at the base of the leaf-stalk," or the description of the position of the leaves of the iris "at the root (radical)." Knowing the very intimate connection between respiration and life, whether of plant or animal, it is surprising to find this important function of the whole plant disposed of in a few lines thus: "In the absorption of oxygen from the air, it is not so much the foliage leaves that take part as the leaves that are not green, the parts of the plant that are in flower; these plants perform, when not in the sunlight, a process akin to that which all animals perform, viz., they take in oxygen gas dissolved in water, and send out the oxygen combined with carbon under the form of carbon dioxide dissolved in water."

We know that respiration is more vigorous in opening flowers and germinating seeds, but this is only because life, expressed as more rapid growth and demanding a co-ordinated supply of energy, is "faster"; just as a man breathes harder with increased effort.

It is only fair to state that these faults preponderate in the earlier lessons; where the author confines himself to descriptions of plants he is more at home, and many of the lessons, *e.g.*, that on the Rose-family, are good.

In spite of the title "Naked Eye Botany," the pupil, as stated in the preface, will find a lens useful, and often indispensable, in the floral dissections.

At the end of each lesson problems are presented to be done out of school. Many are good and suggestive, some hopelessly beyond the pupil. At the end of the first lesson, one on the chickweed, he is asked whether "animals move about and plants are fixed in the ground" is an invariable rule, and invited to mention exceptions; several relating to insect visits and fertilisation are introduced with hardly any previous instruction on the subject.

In the table for determining the natural orders of British flowering plants, no notice is taken of exceptions, so that *Clematis*, *Anemone* and other apetalous Polypetalæ would be sought for among the Apetalæ. We notice, too, that both the goose-foot and polygonum family are styled Polygonaceæ.

The book is no worse than many of its kind; better than some. It illustrates for the hundredth time the fact that it takes the best men in a subject to write a good elementary text-book.

Systematic botanists, and those who make a hobby of the Iris family, will welcome the appearance of Mr. J. G. Baker's monograph on Irideæ (George Bell & Sons). It is uniform with the author's works on the Fern allies, the Amaryllidaceæ and Bromeliaceæ. Mr. Baker speaks of it as the last of the series, but we hope this will not be the case; the Scitamineæ want working up again, for Horaninow's monograph might well give way to a more modern one, and if Mr. Baker took it up we should be sure of having it well done.

Professor Baillon's *Histoire des plantes* (Paris: Hachette & Co.) has reached the twelfth volume, the first part of which, just to hand, includes the Conifers and other Gymnosperms, with the monocotyledonous orders, Alismaceæ, Najadaceæ, Typhaceæ, the curious little tropical Triurideæ, and the equally strange, almost exclusive, Australian Centrolepideæ. The excellence of the previous volumes

is maintained both in the text and illustrations. We notice that *Casuarina* now figures as a tribe of Conifers, though with a query, and the remark that wherever placed they constitute a distinct group, which has been compared on as good grounds with the Cryptogams. We regret that the author has followed the change of nomenclature recently introduced by Kuntze, which substitutes *Podocarpus*, the accepted name of a large and well-known genus, for the equally well-known, but very different, *Phyllocladus*, makes the *Sequoia* an *Arthrotaxis*, and for *Welwitschia* writes *Tumboa*.

DR. P. L. SCLATER has issued a privately-printed *New List of Chilian Birds*, compiled by the late Harry Berkeley James. Some time ago Messrs. Sclater and James agreed to co-operate in the preparation of a work on the Birds of Chili, as a companion volume to Sclater and Hudson's *Argentine Ornithology*. The lamented death of Mr. James last July arrested progress, and his preliminary list of birds, compiled as a basis for the work, is now published by Dr. Sclater, and prefaced by a short biographical notice.

MESSRS. GEORGE PHILIP & SON'S *Child-Life Almanac* (price 1s.) will interest not only children, but nature-lovers of all ages. The aim is to provide kindergarten teachers with suggestions for lessons and observations. The recurring facts of natural history, and the principal events in the social, political, and intellectual life of the country, will serve as pegs on which to hang instruction. The idea is well carried out. The twelve sheets, each 11 × 8 inches, record not only a goodly number of events of common or special interest, but also in a parallel column, a list of phenological observations, the times of appearing of flowers, birds, and insects, of the singing or departure of the birds, the leafing or leaf-fall of our common trees, and many other facts which should be of interest. Hints are also given as to out-of-door work available for the time of year, and general observations on pond-life, moth-hunting, bees, or gardening.

Finally, two prizes of a guinea, one for adults and one for children, are offered for the most complete set of phenological notes for 1893, prepared from actual observation, confirming or supplementing those of the almanac. We hope Messrs. Philip will receive a goodly number of sets. Efforts to increase the interest in the study of Nature deserve support. The almanac is very neatly arranged and printed in a somewhat effective cover.

OBITUARY.

HENRY TIBBETS STAINTON.

Born August 13, 1822. Died December 2, 1892.

WE regret to record the death of this eminent entomologist, an acknowledged leader in the science at home and abroad. Mr. Stainton was one of the highest authorities on the Lepidoptera, especially on the smaller moths, and most particularly on the Tineina, a group in knowledge of which he had been, of late years, without a rival. Like most naturalists, Mr. Stainton had a taste for his subject from his early youth, as he began to collect insects when only twelve years old. He continued faithful to the Lepidoptera throughout his life; and though the groups to which he mainly devoted his attention do not attract the ordinary amateur, their study is of great importance, and the life-histories of many of the Tineina are of surpassing interest. In the study of these minute moths, Mr. Stainton was associated with the great German lepidopterist, Zeller. By their labours the classification of the Microlepidoptera was brought into a natural sequence, and the genera established on good structural characters, a consummation still to be wished for in some of the groups of larger moths. The *Natural History of the Tineina* in four languages was the joint work of Stainton, Zeller, Douglas, and Frey, and appeared in thirteen volumes between 1855 and 1873. Mr. Stainton had published a work on *British Tineina* in 1854; in 1857 he published the *Tineina of Syria and Asia Minor*, and, in 1869, the *Tineina of Southern Europe*. In numerous papers in the entomological journals he elucidated the life-histories of the smaller British moths, and made many additions to our fauna. But his general work on the *British Lepidoptera*, published in two volumes (1857–9), shows his acquaintance with the order as a whole, and is still the most useful book on the subject for the working entomologist; though being mostly composed of synoptical tables and technical descriptions, with numerous abbreviations, it is not attractive to the general reader.

Mr. Stainton will be remembered for his activity in the field of entomological journalism. For twenty years, from 1855 to 1875, he issued his *Entomologists' Annual*; and for ten years, from 1856 to 1866, he conducted the *Entomologists' Weekly Intelligencer*, the only British journal on the subject that ever appeared weekly. From 1864 till his death, he was on the editorial staff of the *Entomologists' Monthly*

Magazine, and until last year he was a frequent contributor to its pages. He was a member of many learned societies, and was elected F.R.S., a rare honour for a systematic zoologist, in 1867. Courteous to all who applied to him for information, personally or by letter, and always ready to place his great knowledge and extensive collections at the service of students, he leaves an example of the best traditions of English naturalists.

AMONG other recent deaths we also have to chronicle those of FLAXMAN SPURRELL, of Crayford, an ardent botanist, who also began the great collection of Pleistocene Mammalia from the Thames Valley, now long utilised and extended by his son, Mr. F. C. J. Spurrell; of W. MATTIEU WILLIAMS, the well-known author of many popular treatises and magazine articles; of the veteran conchologist, P. M. A. MORELET; of two cryptogamic botanists, Baron FELIX VON THÜMEN and C. M. GOTTSCHE, both distinguished for their labours among mosses and hepaticæ; and of ALEXANDER SKOFITZ, founder and editor of the Austrian *Botanische Zeitschrift*.

THE death is announced of the veteran American Professor of Geology, Dr. JOHN STRONG NEWBERRY. We hope to give some account of his life and work next month.

THE death of Sir Richard Owen, K.C.B., on December 18, 1892, is noticed on p. 17.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. G. von Lagerheim, late Director of the Botanic Garden of Quito, has been appointed Curator of the Tromsö Museum, Norway.

Messrs. W. T. Swingle and W. J. Webber have been appointed to direct the newly-founded "Station for Citrous Diseases" at Eustis, Florida.

Mr. E. W. MacBride, M.A., has been appointed Demonstrator in Animal Morphology in the University of Cambridge.

Dr. C. S. Minot has been appointed Professor of Histology and Embryology in Harvard Medical College; and Dr. Harris H. Wilder has been elected to the Chair of Biology in Smith College, Northampton, Mass.

Mr. Joseph E. Carne, late Curator of the Mining and Geological Museum, Sydney, has been appointed a Geological Surveyor in the Department of Mines, New South Wales.

Mr. Edgar R. Waite, for some years Curator of the Leeds Philosophical Society's Museum, and joint editor of *The Naturalist*, has been appointed assistant-curator of the Australian Museum, Sydney. Mr. Waite leaves England early in February.

Dr. F. Elfving has been appointed Professor of Botany in the University of Helsingfors, Finland; and Professor W. R. Dudley has left Cornell University for the Leland Stanford Junior University, Palo Alto, California. Mr. G. F. Atkinson now becomes Assistant Professor of Cryptogamic Botany at Cornell.

Mr. Francis Darwin, Reader in Botany at the University of Cambridge, has been appointed Deputy-Professor of Botany, owing to the ill-health of Professor Babington. Professor Babington has held the post for more than thirty years, and for some time past has taken no active part in the management of the School of Botany, which was so well worked up by Dr. Vines, now Professor at Oxford. Mr. Francis Darwin is well known, both from association with his father in much of the great naturalist's botanical work, especially the "Movement of Plants," and from his own researches in the physiology and biology of plants.

The foundation stone of an extension of the buildings of the Durham College of Science, Newcastle-on-Tyne, was laid by the Earl of Durham last month.

UNDER the direction of Professor Osborn, a series of lectures on the results of current researches in Biology is being delivered this session at the Columbia College, New York. The course is arranged to meet the requirements of advanced students and biologists engaged in research.

ACCORDING to the *American Naturalist* the new Natural History Institute of the University of Illinois, at Champaign, was opened on November 16 last. The cost of the building was about 78,000 dollars, and it will contain the Natural History library (of 20,000 volumes), museum, laboratories, and lecture rooms.

THE Town Council of Ipswich has decided to open the Museum and Public Library on Sunday afternoons.

WE are glad to learn that the Yorkshire Philosophical Society has reduced the admission fee for the York Museum on Saturday afternoons to the small sum of one penny. This generous concession to the public is much appreciated.

THE shareholders of the Bristol Museum and Library have now confirmed their resolution, to offer their property as a gift to the City of Bristol. The discussion of the subject seems to have been chiefly confined to the library, but we hope that some naturalist of influence will be found to watch the interests of the museum. Fortunately, the valuable collections are safeguarded by the endowment fund, which necessitates the interference of the trustees of the British Museum, and their sanction to whatever arrangements may be made.

A FUND for the benefit of the widow and daughter of the late Mr. T. J. Moore, formerly curator of the Liverpool Museum, is being collected by a local committee, of which the Rev. H. H. Higgins is treasurer, and Mr. Richard Paden secretary. Contributions addressed to either of these gentlemen at the Liverpool Museum will be gladly received.

THE plans of the Sedgwick Geological Museum were under discussion at Cambridge last month, and there now seems to be a prospect of the early realisation of the long-projected scheme for a new Memorial Museum. The staff of the Geological Department of the University is also to be increased by the addition of a Demonstrator in Palæozoology.

A HANDBOOK for the Department of Geology in the United States National Museum is in course of publication. Part i., by Mr. George P. Merrill, deals with the Materials of the Earth's Crust, and forms not only a useful guide to the Rock-collection, but also a concise introduction to Petrology. It is illustrated with photographic plates that clearly show various kinds of rock-structure, as seen both in mass and under the microscope.

THE Right Hon. John Morley was elected a Fellow of the Royal Society of London on December 15.

THE Royal Geographical Society's monthly Proceedings appear this month and henceforth under the title of *The Geographical Journal*.

THE section of Medicine and Surgery of the French Academy of Sciences some time ago formed a committee to arrange for an address of congratulation to be

presented to M. Pasteur on the occasion of the seventieth anniversary of his birthday. Subscriptions were received, and a gold medal accompanied the address that was presented to the veteran pathologist on December 27.

THE 150th anniversary of the foundation of the American Philosophical Society will be celebrated at Philadelphia from May 22 to May 26, 1893.

WHEN is the Catalogue of the Linnean Society's Library destined to appear? It has been promised to the Fellows for several years, and so far there has been no fulfilment. The Linnean Society's Library is so useful to a large body of workers, and the courtesy and helpfulness of the officers and staff are so abundant, that there has been hitherto no public grumbling on the subject. It is to be hoped that an energetic effort will soon be made to bring out the Catalogue, since its absence considerably impairs the efficiency of this branch of the Society's work.

THE Yorkshire Naturalists' Union has issued as the seventeenth part of its Transactions another instalment of Mr. J. G. Baker's "North Yorkshire." This is the fifth part of the work, and contains a list of the Flora from *Pyrola* to *Sesleria*. The next part will complete the flowering plants, and commence the mosses and hepatics.

THE North Staffordshire Naturalists' Field Club has issued vol. xxvi. of its *Transactions* (1892). The Garner Memorial Medal has now been established, and will be awarded in alternate years for original researches in the Society's district. The Council regrets that funds do not allow of an annual award. The medal will be of silver, or, at the option of the recipient, it may be substituted by a bronze medal and a gift of books.

THE last part of the *Proceedings of the Yorkshire Geological and Polytechnic Society* (vol. xii., pt. ii., 1892) contains an important illustrated account of the exploration of a tumulus at Howe Hill, Duggleby, by J. R. Mortimer. Other papers relating to the county are a description of the geology of Grassington and Wensleydale by J. R. Dakyns, notes on Flamborough Drifts by G. W. Lamplugh, and a summary of the Lias and Oolite formations by James W. Davis, the editor. Mr. G. R. Vine has a detailed memoir on some Cretaceous Polyzoa, and Dr. Tempest Anderson contributes some personal observations on the volcanoes of Iceland.

THE Palæontographical Society is to be congratulated on having issued its volume of monographs for 1892 within the year to which the subscription relates. The volume contains the final part of Professor Nicholson's monograph on Stromatoporoids, and further instalments of the monographs of Jones and Woodward (Palæozoic Phyllopoda), Hudleston (Jurassic Gasteropoda), Buckman (Lower Oolite Ammonites), and Whidborne (Devonian Fauna of S. England). The plates and illustrations are of the usual high standard, but there is a marked preponderance of illustrations of ammonites; the Society's stock of plates of these fossils long in hand must by this time be almost exhausted. We have on a former occasion referred to the misleading list of "announcements," and regret to find that it still remains unrevised. In the case of several of these monographs announced as "in preparation," we believe the statement is false, and that members of Council are aware of the fact; indeed, the Treasurer of the Society (Mr. R. Etheridge, F.R.S.) authorises us to state that he is *not* preparing the two memoirs placed under his name, and has no intention of doing so. The length of the list of subscribers is gratifying, but the false promises made by the Council are not calculated to retain this support, as soon as the true state of affairs is discovered.

CORRESPONDENCE.

Experimental Evolution.

In the last number or Natural Science, P. C. M. delivers some criticism o my lectures on Experimental Evolution. He is quite right in criticising me when he says that "While royal food may ripen the latent sex of a worker (*ex* bees) a different factor has to do with the distinction between male and female." I should have said that *sexuality*, *i.e.*, the development of sex, or the possession of sex, may be determined by food; *sex* is not determined thus, as while unfertilised eggs develop only into drones, fertilised eggs, according to circumstances—and food—develop into queens *or* workers.

But when P. C M. says, concerning the statement that "we possess in the facts of domestication and inheritance a large number of cases of variation, which occurs in every part, due to environment, and transmitted in various degrees," that "if the statement were true there would be no controversy," this is a rather sweeping criticism. There are five distinct propositions in this statement. Does P. C. M. object to all, or only to part of them? Does he deny variation due to cultivation and domestication? does he deny the occurrence of variation in every part of organisms? does he deny only the fact that variation is due to environment? or does he object to its hereditary transmission? I grant that, in ascribing variation to environment only, I would be saying more than I am prepared to assert, as, in fact, the cause or causes of variation are unknown; but environment in its broad sense seems to have something to do with variation, as many facts go to show. P. C. M. may certainly object to part of my statement, but it would be fair, if only towards his readers, if he stated which part he considers as most heretical.

The same critic informs me that Professor Le Conte's classification of the factors of evolution is "curiously inept," and charges me with approval thereof, which surely cannot improve my situation. However, I must decidedly object to a method (?) of criticism (??) which consists in quoting part of a statement and leaving out the remainder. I have merely given Le Conte's list of "what the factors of evolution are, or are supposed to be" (p. 229: there is not much approval in this), and if P. C. M. had taken the trouble to go on with the quotation, and to quote what I say of this list (which, in fact, is a mere enumeration of the principal factors of evolution proposed on different sides, a mere catalogue), he would have seen that I state that "the five factors are not all recognised by the same group of evolutionists," the two first being especially Lamarckian, and that, in my opinion, no definite view concerning the real value of these different factors can be entertained as long as all have not been subjected to the test of experiment.

If I have "approved" of the list, I do not see that I can be considered as having approved of the scientific soundness of the views expressed in it, inasmuch as no one does adhere simultaneously to each of the five views expressed, and I should be the last to do so, since I ask for proofs of *each* and *all*.

P. C. M. ends with a very virtuous and mighty sentence, which, abridged, might be of high moral use in a copy-book; but I fail to see how it applies to myself. I have not the slightest wish to confound anybody, nor am I familiar with the devices of political controversy—which, I perceive, are exceedingly wicked—and while advocating, from the first to the last page, the necessity of submitting theories to the test of experiment, in order to ascertain the exact situation of that swift-footed fugitive, truth, I certainly did not expect to be charged with resorting to methods which are not only unscientific, but, strictly speaking, unfair.

Paris, *November*, 1892. Henry de Varigny.

[DR. DE VARIGNY brings forward the statement that "we possess, in the facts of domestication," &c., in support of his view that Professor Weismann goes too far when he asserts that we have no proof of the direct production of transmissible changes by means of external influences. As used, the sentence amounts to a single statement that we know many cases of variations due to environment being transmitted.

The whole controversy to which Dr. de Varigny is alluding, concerns the statement which he brings up as an argument on one side of the controversy.

In the matter of Le Conte, I am sorry that I attributed to Dr. de Varigny an approval he now disowns. He wrote (p. 229):—

"What can the methods of experimental transformism be? The only answer to this question is based on the consideration of what the factors of evolution are, or are supposed to be. At the present moment five are usually recognised. I quote from Le Conte's able paper of recent date."

In the two "factors" I quoted in my review, Le Conte runs together the observed facts of phylogenetic variation and phylogenetic decay of disused parts, with the theory that the result of the action of environment is inherited and integrated. A chief object of "experimental transformism" must be to decide on these theories, and any "method" involving preliminary acceptance or rejection of them is valueless.

I do not consider Dr. de Varigny's statements heretical, but if I did it would be of no interest to anyone. I merely showed that they were confused.

P. C. M.]

THE INTERIOR OF THE EARTH.

IN the summary of my late paper in the *Proc. Cambridge Phil. Soc.*, given in "Notes and Comments" of December, there are two points to which I wish to be permitted to refer. It is stated that I have "reinvestigated mathematically" the effects of a tidal yielding of the earth on a tide of short period, according to the canal theory. Professor Darwin had already made that investigation, and there was, consequently, no need to do it over again. But he had left his result in the form of general symbols, and what I have done is to carry his calculation a step further by substituting for the symbols their known astronomical values. It was in this way that I arrived at a conclusion to what extent the ocean tide might be expected to be diminished if the interior of the earth is liquid.

The Reviewer has, however, given my result incorrectly in saying that I have found that the tide, in the case of a liquid interior, "would be about two-fifths of what it would be" in the case of the earth being solid. What I did say was, that it would be *diminished* by two-fifths if the earth was taken as homogeneous, but only by one-fifth when the fact is taken into account that the outer parts of the earth are of half the mean density. This, of course, will leave four-fifths of the height on a solid earth for the height on a liquid earth. For instance, if the height of the tide from highest to lowest on a solid earth were fifty inches, it would still be forty inches if the interior were liquid. Since we do not know what the exact height of the tide would be on a solid earth, it is perfectly possible that the tides actually experienced may be of the height appropriate to a liquid interior, seeing the diminution caused by liquidity to be so small.

On a review of the whole question, it appears to me that what had been previously done by Lord Kelvin and Professor Darwin, and other mathematicians, had been to show that, unless the earth is excessively rigid, the tidal forces must deform it. But it had been *assumed* that, if so deformed, it would carry the water up and down with it, so that the ocean tides would not be noticeable; but the question whether or not this assumption was valid, had never, so far as I know, been brought to the test of numbers until I made the attempt.

O. FISHER.

Harlton, Cambridge, *December* 12, 1892.

THE WALK OF ARTHROPODS.

I WOULD like to supplement my paper on the "Walk of Arthropods" (NAT. SCI., vol. i., p. 676) by calling attention to Mr. Dixon's description of his researches, in *Nature*, vol. xlvii., p. 56, in which some additional details are given, not included in the paper reviewed by me. The spider *(Tarentula)* when running on water caused depressions of the surface-film which were projected as dark shadows on the bottom of the vessel. No shadows were cast by the foremost pair of legs, which proves that the spider, like the insects, has a triangular base of support. The walk of centipedes is also described; the legs are said to move in alternate sets of three.

In referring to investigators of the walk of insects, I regret that I omitted to refer to Professors Miall and Denny, who in their book on the cockroach (p. 79) give a short but excellent account of the locomotion of insects.

Dublin, *December*, 1892. G. H. CARPENTER.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, 67–69 *Chancery Lane, London, W.C.*

The PUBLISHERS *require a few copies of the October number (no. 8) of* NATURAL SCIENCE. *Full price, with postage, will be paid for clean copies not folded, addressed to* MACMILLAN & CO., 29 *Bedford Street, Strand, London, W.C., until the demand is supplied.*

PROFESSOR W. A. HERDMAN (University College, Liverpool) contemplates the preparation of a Handbook to the British Marine Fauna, and desires to receive suggestions from naturalists interested in the subject. He will also be glad to hear from specialists who are willing to co-operate with him in the compilation. The Professor proposes that the work should be a "pocket" or "seaside" handbook, which could be used in much the same way as the botanists' "Field Flora." It might comprise four to six small volumes, each dealing with one or two of the large groups. There would be definitions—perhaps with occasional analytical tables or keys—of orders, families, &c., down to and including genera. Abbreviated descriptions of species would be added, with an indication of size, range, and habitat. Simple figures would be given of all structural features of diagnostic value in the various genera and species.

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NOTES AND COMMENTS.

The Age of the Earth.

AMONG the wider problems of Natural Science towards the solution of which contributions have been made during last month, the most striking is that of the Age of the Earth. Mr. Clarence King, the well-known American geologist and explorer, contributes an elaborate article on the subject to the *American Journal of Science* (ser. 3, vol. xlv., pp. 1–20, pls. i., ii.), in which he claims to have advanced Lord Kelvin's method of determining the earth's age to a further order of importance. He discusses the experimental investigations of Dr. Carl Barus on the effect of heat and pressure on certain rocks, and particularly selects the case of diabase, which has a specific gravity approximately equal to the average specific gravity of the earth's crust. In the light of the new facts, he then reconsiders the probable rate of cooling of the earth, rendering more precise the conclusions of Lord Kelvin, which were arrived at on more imperfect data so long ago as 1862. As the result of the detailed discussion, Mr. King concludes that the earth's age probably does not exceed twenty-four millions of years—in fact, that the estimate of the physicists is approximately correct, while that of the geologists is "vaguely vast" and unreasonable.

We have already referred on a former occasion (vol. i., p. 487) to Professor John Young's observations on the possible sources of error in the geological and biological estimates of past time. We feel convinced there is no sure chronometer beyond the realms of physics and astronomy, and even in those spheres the mathematicians begin with so many assumptions that we are often inclined to look with scepticism on the results. It is, however, satisfactory to learn that those who approach the subject from the physical standpoint

are still pursuing experimental investigations, and if the same conclusions can be arrived at by a multiplicity of methods, biologists and geologists will be constrained to acquiesce.

THE INFLUENCE ON DEVELOPMENT OF CHEMICAL CHANGES IN THE SURROUNDING FLUID.

CURT HERBST, of Zurich, in the *Zeitschrift für Wissenschaftliche Zoologie* (vol. lv., p. 456), has published the results of an interesting series of experiments on the eggs and larvæ of Echinoids. He concludes that sudden changes in the chemical composition of the sea-water produce great changes in the larvæ. But these changes are due, not to the chemical nature of the introduced material, but to changed physical conditions—chiefly to changes of the osmotic action between the water and the larvæ. He hopes that such experiments tend towards an understanding of normal development, and indicate paths towards the remote goal of a casual interpretation of life-history. By changing the chemical conditions, he hoped to find whether the form of a larva at all depended on the composition of the medium in which it grew. His first result is against a chemical, in favour of a physical influence. The form of the larva varies with osmosis within and without the larva. Clearly Curt Herbst—like so many other zoologists—has not abandoned hope of resolving vital actions into their chemical and physical elements.

VITALISM.

AT the present time, indeed, there is abundant evidence of a fundamental change in the attitude of biologists. Among zoologists, one often hears it said that morphology is played out. By this it is apparently meant that the obvious problems of microscopic anatomy are done with; that series of sections have been made of all the more familiar animals and organs, and that three weeks at the seaside with borax-carmine and a rocker microtome, no longer result in an epoch-making paper, containing a hypothetical ancestor, and a brand new pedigree. This is, no doubt, very depressing; but there are still problems in abundance to solve, and we are far from regarding the microtome as obsolete. On the other hand, it is undoubtedly the case that the pristine enthusiasm for microscopic anatomy resulted in a neglect of many other sides of zoology. Examine the scientific journals of from four to twenty years ago, and you shall find hardly a word of that side of biology Professor Lankester has called Bionomics. At the present time, in almost every journal one sees studies on variation, experimental work on development, investigations of living things as alive.

It is, however, among physiologists that the revival of this side of biology is most apparent, and it is among physiologists that the

word "Vitalism" has been born. Some time ago, we were all taught to regard the human body as a machine. In digestion the processes were regarded as simple chemical processes: in assimilation the intestines and the vessels were membranes obeying the laws of osmosis and dialysis. Respiration was a simple interchange of gases under conditions that could be paralleled with glass tubing and physical apparatus. Now, all this is given up. The elements of the machine are living cells, and the protoplasm of the living cells refuses to act as a piece of apparatus, but remains isolated from, unaccountable to, the laws of physics and chemistry. So, says the new physiology you must give up attempts at chemical and physical explanations; not only are they erroneous but fatally misleading. Protoplasm must be studied as a thing of its own kind; its own facts taken by themselves; its own empirical laws sought after.

Tropical Seeds in the Hebrides.

In the December number of the *Annals of Botany* (vol. vi., p. 369), Mr. Hemsley gives an account of a drift-seed of the tropical *Ipomea tuberosa* which reached the Hebrides, probably from the West Indies, by the agency of the Gulf Stream. Only one instance is recorded, but from the fact of its having in Long Island a Gaelic name, signifying Mary's Bean, it would seem that its appearance on their shore is not an extreme rarity.

Physiological Action at a Distance.

In the same number of the *Annals* (p. 373), Professor Léo Errera, of Brussels, communicates an interesting note on the Cause of Physiological Action at a Distance. This term was used by Elfving to explain some phenomena of attraction and repulsion which did not seem to belong to any of the known categories of geotropic, heliotropic, hydrotropic, &c. He found that pieces of iron and, to a less degree, of zinc or aluminium, as well as different organic substances, such as sealing wax or resin, attract the growing sporangium-bearing filaments of the well-known fungus, *Phycomyces nitens*. All other metals tried were inactive, and the filaments of *Phycomyces* itself showed a mutual repulsion.

From careful experiments, Professor Errera concludes that this apparently mysterious action is merely a matter of hydrotropism; hydrotropism (negative or positive) being the bending of a plant-organ towards the point, not where it will find a minimum or maximum of moisture, but where it will, within certain limits, lose (by transpiration) the greatest or least amount of water. Knowing that a surface which emits moisture repels the *Phycomyces* filaments, it seemed probable that moisture-absorbing substances would produce the reverse effect, and attract them; and as iron certainly absorbs aqueous vapour when rusting, its action on the fungus filament might

be a case of hydrotropism. Thus any modification of iron which lessened its capacity for rusting was also found to diminish its attraction on *Phycomyces;* polished steel scarcely attracts, and nickeled steel not at all.

China clay, which is very hygroscopic, attracted energetically, but china showed no attraction. It has been shown that, although both are essentially formed of silica, agate is very hygroscopic, while rock crystal is not, and agate strongly attracts the filament, whereas rock crystal is quite inactive. The strongly hygroscopic sulphuric acid is also strongly attractive; certain moderately hygroscopic bodies, like white soap, which lose or gain moisture, according to the relative dampness of the atmosphere, repel or attract the *Phycomyces* accordingly.

So great, in fact, is this sensibility of *Phycomyces*, that it may be used as a test of the presence of hygroscopic power. Having noticed that camphor distinctly attracted the filaments and thymol did not, the observer was led to anticipate that camphor is hygroscopic, and this, a fact hitherto unknown to chemists, was confirmed by careful weighing.

On the other hand, the roots of higher plants are positively hydrotropic, and, as would be expected if the author's views held good, they were found to bend away from iron instead of being attracted by it.

CRYPTOGAMS.

THE same number of the *Annals of Botany* is also of special interest to the Cryptogamic Botanist. It contains a paper by Mr. Barber on a new fossil Alga which he has placed with *Nematophycus*—on perhaps scarcely sufficient grounds; one on the development of *Champia parvula* by Mr. B. M. Davis, an American phycologist—a capital piece of sound work; Professor Karl Goebel's paper "on the simplest form of moss," read at the British Association; and Professor Johnson on *Stenogramme interrupta*.

ALGÆ.

THE venerable Swedish phycologist, Professor J. G. Agardh, has earned hearty congratulations by the production of the first memoir of what, it is to be hoped, will prove a long series, in succession to the well-known *Till Algernes Systematik*. The *Analecta Algologica* is a part of the *Acta Soc. Physiograph. Lund.*, vol. xxviii., and bears the stamp of careful and critical work on a level with the best which this great systematist has given us.

The wonderful fertility of the Scandinavian school of systematic workers on Algæ (including such contemporaries as Nordstedt, Kjellman, Areschoug) is only paralleled by the past generation of Britons, which gave us Harvey, Greville, Ralfs, &c. Criticism,

sometimes rather malignant, of the labours of such great men as Agardh and Harvey proceeds mostly from Germany, where the complaint is made that they were not minute histologists, and did not proceed on the lines laid down in the most recent book on the *Mikrotechnik* of Botany. Their only productive systematists in this branch of science, Kützing and Rabenhorst, conspicuously failed to reach the standard of Agardh and Harvey in this respect, and the young German school has yet to show that improved instruments will profit them to the extent of producing a single systematist of the first rank—one whom they can place alongside of Thuret and Bornet in France. The first great phycological systematist "made in Germany" will obtain a hearty welcome.

Mr. Bracebridge Wilson, of the Church of England Grammar School at Geelong, Victoria, who has collected many of the new forms described by Agardh, has printed a very useful list of his collection of Algæ. He rivals Mr. George Clifton and other correspondents of Harvey in the palmy days of collecting Australian Algæ, when new forms needed less looking for than in these later times.

The Pliocene Birds of Oregon.

In a recent issue of the *Journal of the Academy of Sciences of Philadelphia* (ser. 2, vol. ix., pp. 389–425, pls. xv.–xvii.), Dr. R. W. Shufeldt contributes an interesting memoir on the fossil bird-remains from the later Pliocene deposits of the Silver Lake district, Oregon. The bird-bones obtained from these deposits are generally more or less nearly perfect, and are in almost all cases sufficiently well preserved to fully justify the author in the determinations he has made. The majority of the forms belong to existing genera and species, and the bird-life of the Oregon lakes in Pliocene times must accordingly have been very similar to that of the present day. Then, as now, great flocks of swans, geese, and ducks frequented the lakes at certain seasons of the year; while cormorants and pelicans lined the shores, and gulls and terns hovered in the air. Grebes also frequented the sedges, and sandpipers and phaleropes coursed along the marge of the waters. Still, however, in spite of this general similarity in the avifauna of the past and present, there were certain types in the former epoch which would be missed now. For instance, there was a ponderous goose, and likewise a swan, both of which are now extinct. More remarkable, however, was the presence of a gigantic cormorant, of even larger size than the recently extinct Pallas's cormorant of Behring Island; and scarcely less so was that of a flamingo, of which numerous characteristic bones are figured. Herons were represented by an extinct species of the type genus; and a similar remark will apply to the group of eagles. More noteworthy, however, is the presence of a grouse believed to belong to an extinct genus; although it must be confessed that it would have

been desirable that the author should have some more satisfactory specimen than a metacarpus on which to make the determination.

Some interesting observations on the phylogeny of birds, in the course of which the author states that he believes the ancestral types of all the groups to have had keel-less sterna, will be read with interest.

OYSTERS.

DR. BASHFORD DEAN, who was sent by the United States Fish Commission in 1891 to study the cultivation of the Oyster in Europe and America, has just issued a "Report on the Present Methods of Oyster Culture in France" (*Bull. U.S. Fish Comm.* for 1890, art. 14, pp. 363–388, pls. lxviii.–lxxviii.), and "The Physical and Biological Characteristics of the Natural Oyster Grounds of South Carolina" (*Ibidem*, art. 13, pp. 335–361, pls. lxii.–lxvii.), published at Washington, 1892. The decreasing supply of the home markets, and the need for information as to the means for keeping up the supply in the best and most profitable manner, were the reasons for Dr. Dean's studies.

In the first report quoted above special reference is made to the difficulties which had to be surmounted by the French, and to the very high state of perfection to which the cultivation of the oyster has been brought, especially in the districts of Arcachon and Auray. "Natural difficulties," he says, "have caused the French to study division of labour in the industry; to make, for example, one locality furnish the seed, another to raise the oyster to maturity, a third to flavour or colour it, and sometimes a fourth to prepare it for transport. Under these conditions the growth of the industry has been especially and almost entirely dependent upon the wise action of the Government. The reservation of the natural grounds as State property, and the forbidding of general public dredging, is generally regarded as the keystone of French oyster-culture. These grounds—once exhausted, now flourishing—are regarded as the permanent capital of surrounding areas, whose profits, in the form of seed-oysters, are shared by all alike." The importance of Coste's experiments and deductions is warmly referred to, and the industry has become a source of considerable revenue, both to the State and to the culturist. The report goes into the full details of the culture, and is considerably enriched by numerous reproductions of photographs of the "pashs" taken by the author and others.

In the second paper, Dr. Dean describes the celebrated "oyster flats" of South Carolina, in which the oyster ("cats' tongues" or "raccoons") may be said to grow wild. The flats on which these oysters grow are acres in extent, and have the general appearance of a low coral reef. Half the life of the oyster is spent in the air, and half under the water. As these areas are mainly mud banks, the

details of the lives of these molluscs in this district are particularly interesting. The report deals in the fullest manner with the natural conditions affecting the growth, the nature of the bottom, the food, enemies, &c., of the South Carolinan oyster, gives detailed analyses of the waters in all districts where the oyster lives, and is illustrated and made clearer by the reproductions of photographs. It would be well if our own Government were to secure a number of copies of these reports, and furnish them to the oyster growers of our coasts.

Mr. John Cordeaux, in the *Naturalist* for January, 1893 (p. 5), continues his records of the migration of birds, as observed on our East Coast. From these "Bird-Notes from the Humber District in the Autumn of 1892," we learn that two "great rushes" of migrants occurred on September 20 and 21, and again on October 13 to 16. Both these rushes took place under exactly similar conditions, *i.e.*, with easterly gales. The past autumn was also remarkable for the unusual number of rare or occasional wanderers which turned up in the district.

"There were brave men before Agamemnon," and there have been ornithologists since Gould, but the results of their labours appear to be unknown to a writer who discourses of the Oházal and Shama in last month's *English Illustrated Magazine*, and displays, in his somewhat pretentious article, an astounding ignorance of recent work on these species and their allies. But what can be expected of one who uses the term "hybrid" as if it inevitably connoted sterility? We can only hope that in future his "ornithological researches" will be brought more up to date, in which case he may be mortified by discovering in his paper "little mistakes" quite as serious, if not as diverting, as that which, according to him, caused Linnæus to name the Oházal *Copsychus* (or rather *Gracula*) *saularis*.

In the *Journal of the Anthropological Institute* for August and November, 1892, Mr. John Allen Brown writes on the "Continuity of the Palæolithic and Neolithic Periods." He divides the "stone age" into Eolithic, Palæolithic, Mesolithic, and Neolithic, mainly according to the workmanship of the implements. Workmanship alone is, however, scarcely a satisfactory test of date, in the absence of distinct geological evidence as to the relative age of the specimens. We observe that the whole of the implements figured by Mr. Brown were found on the surface. The gap that exists in this country between Palæolithic and Neolithic does not yet appear to have been satisfactorily bridged.

An interesting contribution to our knowledge of the reproduction of the Foraminifera was made by Mr. J. J. Lister at the meeting of

the Cambridge Philosophical Society on November 14. Mr. H. B. Brady had already described specimens of *Orbitolites*, showing the margin of the disc crowded with young shells, and Mr. Lister was able to extend his observations by studying the soft parts of specimens collected on the Tonga reefs. It now appears that the reproduction of *Orbitolites* takes place by the formation of spores. Each spore contains a nucleus lying in its "primordial chamber." After several rings of chamberlets have been added, a stage is reached at which the nucleus appears to be represented by numbers of irregular, darkly staining masses scattered through the protoplasm of the central part of the disc. In the later stages numbers of oval nuclei are found in the protoplasm, often arranged in pairs, and in favourable preparations they may be seen to be undergoing division.

WE are glad to notice that the New Zealand Government is actively engaged in preventing the total extinction of the rarer plants and animals of the colony. Acting on the advice of Mr. Henry Wright, the Government has arranged for the purchase of Little Barrier or Hauturu Island, near Auckland, which will be kept as a national preserve. This island measures $4\frac{1}{4}$ miles in length by $3\frac{1}{2}$ miles in breadth, and rises in the centre to an elevation of 2,000 ft. It is generally rugged, but there is comparatively flat land at the northern and southern extremities. Even now its flora and fauna is particularly rich and varied, and no more suitable area could have been secured.

Bulletin de L'Herbier Boissier is the title of a new publication in the interests of Systematic Botany, issued under the direction of M. Eugène Autran, Curator of the Boissier Herbarium at Geneva. The bulletin is to contain original articles, notes, &c., will appear irregularly, and form each year an octavo volume, of about 400 pages, with plates. The first part contains a paper, with two plates, on the genera *Achatocarpus* and *Bosia*, and their place in the natural system, by M. Schinz and the editor; also an enumeration of the plants contained in Fascicle V. of "Plantæ Postianæ," by the collector himself, including a description of the new species. The specimens were gathered for the most part in the mountain chains of Amanus and Kurd Dagh, in the north-west corner of Syria. The chain of Amanus is well wooded, and the flora consequently differs considerably from that of the almost bare mountains of Lebanon and Palestine. The whole occupies thirty-two pages.

THE severe frost of the early part of last month gave unusual opportunities for the study of river-ice on the Thames. In the upper part of the tidal waters, where there is no salt, though the rise and fall of the tide is still considerable, the "ice-foot," or ledge along the

shore, was often three or four feet in thickness, in one place it measured five feet. With the rise of the tide many of these masses of spongy and cavernous, gravel-laden ice were detached, often in blocks of sufficient size to support and carry away the largest stones in the river-bank. So the ice-age is not quite over, and many erratics may have again started on their travels during the recent frost.

In the *Proc. Roy. Phys. Soc. Edinburgh*, vol. xi., p. 215, Mr. James Bennie gives a valuable list of the fossils found in the raised sea bottom at Fillyside Bank, near Leith; a deposit described by Hugh Miller so long ago as 1854. The most interesting result of a closer examination is, perhaps, that the change of level does not seem to coincide with any climatic change, like that indicated by the Arctic shells in the raised sea-beds of the Clyde district. The whole of the mollusca from Fillyside, determined by Mr. Andrew Scott, are still living in the neighbourhood; and the same is the case with the associated flowering plants identified by Mr. Clement Reid.

Mr. R. Etheridge, jun., is engaged on a monograph of the Permo-Carboniferous Invertebrata of New South Wales, and the second part, relating to the Echinodermata, Annelida and Crustacea, has just reached this country. In their general aspect the fossils are much like those from corresponding beds in Europe; at the same time, slight differences are perceptible, which have necessitated the erection of new genera and subgenera, and indicate that the separation of a marine Australian province had already begun. The characteristic, however, that first strikes the eye with regard to this fauna is the large size of the individuals; especially is this noticeable with regard to the crinoids. We are glad to learn that Mr. Etheridge is not only completing the present monograph, but is extending his researches to the Silurian Invertebrata of New South Wales, and to the Palæontology of Queensland and New Guinea.

The Devonian rocks have furnished fields for many geological battles. Peace, however, reigned for some years in the North Devon area, until Dr. Hicks, in 1890, renewed the attack on the presumed orderly succession of rocks, found fossils in the Morte Slates, and claimed them to be no part of the Devonian system. The fossils discovered at that time were too obscure for specific identification, but he announces (*Geological Magazine* for January) that the Morte Slates "are now proved by their contained fossils to be of Silurian age." We have yet to wait the particular description of these fossils, but, in the meantime, Dr. Hicks gives examples of Folds and Faults in the Devonian rocks at and near Ilfracombe, and these disturbances (in his opinion) necessitate a different interpretation of the succession of strata from that generally adopted. Instead of being one con-

tinuous series, with a regular dip to the south, he finds the beds to be much folded in several broken troughs, and to be constantly inverted. Older beds thus appear to overlie (conformably) newer strata. He concludes that the realisation of these facts will necessitate, in future, a great reduction in the thickness hitherto given to the Ilfracombe series, and the rearrangement of the fossil zones. Jukes, in 1866, drew prominent attention to the crumplings of the strata, and he then remarked: "From what I saw elsewhere about Ilfracombe and Mortehoe, I believe that, while there is a real general dip to the south throughout the district, this dip is by no means so prevalent as it appears to be, and that the real thickness is accordingly much less than would be at first supposed." (*Quart. Journ. Geol. Soc.*, vol. xxii., p. 357.)

An excellent portrait of Sir Archibald Geikie, and a memoir of him by M. de Lapparent, appear in *Nature* for January 5. A brief memoir and a portrait of Professor T. Rupert Jones are published in the *Geological Magazine* for January. The biographical notice of Sir Archibald Geikie is one of the strangest misrepresentations that has appeared for some time. We are glad to observe that a writer in the *Daily Chronicle* of January 14 has placed the facts of the case before the public.

Attention is sometimes called to the poor attendance at the meetings of the learned societies, except on occasions when a warm and exciting debate is expected. This is natural enough when the papers to be read are of a detailed character; they may be important, and they will interest a few members, but to the many they must be dry. At the Linnean Society much time is profitably devoted to the explanation of specimens exhibited. It is, however, questionable whether the average attendance at scientific meetings has seriously decreased. Writing in 1821, Leonard Horner says: "I went to the Geological Society, which seems to me to have got into very feeble hands, and to want a great deal of the energy it had in former days." He refers to times when Warburton, Wollaston, and Greenough were among the leading spirits. Horner, who had joined the Society in 1808, and was chosen as one of the secretaries in 1810, was (at the time he writes) living in Edinburgh, so that he only occasionally attended the meetings. Coming to reside again in London in 1827, he must have enjoyed many of the gatherings, when Sedgwick, Fitton, Buckland, Murchison, De la Beche, and Lyell were there; and also the subsequent proceedings, when, "after the meeting, we adjourned to Lord Enniskillen's; Owen, Clift, Buckland, Fitton, Major Clarke, and Edward Bunbury and his brother, and we had Crustacea, carbonised fragments of Costæ of a mammal (probably Bos-broiled boniensis), and much smoke and merriment."[1]

[1] See Memoir of Leonard Horner, by K. M. Lyell, vol. i., p. 192, and vol. ii., p. 44.

THE illustration of scientific lectures or papers by means of lantern-slides is becoming fairly general; and it certainly tends to render the meetings of learned societies more instructive as well as more interesting. In this way the physical features of a country or the microscopic structure of a rock, the organisms of sewage or the "extinct monsters" of many geological periods, may be faithfully reproduced on the screen from photographs or original drawings. The Royal Society, the Linnean Society, the Royal and London Institutions, the Geologists' Association, and other bodies in London, have introduced the lantern into their meeting-rooms with marked success; the Geological Society, however, has hitherto held aloof.

THERE must be a sad lack of originality, combined with a confused sense of the rights of literary property, at the Royal Gardens, Kew. Not very long ago, the Assistant Director was in trouble about the originality of some observations about sugar canes, and now the Director himself has to answer for a remarkable feat in the way of piracy. At least, this is the case according to a letter we have received from Mr. James Britten, Editor of the *Journal of Botany*. It appears that when Miss North died, Mr. Hemsley wrote for the *Journal of Botany* an obituary notice which was published in that *Journal* for 1890 (p. 329). Last year, Miss North's *Recollections of a Happy Life* was published, and the Fifth Edition of the Guide to the North Gallery has recently appeared, with a short biographical account of Miss North, which is officially stated to be compiled from the *Recollections*, and other sources. Mr. Britten writes that, on the contrary, this biography "is taken bodily, and without a word of acknowledgment, from the *Journal of Botany*. . . . The *Recollections* have yielded twelve lines out of five pages; from the 'other sources,' apart from the *Journal of Botany*, not a sentence has been cited."

Two new botanical journals appeared last month. The one is under the direction of the Department of Botany in the University of California, and named *Erythea: a Journal of Botany, West American and General*. The other is a monthly, entitled *The Orchid Review*, published by Messrs. West, Newman & Co., London. The Germans have also issued a new geological monthly, entitled *Zeitschrift für praktische Geologie*.

I.

On some Problems of the Distribution of Marine Animals.

WHEN Professor Hensen started on his Plankton expedition in 1889, to attack the problem of the "metabolism of the ocean" from a new point of view, and with methods other than had been employed hitherto, he wanted, as is well known, to study the organic life in high seas as free from the influence of coasts as possible, so as to obtain a more accurate conception of the distribution of animals and plants, and to procure a large quantity of material to which no exception could be taken from which to draw conclusions. Of course, he does not believe, as some of his opponents would have one think, that he has determined once and for all, by his captures, the contents of the part of the ocean he passed through; he merely intended to see what kind of material occurred at that particular time, and Hensen himself is well aware that such complicated problems as are resting in the bosom of the ocean can only be attacked, and not solved offhand.

The hypothesis upon which he acted was, that the distribution of organic life, if not influenced by the coasts and the everchanging conditions we observe there, must be sufficiently constant to enable us to obtain an accurate idea of the contents of the ocean by fishing in more or less limited areas with the help of accurate nets.

His views were already contested and defended at the time when the question of supplies for the expedition was being discussed, and there were naturalists who, without being quite convinced of Hensen's views as to the uniformity of the marine fauna, were, nevertheless, strong upholders of his plans, saying that such an undertaking as his was worthy of support in any case, as the results, whatever they might be regarding the uniformity of ocean life, would certainly be a large contribution to our knowledge of biological and morphological questions.

So far as I can ascertain from a relatively small part of the rich material which the expedition brought home (the Craspedote Medusæ, which were given to me to work out), this view has, as a matter of fact, proved true. In the bottles entrusted to me, I was able to examine several new and interesting forms, besides many other species not sufficiently characterised at present, and the material,

though small in comparison to some other groups, is rich enough in itself to allow conclusions in faunistic problems.

The problems of the marine fauna are so varied and numerous that we can discuss only a few of them in this place. For convenience, we might divide them into quantitative and qualitative problems. It is about the former that the combat between Haeckel and Hensen is carried on, *i.e.*, about the uniformity of the distribution, and about the foundation which is given by *single* captures to *general* conclusions. It is too early as yet to enter into this question, basing conclusions only on the results of single groups, but it may be affirmed that everything known until now from the different investigators indicates a much more equal composition of the Plankton than was supposed even by Hensen himself. It is certain that the influence of the coasts extends to a considerable distance, that even islands cause modifications, and that currents often change the aspect of the fauna almost at once; but Hensen is the first to acknowledge the importance of all these factors.

Setting aside these quantitative problems, there remain the qualitative ones, namely, the nature of the composition of the fauna with regard to the different species, and the geographical occurrence of the single forms. In other words, *are there in the sea as as on the mainland areas of distribution with characteristic inhabitants, or* (though certainly the coast has zones of life) *are the peculiarly Plankton forms universally distributed in it?* We know for certain that there are forms of life peculiar to the open sea, Plankton animals, *par excellence*, which are cosmopolitan, and which occur both in the Atlantic and in the Pacific or Indian Ocean, and in very different latitudes. At the last Congress of the German Zoological Society, von Graff exhibited such cosmopolitan forms of the group Turbellaria, and von Martens, Spengel, Chun, and others seconded him by relating similar facts in the Molluscs, Tornarias, Siphonophores, &c., and this led to an interesting discussion as to the probable continuity of the two oceans in former ages. Among the Medusæ we meet with species the distinguishing characters of which are so insignificant that we should, without doubt, consider them as belonging to *one* species had they not been found in such different regions. On the other hand, we know some forms of Medusæ which have been found hitherto only in a certain limited district, which occur in this district regularly, but which have never been seen elsewhere.

Without impairing the fact that there are cosmopolitan forms which are as regular inhabitants of the Atlantic as of the Pacific Ocean, it is to be expected *a priori* that we should find in different latitudes at least a different fauna. Since in a different latitude the life-conditions undergo a marked change, and show differences in the temperature, in the movement of the water, in the weather, and so forth, we ought to find a different adaptation and different characters.

Dana was one of the first to distinguish such geographical faunistic provinces in the open sea by drawing thermic (or, as he called them "isocrymal") lines on the map, which did not quite correspond to the lines of geographical latitude. The observations of temperature were made on shore, and the empirical data were not very large, but he has taken into consideration the "modifying principles," currents, &c., and has deduced some good general conceptions for his work on Crustaceans, the distribution of which had led him to his ideas.

As another interesting attempt in this regard, we may mention the account given by Brandt in his monograph of the Radiolarians of the great oceanic currents in the Atlantic, which he supposes to be the most important factors in the distribution of animal life in the ocean. It is remarkable that he considers them as "circle streams," by the direction of which a large amount of pelagic animals are always kept within certain limits, and only very few of them can be carried away by the side branches of the currents to other currents, temperatures, &c., where they probably perish, while the majority remain under their regular life-conditions. But the attempts to divide the ocean into faunistic districts are very few, whether it is that no such limits have been believed to exist at all, or that our empirical knowledge has not been large enough till now to allow conclusions; and we have to wait for the results of many explorations till we can obtain an idea about these complicated relations.

The Plankton Expedition, limited as it was in extent, has given us some insight at least into a certain part of the ocean, and we may draw conclusions from it with comparative security, since the fishing-stations were fairly close together, and since improved methods were employed, such as, for example, the ordinary vertical net going down always to a depth of 400 m. in order to catch the animals which rise and sink to avoid the changing influences of the surface at certain times. The results which I have obtained in the Medusæ have confirmed my conviction that we can distinguish in the ocean certain districts of horizontal distribution.

The Craspedote Medusæ can be divided, as is well known, into *Leptolina* or metagenetic forms, which are derived from a polypoid stage, and *Trachylina*, or forms with a direct (hypogenetic) development through a free swimming planula and actinula stage. As might be expected from the places of capture, chiefly lying in the open sea, where polyps have scarcely any chance of flourishing, the Trachylina form by far the majority of the Plankton Medusæ, and the few Leptolina which have been discovered always show a relation to the coast. It might be interesting to control this advance, as the result of life in the open sea, by a comparison with other groups, which also have a sessile stage in their life-history. The Narcomedusæ among the Trachylina being acknowledged by all compe-

tent observers to be relatively rare animals, we might not be astonished to find that the Trachymedusæ proper, with the families Trachynemidæ, Aglauridæ, and Geryonidæ, form the chief contents of the Medusan hauls. However, the members of these three families participate in the composition of the hauls in a very different manner in the different regions, as well in quantity as in species.

First of all we notice that the common law on the mainland, that the number of the species constituting a fauna increases towards the Equator, obtains also in the sea. In the North Atlantic very often one single species, sometimes in an enormous number of individuals, formed the contents of the Medusan haul, while near the Equator the net always brought different species of Craspedota, sometimes five or more, to the eyes of the naturalists. Besides this general law we notice that we can distinguish limits of distribution, not only for species, but also for whole families; of course, these are not sharp dividing lines, such as the currents furnish for some species, but one can speak of districts where this or that family is predominant. The Aglauridæ were the chief constituents in the northern part, the Trachynemidæ in the middle, and the Geryonidæ in the equatorial part of the course followed by the ship; we find Aglauridæ and Trachynemidæ in the equatorial district too, but in much inferior quantity and variety of forms, so that we are fully entitled to look upon the Geryonidæ as inhabitants of the warmer oceans. It is in correspondence with this fact that Geryonidæ have not been found hitherto in the North Sea, and that only one example is known from the Atlantic coast of England, whereas they are abundant in the Mediterranean, and are well known to the investigator of embryology there.

The Trachynemidæ cannot be called a tropical and subtropical family with as much right, since their abundance in these districts is not so great; however, they do not seem to pass beyond a certain northern limit, Florida and the Gulf Stream. Haeckel has already recorded as a strange fact that, in spite of so many explorations, no Trachynemidæ have been seen in the North Atlantic. We may confirm these statements by the results of the expedition for the *typical* relatives of this family; however, three somewhat *aberrant* forms have been found in the northern part of the course of the expedition, which probably come from greater depths, and which occur nowhere else, and claim special morphological interest.

A picture of the distribution of whole families can only be a rough one, and to get a more precise conception of the fauna we have to consider the single species. Thus we are able to divide the ocean traversed by the expedition into different regions, with their characteristic or "leading forms." First, we can distinguish a *northern district*, beginning at the Scottish coast, and having the Gulf Stream and the Azores as its southern limits. The characteristic forms here are *Aglantha digitalis*, *Solmaris multilobata*, and *Homoionema* (n. gen.), all

of which never occur in any succeeding part of the course. To the south of the Gulf Stream the composition of the Medusa Plankton changes at once, and we meet with another fauna, which remains the same in its chief components till we pass into the North Equatorial Current, which the expedition passed near the Cape Verde Islands to the west of Africa, so that we might speak of a *second district*. The leading forms in it are *Liriope cerasiformis*, *Rhopolonema velatum*, and *Aglaura hemistoma*. However, this district is not quite uniform; in its western part, from the Gulf Stream to the Bermudas, and to the Sargasso Sea, we find, besides the three forms mentioned above, several other characteristic species, which occur also in the Guinea, and even in the South Equatorial Current, and which can only have come there by the connection of the "circle currents"; while another part of this second district, from the Sargasso Sea to the North Equatorial Current, shows only the three leading forms in astonishing equality. In a *third district*, namely, from the North Equatorial Current through the Guinea Stream to the South Equatorial Current (to the north of Ascension) the fauna is not such a definite one, as we have forms there which occur in the second district also, together with forms which we find only in these southern currents.

From Ascension, Hensen's expedition crossed the Atlantic to Brazil (mouth of the river Amazon), and it is remarkable that in the eastern part of this course some species appear which were not to be found in the western part. Probably they are inhabitants of a colder current coming from, and turning to, the southern temperate zone, and are not able to flourish in the warm water of the equatorial currents. On the other hand, we find in the western half some characteristic forms, as, for example, *Liriope catharinensis*, which occur only in this limited district. From Brazil homewards the ship crossed the third and second districts again, and the same species were found at the corresponding places. Thus we see positively that certain species are confined to certain districts, and we are entitled to speak of the existence of faunistic regions in the ocean. The temperature seems to be an important factor for producing such limits, but what is of still greater influence in determining them is the course of the great oceanic currents, as is quite evident from a comparison of the map with the facts mentioned above. By their effect, also, the wide distribution of other forms, as *Aglaura hemistoma*, which occurs in the whole Atlantic to the north of the Gulf Stream, can be explained. A further interesting fact is the identity of some species of the middle (second) district with the hypogenetic Medusæ of the Mediterranean, and I can state this identity with the greater certainty, since I have studied the Mediterranean fauna throughout a whole year.

In a former publication I made all these statements with great reserve, as being of value in the first place for the Medusæ only, and I laid stress on the fact that especially the division of the ocean into

districts required further confirmation in other groups. Meanwhile, several of the Plankton investigators seem to have come to similar conclusions, and quite recently an interesting paper by Dr. Dahl has appeared, which treats of the different species of the genus *Copilia* (Saphirine Copepods of the open sea) and their share in the composition of the Plankton. Without entering into the quantitative questions, we will compare only the results of this author regarding the qualitative distribution.

First we notice that Dahl distinguishes a difference between a northern region of the course taken by the expedition and a southern one, inasmuch as in the whole district to the north of the Florida Current and the Azores no members of the genus *Copilia* occur; so it appears that the genus *Copilia* is tropical or subtropical, as, for example, the family Geryonidæ among the Medusæ. In the remaining part of the course he found five different species (it is remarkable that both sexes could be recognised, which show, as is well-known, a pronounced dimorphism), and these five species are distributed in a very characteristic manner. Two of them, *C. lata* and *C. vitrea*, appear along the whole course from the Florida Current to Ascension, to Brazil and back to the Azores; they might be regarded as similar to such forms as *Aglaura hemistoma* among the Medusæ. Two other species, *C. mirabilis* and *C. media*, alternate with one another in their occurrence; *media* being found in the Sargasso and in the part northwards of it, *mirabilis* in the southern parts. So these forms seem to substitute each other, while a fifth form, *C. quadrata*, occurs chiefly in the warm currents from the Cape Verde Islands to Ascension. Dahl suggests rightly that this distribution cannot as a matter of fact result from mere haphazard, and he discusses the probable reasons of it, the Atlantic currents and the temperature, in an interesting manner.

If we consider, not the *single* species in their distribution, but the faunistic picture which results from the simultaneous occurrence of several species, we may distinguish for these Copepods the same limits of districts which we have noticed for the Medusæ. If we look at Dahl's chart, we see the abundance of species in the district from the Florida Current to the Bermudas, where some species of the more southern currents occur besides the forms of district No. 2; we see that one district shows the species schematically expressed *a*, *b*, *c*, *d*, another *a*, *d*, *e*, and a third *a*, *c*, *d*, and so on; and we see further that there is a difference between a western part of the Atlantic traversed and an eastern part, all of which results correspond to what we have found in the group of the Medusæ.

Another publication, by Dr. Apstein, deals with pelagic Annelids, the Tomopteridæ and the Alciopidæ. We find that this author, too, can distinguish a northern and a southern region, with the limits drawn above. The Alciopidæ do not occur to the north of

the Gulf Stream and the Azores, whereas the Tomopteridæ are abundant there. He mentions an interesting form, which appears to be cosmopolitan, since it has been found in the China Sea as well; he says that several species also occur in the whole southern region; but for other forms he gives special districts of distribution.

Finally, passing from these groups of animals, we may look forward with interest to the *faunistic results* that will be obtained in other groups. Some preliminary communications of *morphological* interest have also appeared (on the pelagic Anthozoa, by Van Beneden, and on Gastropods, by Simroth), and whatever the results may be regarding the uniform distribution of the Plankton in the open sea, though till now they agree very well with the views of the originator of the expedition, we may be sure that our knowledge of the faunistic distribution and of morphological facts will undergo a remarkable extension by this expedition.

I will not conclude this short sketch without drawing attention to the practical side of such an expedition, and its value for fishery questions in general. Though it may not appear so at first sight, yet, however any group of the animal kingdom may be related to the other groups, and however far apart some families, classes, or types may stand in the morphological system, they are nevertheless in direct physiological connection. The fishery commissioners' task must be not only to observe the fishes in their occurrence and their wanderings, and to study their enemies and their food, but also to take into consideration every pelagic animal, and to calculate the complicated mutual relations existing between the single groups.

Thus I can affirm that even the Medusæ have an importance for the fishery questions; not as enemies of the fishes nor as their food, but as competitors. It is well known that small Crustaceans, chiefly Copepods, form the food of most fishes, and the oceanic Medusæ, which are beasts of prey, feed on the same material. These Medusæ are very voracious (one can often find their stomachs filled to bursting with Copepods) and are very bold in their movements; they are well provided for the struggle for life in the open ocean, and since they occur in enormous quantities, they are certainly of some importance from the practical point of view as well.

If we make a little step forwards in the knowledge of the complicated relations in which the inhabitants of the ocean stand to one another, the beast of prey and the animal it feeds on, as well as the plants which form the "primæval food" (Urnahrung); in other words, if we come to some understanding of the "production and the metabolism of the ocean," then a chief object of the expedition will have been attained.[1]

[1] Some of the first publications of Hensen's Expedition, namely, the description of the journey, &c., have now begun to appear.

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OTTO MAAS.

II.

On Pasteur's Method of Inoculation and its Hypothetical Explanation.

"IN Nature's infinite book of Secrecy," the bacillus and kindred organisms form an interesting, and of late a much-read, page. Their very minuteness, and the difficulty of studying them, lends an additional fascination; and they rise into painful importance in the light of the modern view that they are the cause of disease.

Since splenic fever was first attributed to the bacillus by the two French observers, MM. Davainne and Rayer, in 1861, one disease after another has followed, until finally the cause of influenza has revealed itself under the microscope of the investigator. To give a list of diseases now attributed to it, would be to compile a page from a medical treatise.

All this renders the bacillus of paramount interest, yet a still greater interest attaches to the extraordinary pathways to health pointed out by the brilliant researches of Pasteur. I allude to his well-known system of inoculation in order to confer immunity from disease.

The foundation of Pasteur's process is the cultivation of the microbe—bacillus, micrococcus or bacterium—outside the animal body in a suitable medium. Various substances—meat broth, sugar and peptone, Liebig's extract, &c.—are used in which to grow the microbes. These liquids, or solids, must be sterilised—that is to say, heated until all germs which may have existed in them have been destroyed—and afterwards protected from atmospheric germs by plugs of cotton wool. With these precautions it is found possible to obtain pure cultivations of any microbe which may be sown in the medium.

Into a tube or flask, then, containing this medium a drop of blood, or fragment of tissue, from a diseased animal is introduced. The microbe existing in it at once increases in numbers, and soon renders the medium turbid. A drop of this first cultivation introduced under the skin of a healthy animal reproduces the original disease with which it was associated. To obtain a second cultivation, a drop from the first is placed in another portion of the medium, where the microbe increases as it did in the first. Proceeding in this way, a succession of cultivations may be obtained. Now Pasteur

found that if the cultivations succeeded each other at intervals not greater than twenty-four hours, each successive cultivation retained the power of producing disease with as great energy as did the first. If, however, a longer interval was allowed to elapse a remarkable change took place: the disease was produced in a milder form, and, more remarkable still, the animal inoculated with one of the later cultivations was protected from the more deadly disease. Moreover, by prolonging the period between the successive cultivations the power of producing disease became less and less, until it was entirely lost.

In attenuating—as he terms it—the virus of splenic fever, again, Pasteur used heat and exposure to the air, while in other cases he has used the still more remarkable method of making his successive cultivations in the body of some animal.

This is the method originally pursued with the microbe of hydrophobia. A monkey was inoculated with the virus from a mad dog, and the spinal cord of this monkey was afterwards used to inoculate a second, which in its turn furnished matter to inoculate a third, and so on; and Pasteur found that the matter from the spinal cord of the first monkey produced a milder disease than the original virus; that from the second monkey a still milder one, and so on; and by continuing the process long enough he could obtain a virus of any degree of mildness desired.

As with the virus attenuated otherwise, inoculation with this gave immunity from the severer forms of the disease. Closely connected with this is the fact that if the virus, attenuated by passing through a series of monkeys, be passed in the same way through a series of rabbits, it regains all its former virulence and reproduces the disease in its original form; and Pasteur states that he is able to revive the power of the attenuated virus of splenic fever by passing it through a series of guinea-pigs, beginning with one just born and gradually increasing the age; and that of fowl-cholera by passing it in like manner through canaries, blackbirds, &c.

A later method of attenuating the virus of rabies was the simple exposure of the spinal cord of a rabid rabbit to the influence of dry air in a flask. At the end of about fifteen days it was found that the spinal cord thus exposed had almost entirely lost its virulence. And in preventive inoculation a series of such cords was used, beginning with the oldest and least virulent, and proceeding to the newest and most virulent.

In the case of swine fever, again, Pasteur attenuated the virus by passing it through a series of rabbits.

Various other methods of attenuation have been used. M. Chauveau[1] has shown that it may be effected by compressed oxygen. With a moderate degree of compression he found the growth of the

[1] *Comptes Rendus*, vol. xcviii., pp. 1232—35.

microbes was rendered more vigorous, while excessive pressure killed them altogether; but with great care he found a certain zone of pressure under which the bacillus of splenic fever was modified in such a way that sheep inoculated with it were protected against the disease.

M. A. d'Arsonval,[2] again, claims that cultures may be attenuated, as well as sterilised, by the use of carbonic acid gas under high pressures.

MM. Héricourt and Richet[3] mention three ways in which cultures may be attenuated so as to be suitable for protective inoculation, viz.:—

1. Making the culture in a less suitable medium, (*e.g.*, beef-tea *not* peptonised).
2. Keeping the cultures till they have passed the period of their most vigorous growth.
3. Cultivating above or below the most favourable temperature for growth.

The general principle of attenuation seems to be, to make the cultures under slightly unfavourable conditions, by which the microbes are rendered less vigorous.

It is not, however, so much with these interesting experiments that I am concerned at present as with the explanation offered by Pasteur and others of the way in which these attenuated viruses are supposed to confer immunity from disease.

There are two general views, the first being that the microbe of the "modified cultivation exhausts the soil," that is to say, deprives the blood or tissues of something necessary to the growth of the unmodified form. This is known as the *Exhaustion theory*. The second view is that some product of growth—some chemical secretion of a toxic character—of the modified virus is inimical to the growth of the original. This is the *Antidote theory*.

Bacteria belong to the vegetable kingdom, and both theories have the support of analogy, that is to say, both explanations have been applied to the case of the higher plants. It is well known that after repeated crops of any species of plant on a particular spot, the land seems, as it were, to become tired of it, and the growth is less vigorous. Two explanations are offered: first, that the particular plant has exhausted the soil of something specially needful for its growth—this is the *exhaustion* theory; secondly, that the plant renders the soil unsuitable for another of its own species by polluting it with its excretions—this is the *antidote* theory.

The former is the explanation most generally received, and is the one which has obtained the support of Professor Tyndall, whose exact words may here be quoted:—

"Now contagia are living things, which demand certain elements of life, just as inexorably as trees, or wheat, or barley; and it is not

[2] *Comptes Rendus*, vol. cxii., p. 667. [3] *Comptes Rendus*, vol. cvii., p. 690.

difficult to see that a crop of a given parasite may so far use up a constituent existing in small quantities in the body, but essential to the growth of the parasite, as to render the body unfit for the production of a second crop. The soil is exhausted, and until the lost constituent is restored, the body is protected from any further attack of the same disorder. Such an explanation of non-recurrent diseases naturally presents itself to a thorough believer in the germ theory, and such was the solution which, in reply to a question, I ventured to offer nearly fifteen years ago to an eminent London physician. To exhaust a soil, however, a parasite less vigorous and destructive than the really virulent one may suffice; and if, after having, by means of a feebler organism, exhausted the soil without fatal result, the most highly virulent parasite be introduced into the system it will prove powerless. This, in the language of the germ theory, is the whole secret of vaccination."[4]

Serious objections may be urged against both explanations. Indeed, the difficulties in the theoretical conceptions of how the benefits are supposed to arise are sufficiently great even to raise doubts as to the reality of the benefits themselves.

At the very outset of our enquiry, we are confronted by the fact that these organisms, which are thus supposed to depend for their power of increasing on certain substances secreted in small quantities in the animal body, can yet be grown outside of it in various preparations. The various meat broths, sugar and peptone, extract of beef, yeast water, blood serum, &c., which are used, can scarcely be supposed to contain these substances, which are not essential constituents of the body, since the animal—as in the case of those inoculated for any disease—can live and be perfectly healthy without them.

Whatever, at any rate, may be thought of the others, water of yeast, in which Pasteur cultivated the bacillus of splenic fever, cannot be supposed to do so; and although boiled potato is a medium not generally used for pathogenic—that is, disease-producing—microbes, they have been cultivated on it. Thus, Dr. Klein ("Micro-Organisms and Disease," p. 50) relates that good crops of the micrococcus of pneumonia had been reared on boiled potato. Again (p. 103), he tells us how Groffky grew the bacillus of malignant œdema on potatoes. This, at least, most certainly shows that the growth of the microbe cannot depend on a something which exists only in minute quantities, and as an unessential ingredient in the blood or tissues of an animal.

In these preparations, moreover, the microbes increase not merely until they might be supposed to exhaust the medium of the small quantity of this something, but until they have exhausted it of all its nutritive elements, or have produced enough of their toxic principle to prevent their own further growth.

If this something is a normal constituent, how can the blood be deprived of it without serious injury to health? If, on the other

[4] "Louis Pasteur: his life and labours." Introduction, pp. xxxv. and xxxvi.

hand, it is not a normal constituent, how could it ever be present without injury? And, under the latter supposition, a person in perfectly normal health would receive no benefit from inoculation, since there would be nothing for the modified microbe to remove.

Nor has it been explained how the microbe of the modified cultivation could multiply in the body without producing the same inconvenience as the original form, supposing the microbe is, in itself, the cause of the disease.

If, as has been suggested, the malady is caused by the mere number of the microbes clogging the veins, obstructing the circulation, and taking matter from the blood, then the modified form would accomplish this as readily as the other.

But passages might be quoted which seem to imply Pasteur's belief that the "attenuation" of the virus is due to the loss of power to multiply in the animal body. The following is from the English translation of the life of Pasteur:—

"After having reduced the microbes of fowl, cholera, and splenic fever to all degrees of virulence, and brought them to a point where they could no longer multiply in the bodies of animals inoculated with them, and fixed them in media appropriate to their life, Pasteur asked himself whether it would not be possible to restore to these attenuated microbes—weakened to such a degree as to have lost all virulence—a deadly virulence, and to render them again capable of living and multiplying in the bodies of animals."[5]

Enfeebled in such a way, the microbes could scarcely exhaust the blood of that which would have supported a sufficient number of the unmodified microbe to produce disease.

Again, in the following passage it is distinctly asserted that the lack of virulence is due to the loss of power of living in the animal body:—

"The extraordinary fact is then established that the virulence may be entirely gone while yet the microbe lives. The cultures offer the spectacle of a microbe indefinitely cultivable, yet, on the other hand, incapable of living in the bodies of fowls, and, in consequence, deprived of virulence."[6]

In the modification of the splenic fever microbe the cultivations gradually lose their power of increasing in the artificial medium, and finally refuse to multiply any further; the last sowing does not produce a crop. It is one or more of the cultivations preceding the last which are used as vaccine.

Can this microbe, which has almost lost the power of reproduction, be supposed to be able to exhaust the blood of that which would support an abundant crop of the unmodified form? And if this Scylla of lack of reproductive power be hypothetically avoided, the attempted explanation splits on the Charybdis of abundant reproduction producing all the evil effects of the unmodified virus.

[5] *Op. cit.*, p. 246. [6] *Op. cit.*, p. 226.

On the other hand, if the disease is caused by the production of some toxic principle, we must surely suppose this power of production to be an essential part of the nature of the microbe, and it is difficult to understand how this power could be lost by mere cultivation; for the fact is insisted on that the modified virus is the same organism—a definite and distinct species of microbe—as the one which produces the disease in its deadly form; and if it produces the same poison it ought to produce the same disease.

If, again, the microbe produces disease by depriving the blood and tissues of matter essential to life, would not the modified cultivation do the same? In fact, we may assume that to use up the essential specific matter—"exhaust the soil"—the microbe of the modified cultivation would have to increase as greatly as the unmodified form would require to do to produce the disease; and so, either by obstructing the circulation by mere numbers, by robbing the organs of their nourishment, or by the production of morbific matter, it would cause the same inconvenience as the original virus.

Leaving these difficulties, what grounds are there for the assumption that there can exist in the blood and tissues a something secreted slowly and in small quantities, and yet not essential to health? As far as I am aware, there is absolutely no proof of the existence of anything of the sort. According to Pasteur it is secreted continuously, and yet in such small quantities that it requires in some cases years to restore a sufficient quantity to the blood to allow of the free growth of the organism. It seems to me contrary to the general principles of physiology to suppose the blood is continuously elaborating a useless product, which accumulates, and yet causes no harm: it is a direct violation of the much-talked-of "law of parsimony" in nature.

A curious and ingenious suggestion as to what this essential something may be is made by Dr. Maclagen, in an article on "Influenza and Salicin" (*Nineteenth Century*, February, 1892).

The microbe, he says, is a parasite, and every parasite requires, besides the essential elements of growth, its own particular nidus. Each microbe finds its nidus in some special part of the body—the liver, the spleen, the skin, &c., and this nidus, he suggests, may be some now-useless character handed down from some of our remote ancestors:

"This nidus once exhausted, is, as a rule, never replaced, showing that, like our rudimentary tail, it is something which is not really essential to our well-being—like our rudimentary tail, it may be some peculiarity derived from a very remote ancestor."

Once exhausted, this nidus is not replaced, and that, according to Dr. Maclagen, accounts for the immunity which one attack of disease confers from a second.

It does not, however, account for the fact that inoculation requires to be frequently renewed—every year for splenic fever according to Pasteur, and every seven years for small pox according

to the medical faculty; and, like Pasteur's, Dr. Maclagen's explanation does not account for the fact that the microbe can be grown outside the body where the nidus does not exist.

And how is the change in the organism supposed to be brought about? Pasteur believes the attenuation of the virus to be due to the action of the oxygen of the air, except, of course, in those cases where it is passed though a series of animals, or where the change is partly attributed to heat; but it is difficult to understand in what way oxygen could alter the character of a living organism so as to cause it to produce varying effects. Is there any case known of an organism altered in such a way? There are three cases among the organisms we are considering, in which the essential characters of the species are said to have been thus changed by the method of cultivation :—

(1.) Buchner has stated that by successive cultivations he has transformed the bacillus of anthrax into the harmless hay bacillus. He likewise claims to have transformed the hay bacillus into the deadly bacillus of anthrax.

(2.) A bacillus found on the seeds of *Alexus precatorius* (an Indian and South American leguminous plant used in certain diseases of the eye), and apparently identical with the hay bacillus, is, according to Sattler, capable of being transformed into a disease-producing form. An infusion of the seeds, known as jequirity, is made and several cultivations of the bacillus started from this in the ordinary sterilised media. The change in the nature of the bacillus is supposed to be effected by growth in the original jequirity fluid, and to be retained in the successive cultivations, since all these were found to produce severe ophthalmia; and not only did Sattler suppose the original jequirity bacillus was thus modified so as to produce disease, but that germs of other harmless forms which might settle down in the jequirity solution likewise became pathogenic.

(3.) The third case is that of a common mould, *Aspergillus*, which has been found to produce death in rabbits when its spores are introduced into their systems, although not in the proper sense of the term a pathogenic form.

Dr. Klein has carefully examined these cases, and repeated the experiments, with the result that all break down as examples of the change of septic into pathogenic microbes. "I feel sure," he says, "any one might as soon attempt to transform the bulb of the common onion into the bulb of the poisonous colchicum." Other observers have corroborated Klein's conclusions.

Ought we, then, to believe that by a series of cultivations a microbe can lose the power of producing disease which it once possessed?

If the microbes of disease can be altered so as to produce a modified disease, it ought to be possible to produce, say, a lactic acid microbe, which would produce a modified lactic fermentation; and further, such a modified fermentation should protect the liquid from

fermentation by the unmodified ferment. In the same way, it ought to be possible to modify the microbe of putrefaction so as to produce a decay-proof body.

With regard, again, to the other explanation that the modified microbe confers immunity by the secretion of some toxic principle which prevents the growth of the unmodified form, it might be asked:

(1.) Would not the secretion—or production in any way—of such a toxic principle be highly injurious to the animal in which it was secreted?

(2.) Why should a toxic principle be allowed to remain in the system for the year required by Pasteur's view on inoculation for splenic fever, or for the seven years required by the received view on vaccination?

(3.) Would not the mere existence in the blood and tissues of the number of microbes necessary to produce a sufficiency of the toxic principle be in itself enough to produce the unmodified disease?

Dr. Klein ("Micro-organisms and Disease") adopts this "Antidote theory" of inoculation.

But apart from the difficulties I have hinted at above, another arises for Dr. Klein, in the fact that he himself adopts the view that death, in the case of any disease caused by a microbe, is due to the chemical alteration produced in the blood and tissues. That is to say, either a change is produced in the blood analogous to that from sugar to alcohol effected by the yeast-plant, or a special ferment is secreted by the microbe. Thus the same toxic principle which causes death in the disease, is the cause of immunity from the disease when the animal is inoculated.

Now if we suppose the microbe used for inoculating increases as much as the unmodified form, then it ought to produce as great an amount of the toxic principle, and hence cause the disease in as severe a form. If, on the other hand, by increasing less, it produces less of the poison, we cannot suppose there would be sufficient to prevent the growth of the unmodified microbe if introduced; for it is to be remembered that the unmodified microbe is supposed to go on increasing until it has produced sufficient of the poison to prevent its own further growth. This is the explanation of the cessation of the disease. Hence the amount of poison necessary to prevent the growth of the unmodified microbe—in other words, to prevent the disease—is the same as that required to produce the normal type of the same. No amount of the toxic principle less than what would itself produce the disease, can be sufficient to prevent the growth of the unmodified microbe—that is, to prevent the disease.

Thus, then, as it appears to me, neither the "Exhaustion theory" nor the "Antidote theory" explains immunity from disease conferred by inoculation.

An assumption which underlies both explanations is, that after a certain number of generations, the microbes are obtained

absolutely free from all matter derived from the body of the diseased animal; but with regard to all experiments on the subject, it is to be wished it could be more conclusively shown that a more subtle poison than the comparatively gross microbe may not be present. It is assumed that the quantity of anything existing besides the microbes in the original particles of injected matter would be so small that after a few generations nothing could remain; but in connection with this we have to remember the extraordinary divisibility of matter, and also that, conceivably, this other something may have the power of increasing as well as the microbe.

Might it not possibly be that the action of the oxygen—which Pasteur thinks modifies the microbe—gradually destroys this other poison? and might not the phenomena of protective inoculation be an acclimatisation of the system to gradually increasing doses of poison, akin to that by which a man can accustom himself to larger and larger doses of arsenic and opium? But this is the merest suggestion.

It involves, moreover, the larger question of whether the experiments of Pasteur and others really necessitate the belief that the microbe is the sole and primary cause of the disease, and cannot be treated of here.

The above remarks apply to attenuated viruses in which the microbe itself is supposed to be the active agent. But many recent investigators, instead of using an attenuated culture of the microbe for inoculation, inject small quantities of the toxic principle produced by its growth. After the culture has proceeded to a certain extent, it is carefully sterilised, and the toxic principle is obtained free from microbes or their germs. To such methods of inoculation the exhaustion theory, of course, does not apply.

It has even been shown, in the case of the bacillus of tetanus, that it is the toxic principle, and not the microbe, which causes the disease. Thus MM. Vaillard and Vincent[7] assert that if the inoculation be effected with a culture in which the bacillus has not had time to produce the toxic principle, or with a culture further advanced, but washed, so as to get rid of the poison, the symptoms of tetanus are not produced. On the other hand, a very small dose of the toxic principle does produce them.

MM. Rodet and Courmont,[8] again, have shown that in the cultures of *Staphylocoque pyozène* there are two distinct principles, which can be separated by the action of alcohol. The one, insoluble in alcohol, has a prophylactic value; the other, soluble in alcohol, renders an animal more susceptible to the disease produced by the microbe. Thus, while animals inoculated with the former are more or less protected from the disease, those inoculated with the latter are rendered more susceptible to it.

[7] *Comptes Rendus*, vol. cxii., p. 239. [8] *Comptes Rendus*, vol. cxiii., p. 432.

Since there is no proof that the toxic principle is retained in the body—indeed, it has been shown that in a diseased or vaccinated animal the toxic products are eliminated in the urine—immunity conferred by inoculation, if of more than momentary duration, cannot be due to the actual presence of the poison.

M. Bouchard,[9] again, has shown that the toxic products of bacterial growth contain two antagonistic principles, which he has termed *anectasine* and *ectasine*. Possibly these may correspond to the two principles of MM. Rodet and Courmont.

According to Cohnheim, the passage of the white corpuscles of the blood through the vessels (diapedesis) is the dominant phenomenon of inflammation; and, according to MM. Massart and Brodet, these white corpuscles possess a certain chemical irritability, which causes them, when placed in solutions containing the products of bacterial growth, to pass from those parts where the solution is more dilute to those where it is more concentrated. These facts are connected in an interesting manner with inoculation.

Local inflammation is one of its phenomena, and it may be supposed that the attractive action of the toxic principle of the vaccine has caused diapedesis of the leucocytes.

According to M. Bouchard, however, it is only the principle he has named ectasine which promotes diapedesis; the other, anectasine, hinders it, and has been used by him for the prevention of hæmorrhage. When the leucocytes, thus drawn to the seat of local inflammation, meet with any microbes, they envelop and digest them. This is the phenomenon of phagocytosis. It may be held to explain a temporary immunity from disease when subject to infection; for supposing an animal is inoculated with the toxic principle, then the leucocytes are drawn to the seat of inoculation by the attractive force of the ectasine. In their consequent state of activity, and outside their proper vessels, they are in a favourable state for meeting with and destroying, accidentally or purposely, introduced microbes. This would explain immunity from disease existing *immediately after inoculation*, which is, perhaps, all that has really been experimentally proved. But it can scarcely be supposed that this abnormal activity of the leucocytes, and their readiness to attack the microbes, could be retained for any length of time.

It may be further added that Pasteur himself has expressed his conviction that the vaccinal matter of rabies is really a toxic principle produced by the microbe, and that in the case of the spinal cord of the rabbit exposed to heat and dry air, the microbe is destroyed and not modified.

G. W. Bulman.

[9] *Comptes Rendus*, vol. cxiii., p. 624.

III.

The Industries of the Maoris.

DEFINITE and well authenticated details of the habits, customs, or manufactures of rapidly disappearing peoples of the world are exceptionally interesting, not only on account of their intrinsic value, but because of the aid they give in the elucidation of phenomena connected with nations which have totally disappeared, and are only known by the few scattered objects they have left behind. From this point of view, the researches of Colenso[1] and Chapman[2] on the ancient works of the Maoris of New Zealand are valuable. Mr. Colenso has an experience extending over more than half a century, and was personally known to many of the old Maori chiefs. During his early years he had opportunities to become acquainted with many of their works which have long been obsolete, and are "scarcely known even by name to the present generation of Maoris." Nature provided these people with a variety of plants from which flax can be obtained, the principal one being the well-known *Phormium*. The finest flax was used in the weaving of garments or dress mats, the weft and warp were of different sorts of flax, and the extremely soft lustrous appearance was obtained by repeated tanning and the most careful selection of threads of the proper colour. The finest and most beautiful of these dresses are twenty or thirty years old, and it is doubtful if they can be produced at the present time. It is not that the art of weaving is lost, but the taste, skill, and patience in the selection of fibres and their dyeing are no longer to be found among the degenerate Maoris. They also wove floor and bed-mats of flax leaves, cut into narrow lengths and bleached in the sun. Baskets were made of similar materials, and little cots for the first-born child are frequently gems of weaving art made by the mother.

Flax was also used for making cords and threads. "It was ever to me an interesting sight to see an old chief diligently spinning such lines and cords—always done by hand, and on his bare thigh. The

[1] "Vestiges, Reminiscences, Memorabilia of Works, Deeds and Sayings of the Ancient Maoris." By W. Colenso, F.R.S. *Trans. and Proc. New Zealand Inst.*, vol. xxiv., p. 445, 1892.

[2] "On the Working of Greenstone or Nephrite by the Maoris." By F. R. Chapman. *Loc. cit.*, p. 479.

dexterity and rapidity with which he produced his long hanks and coils of twine and cord, keeping them regular, too, as to thickness, was truly wonderful." A great variety of ropes were made to suit special requirements; flat plaited ones to put over the shoulders in carrying loads, thick stranded ones for heavy work, and a peculiar fine cord bound round with a still finer one like the fourth string of a fiddle, used only for one purpose—to bind the under aprons of chiefs' daughters.

The Maoris made fishing nets of enormous size, 5 fathoms deep and two or three hundred fathoms in length. With these mackerel were caught, a fish which has since disappeared almost entirely from the waters round New Zealand. They also caught sharks and fresh-water eels, which were dried and preserved for future use; there was no mutton in those days. Bivalves and crayfish were also extracted, dried, and preserved. The rat was a great delicacy, once very plentiful; it is now extinct. The following is Mr. Colenso's description of its preparation;—"It was carefully singed and so denuded of its fur, then its bones were broken within the body, and extracted by the anus without breaking the skin; this done it was cooked in their earth-ovens, and being very fat, made choice plump morsels, somewhat resembling large sausages. The contents of its stomach (being a frugivorous animal) were also eaten." The Maories were fond of perfumes which they prepared from Hepaticæ, Hymenophyllum, and other plants; the choicest was got from the gum of the peculiar plant *taramea* (*Aciphylla colensoi*) with much ceremony. Interesting descriptions of the manufacture of black pigment for tattooing made by burning resins and catching the soot are given; the soot is mixed with fats, and must then be eaten by a starved dog, and the voided fæces gathered for use.

The extremely interesting and valuable paper by Mr. Chapman is replete with information of which only a brief abstract can be given. It is forty or fifty years since there was a regular manufacture of stone implements, and too little is known of the way in which these implements were made and used. So soon as the savage acquires a steel axe and a gun, his beautiful but ineffective stone weapon becomes useless; it is laid aside, and no more are made. In a few years all the elder savages are dead, and the younger ones have a very transient impression of the ways of their cannibal ancestry.

With small exception, the whole of the various kinds of *pounamu* or "greenstone" is found in a restricted locality on the west coast of the south island. It occurs in boulders in the deposits of gravel in the beds of the Rivers Taramakau and Arahura, and the boulders are also found on the beach at the mouth of the rivers, cast up by the sea. The location of the dyke or vein from which the boulders came is not known. Formerly the stone was rare and expensive, but since these river gravels have been worked and washed for gold, considerable quantities have been found, and it is now not worth more than

one shilling a pound. The ancient importance attached to this district and its greenstone is indicated by the Maori name Waipounamu, which has given its name to the whole of the island. Pounamu was one of the sons of the great Polynesian deity Tangaroa (Lord of the Ocean), who was the son of Rangi (Heaven) and Papa (Earth). The substance *pounamu* was formerly supposed to be generated inside a fish, a shark, and only to become hard on exposure to the air. It appears to have always occupied a prominent position in the mythology, and to have been intimately associated with the history of the Maoris.

The mode of working and fashioning the *pounamu*, or "greenstone," is variously described. The material was first cut into slabs of the form required; this was done by sawing with thin pieces of slate or other hard material, assisted by sand and water, first on one side then the other, until the required piece could be broken off. The smaller fragments were fashioned into ear pendants, by the women and children. "With pretty constant work, a man will get a slab into a rough triangular shape, and about 1½ inches thick, in a month, and, with the aid of some blocks of sharp, sandy-gritted limestone, will work down the faces and edges of it into proper shape in six weeks more. The most difficult part of the work is to drill the hole for the thong in the handle." For this, pieces of sharp flint are obtained, and set in the end of a split stick, being lashed in very neatly, with a stone weight on either side, and forming a sort of tee-totum drill; as one flint is used up, another is inserted in its place. The enthusiastic Maori carried his partly-manufactured *mere* about with him, and every halt was utilised for taking a rub at it. The Greenstone was probably worked at all the Maori villages; but some places have the appearance of having been especially engaged in the manufacture; in these, numerous fragments, unfinished objects, as well as finished and polished implements, are found. In one instance, immense numbers were dug up in making a garden. In another, most of the remains are found in *whares* or dwellings; the latter, buried in sand, are indicated by hearth-stones, below the level of which there is usually a receptacle under the floor, probably covered by a flax mat when inhabited. In this secret repository are found beautifully finished objects of greenstone, and, perhaps, pieces of hæmatite; the latter, ground and mixed with sharks' oil, was used to adorn the person of the ancient Maori.

The *mere* or axe was the most famous weapon of the Maoris; it was usually 13 to 15 inches in length, sharpened at one end, with a hole through the handle, through which was a strong thong of dogskin made into a running noose, through which the thumb would slip easily. It was carried in the belt. The *mere* was not used like an axe, there was too great danger of its being broken and the labour of years lost. The first contact of the fighting forces was with the *hani*, a sort of quarterstaff. As the fighting got closer the *mere* was

taken in hand. With the left hand the enemy's hair was grasped, and with the right the *mere* was plunged into the side of the head where the bones are weakest. It is recorded that Te Wherowhero, the father of the chief who afterwards became the Maori king, and is still so called, killed 250 prisoners of war at a sitting, smashing the head of each at a single blow. His son still has the *mere*. The weapons were held in high veneration, and were frequently buried with the chief. The *mere*, as well as other objects, were often held as symbolical of ownership of land. The title deeds of the famous Heretaunga Block, now worth three-quarters of a million, was a small *pounamu* pendant, now worn by a gentleman on his watch chain. In 1856, when the final negotiations were made which secured to England the northern part of the South Island—a district very highly prized by the Maoris as the scene of many hard-fought battles—Ropoama-te-One, after alluding to those wars, struck into the ground, at the feet of the Commissioner, Sir D. McLean, a greenstone axe, saying, "Now that we have for ever launched this land into the sea, we hereby make over to you, as lasting evidence of its surrender, this adze, named Paiwhenua, which we have always highly prized from having regained it in battle after it was used by our enemies to kill two of our most celebrated chiefs, Te Pehi and Pokaitara. Money vanishes and disappears, but this greenstone will endure as durable a witness of our act as the land itself which we have now, under the shining sun of this day, transferred to you for ever."

A large mass of detailed observations is recorded by the authors of the papers; sufficient has been said, however, to show the very interesting character and high value of their communications.

James W. Davis.

IV.

Some Recent Researches on Insect Anatomy.

IN a recent review of Lowne's work on the Anatomy of the Blow-fly in NATURAL SCIENCE (vol. i., p. 551), the question of the morphology of the mouth parts in the sucking insects was discussed.[1] Two noteworthy contributions to the subject have lately been made. Lêon (1) describes and figures from a photograph a rudimentary three-jointed labial palp on either side of the base of the rostrum of an undetermined hemipterous insect. It is clear from this that the labial palps do not form any part of the rostrum in the Hemiptera, and Léon supports Gerstfeldt's view that that organ represents the parts of the labium (second pair of maxillæ) except the palps.

Of the mouth-parts of the Diptera, Müggenburg (2) has lately written, describing in detail the proboscis in the remarkable parasitic group generally known as the Pupipara. These insects are distinguished from other Diptera by their metamorphosis, as far as the pupal stage, taking place within the body of the mother; and they were, on this account, long separated as a distinct sub-order. Brauer, however, considered them aberrant and degraded members of the group to which the house-fly and blow-fly belong; his view has been shared by most recent writers on the Diptera, and is supported by Müggenburg's researches. The latter observer describes in detail the mouth organs of *Melophagus* (the sheep-tick) and *Braula* (the bee-louse). In the former insect the proboscis is elongated (Fig. 1) as it is also in *Hippobosca* (the horse-fly), *Anapera* (the bird-louse), and *Lipoptena* (the deer-fly), which all belong to the same family. In *Braula* (Figs. 2, 3) and also in *Nycteribia* (the bat-louse) the mouth-parts are much shortened. In no case are mandibles present. The maxillæ (which it will be remembered are believed by Lowne to form the larger part of the proboscis in the Diptera) are, according to Müggenburg, in the Pupipara represented by two elongated structures (Fig. 1, mx^1), situated within the head capsule, which, moved by a complicated system of muscles, assist in protruding or withdrawing the proboscis to whose base their distal extremities are applied. Maxillary palps are present in both

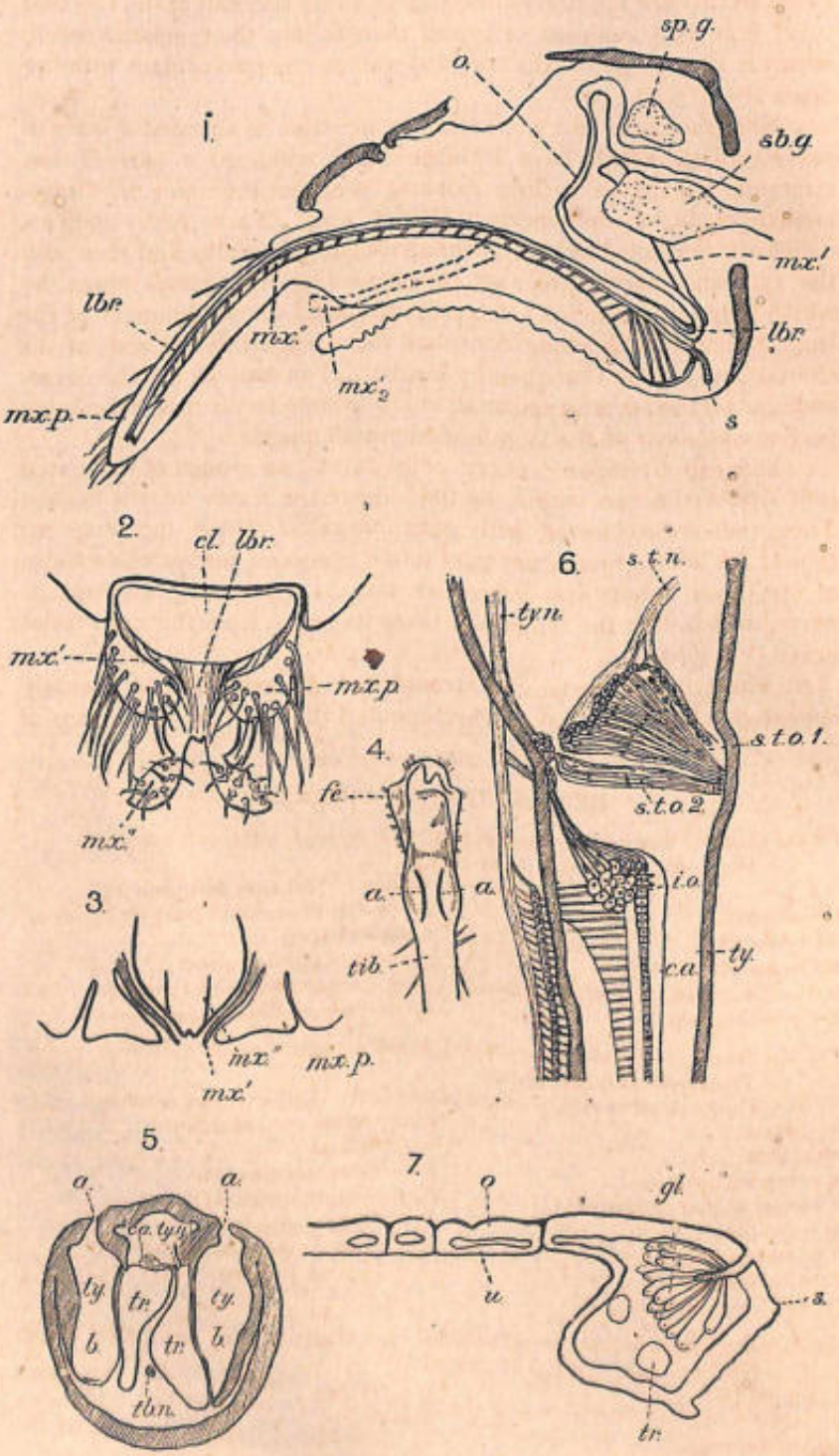
[1] In the last number of the *Ann. Mag. Nat. Hist.* (6), vol. xi., p. 45, Mr. C. O. Waterhouse also criticises Professor Lowne's views on the structure of the proboscis in the Diptera.

the Hippoboscidæ and in *Braula* and *Nycteribia*, elongated in the former and short in the latter group. Müggenburg supports the generally received view that the true proboscis is formed by the labrum, hypopharynx, and second pair of maxillæ (labium). In the Hippoboscidæ this organ is long and flexible; in *Braula* and *Nycteribia* it is much shortened, and the formation of its ventral (labial) part by the fusion of a pair of jaws seems very clear (Fig. 2, mx^{II}). In the latter insects, also, the maxillæ and their palps are connected at their bases; the mouth-parts have altogether a more typical aspect than in Diptera generally; and it would seem that, in the course of the degradation consequent on their parasitic habit, the jaws of these insects have reverted in some degree towards a primitive form.

Our knowledge of the ears of insects has lately received another contribution. Organs of hearing have not been recognised in many groups, but they have long been known in the jumping Orthoptera, the insects included under the popular terms grasshoppers and locusts. In the short-horned family of these insects, generally known as the Acrididæ, but henceforth in accordance with the law of priority to be styled Locustidæ,[2] the organs of hearing are paired, and situated on either side of the first abdominal segment. In the long-horned group, which is called Locustidæ by most entomologists, but which we must now learn to know as Phasgonuridæ, the ears are placed in the upper part of the tibial joints of the front pair of legs. The tibia is swollen just below the knee, and two slightly curved slits can be seen running longitudinally along the joint for a short distance (Figs. 4, 5). These are the openings into the outer auditory chambers. An account of these remarkable ears has lately been published by Von Adelung (3) who has studied their structure in the genera *Phasgonura* (*Locusta*, auct.), *Decticus*, *Thamnotrizon*, and *Meconema*.

The slits mentioned above open into chambers formed by the in-pushing of the integument of the leg. These chambers (Fig. 5) are thick-walled outwardly, but their inner walls are thin, and form the two oval tympana or drums, each of which is in contact with the wall of a trachea, or breathing-tube, the tracheal stem dividing in this region into two parallel branches, whose walls, however, remain in contact. The interior of these air-vessels functions as an internal auditory chamber. Along the outer wall of one of the air-vessels runs a ridge of tissue—the crista acustica—in which are arranged in linear series the cone-shaped endings of the nerve-fibres, each capped by a large cover-cell. These nerve-endings and their cover-cells become successively smaller from top to bottom of the series, which stretches parallel to and about as long as the longer axis of the drum.

[2] This change is less objectionable than many recent revolutions in nomenclature, as the true locusts, which belong to this family, will henceforth be called Locustidæ. Under the former arrangement they were excluded from the family named after them.



INSECT ANATOMY.

From each cone the nerve-fibre passes along the wall of the tracheal tube, first to a ganglion cell, and then to join the tympanal nerve, which is situated where the tracheal wall comes into contact with the drum (Figs. 5, 6).

Near the upper end of the crista acustica is situated a mass of nerve-endings which have hitherto been reckoned a part of that structure, but for which Von Adelung proposes the name of "intermediate organ" (zwischenorgan) (Fig. 6, i.o.). These nerve-endings, which are connected by nerve-fibres with ganglion-cells, and then with the tympanal nerve, are also in connection with tissue-fibres, by which they are stretched between portions of the integument of the leg. Hence, Von Adelung considers them related to the rods of the chordotonal organs described by Gruber (4) as well as to the nerve-endings of the supra-tympanal organ which forms the remaining portion of the ear of the long-horned grasshoppers.

This supra-tympanal organ consists of two groups of elongated rods stretched across within the tibia above the region of the drums. These rods are connected with ganglion cells. From the ganglion-cells of the lower group fibres pass to the tympanal nerve, while those of the upper group are connected with a special supra-tympanal nerve, which, like the tympanal, takes its origin from the main tibial nerve (Fig. 6, s.t.o.).

An interesting paper on the structure of insect-wings has recently appeared. Hoffbauer (5) nas investigated these organs by means of

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EXPLANATION OF FIGURES.

Fig. 1.—Longitudinal section through head of *Melophagus* (proboscis retracted).
2.—Mouth-parts of *Braula* (front view).
3.— Do. (horizontal section.) [All after Müggenburg.]

cl.—clypens.
lbr.—labrum.
$mx.^1$—maxillæ (first pair).
$mx.^1{}_g$—Do., when proboscis is exserted.
mx. p.—Do., palp.
$mx.^{II}$—maxillæ (second pair) (or labium).
o.—œsophagus.
s.—duct of salivary gland.
sp. g.—super-œsophageal ganglion.
sb. g.—sub-œsophageal ganglion.

Fig. 4.—Sketch of tibia of a Phasgonurid, to show position of ear apertures.
5.—Transverse section of tibia.
6.—Longitudinal section, showing parts of ear. [All after von Adelung.]

fe.—femur.
tib.—tibia.
a.—external apertures.
b.—outer auditory chambers.
ty.—tympana.
c. a.—crista acustica.
tb. n.—tibial nerve.
ty. n.—tympanal nerve.
s.t.n.—supra-tympanal nerve.
tr.—tracheæ.
i. o.—intermediate organ.
$s.t.o._1$—supra-tympanal organ (upper division).
$s.t.o._2$—supra-tympanal organ (lower division).

Fig. 7.—Transverse section through sutural edge of elytron of a Chrysomelid beetle (*Lina populi*). [After Hoffbauer.]

o.—upper surface.
u.—under do.
s.—sutural edge.
tr.—trachea.
gl.—glands.

transverse and longitudinal sections. It has long been known that the wings of insects are formed by the outgrowth of a fold in the thoracic integument, and that they therefore consist of a double membrane. Sections show the membranes lying apposed at most parts of the wing-area; but when the section passes through a nervure the membranes are separated to enclose a tubular space, in which are an air-vessel, blood-passages, and fat body.

It is not, however, to membranous wings, such as those of flies, bees, dragon-flies, or the hind wings of beetles that Hoffbauer has mainly directed his researches, but to the horny front wings of the latter insects, the wing-cases or elytra as they are generally called. Here the lamellæ become thick and chitinous; the upper lamella is sharply folded inwards in places, forming striæ above and making transverse supports between the two surfaces of the elytron. Along the sutural border of the elytron, the lamella forms a tubular space within which are numerous glands which occur in groups leading into common ducts, which open in several series along the suture (Fig. 7). In some cases the glands occur beneath the disc of the elytra, over the surface of which the ducts then open.

To most readers, the question of the homologies of the elytra of beetles as discussed by Hoffbauer will be of interest. They are almost universally accepted as corresponding to the front pair of wings in other insects. Meinert and others have, however, suggested their homology with the tegulæ of Hymenoptera, small scale-like processes in front of the fore-wings, and have imagined the atrophy of the true fore-wings in beetles. Hoffbauer notes a correspondence in structure between the elytra and the sides of the pronotum, and suggests their origin as lateral outgrowths of a thoracic tergum of which the scutellum represents the central part. But this does not contradict the generally-received view that they are modified fore-wings, since the origin of all insect-wings as outgrowths of the thoracic terga is admitted. The presence of two pairs of wings, always connected with the two hinder thoracic segments, throughout all groups of living and extinct insects, is certainly strong evidence in favour of their homology throughout the class; and in the various groups of the Rhynchota we can find a series which goes far to bridge the structural gap between the wing of a fly and the elytron of a beetle.

The wings of insects are studied from another point of view by Spuler, who has given (6) an account of their neuration in various groups of Lepidoptera. He believes that in the most primitive type of neuration the fore- and hind-wings have a similar arrangement of nervures. This occurs in the Neuroptera, and, among the Lepidoptera examined by him, in the genera *Hepialus* and *Micropteryx*, which must therefore be regarded as primitive. The tendency in most groups of the Lepidoptera is towards a reduction in the nervures of the hind-wings. In the development of the individual this rule seems to be followed; the nervures of the hind-wing in the pupa are more

numerous than in the butterfly or moth. Also an arrangement of parallel or forked nervures in the pupa becomes in the imago a neuration with transverse branches forming "cells." Spuler divides the insect-wing into two regions, a smaller "folding part," comprising two or three nervures near the inner margin, and a larger "spreading-part," comprising the rest of the wing-area.

In a later paper (7) he applies these results to a special group, the Papilionidæ. The genus *Thais* is believed to represent the ancestral form of the family whence the *Parnassii* have diverged in one direction and the various species of *Papilio* in the other. A "genealogical tree" is given, showing the supposed relationships of these to each other and to their ancestral stock. Those who have experience of such "trees," will not be surprised that the lines of descent often differ from those indicated by Eimer in his well-known work (8). Nevertheless, the object of natural history research is to discover the natural relationship between living creatures, and such speculations, though they can never be final, are full of interest. The arrangement of the nervures on a butterfly's wing becomes a subject of real importance in the light of the doctrine of descent.

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V.

Parasites on Algæ.

LIKE most other subjects that take their turn in popularity as fields of research, this one is by no means so new as may readily be thought. There are few things in Nature that our "rude forefathers," with their ruder appliances, did not grapple with after some fashion. The earliest mention of any fact that may be classed under this heading is probably the discovery of the so-called "galls" in the well-known siphoneous Alga *Vaucheria*. They were described and figured by Vaucher (1) so long ago as 1803, and many observers since then have added to the list of species infested, and have described the parasites and their operations. Of these writers, Balbiani (2) has given the most exhaustive account. The parasite in this case is an animal, and is described in the earlier papers as *Cyclops lupula*, but Balbiani and others refer it more accurately to *Notommata werneckii*. In a paper on the subject by Professor Oliver (3), the animal is given on the authority of the late Mr. Gosse as probably *Rotifer vulgaris*. The parasite enters a lateral fertile branch at an early stage, and sets up hypertrophy, causing it to swell to four or five times its original size. These galls have been frequently observed, as has been said, and an extensive literature has arisen on the subject, which will be found fully cited by Mr. A. W. Bennett (4), who gives from Benkö the following list of species on which they have been observed:—*V. racemosa*, *dichotoma*, *clavata*, *caespitosa*, *geminata*, *uncinata*, and *terrestris*. Lister observed them on *V. aversa* and *V. dillwyni*.

It is extremely probable that a considerable number of animals make use of Algæ as their hosts, as they do of land plants; but botanical literature, so far as I can discover, contains remarkably little about it. Miss Barton (5) has described malformations of the thallus of the common dulse, *Rhodymenia palmata*, caused by a copepod, *Harpacticus chelifer*, which inhabits the tissues and burrows in them during a stage of its existence. The same enthusiastic phycologist (6) has recently described two other similar cases, viz., gall-like structures on *Desmarestia aculeata*, also caused by a copepod (too immature for determination), and very remarkable malformations of *Ascophyllum nodosum*, caused by a nematode worm, *Tylenchus fucicola*. This worm, nearly related to the well-known "wheat-eels," has been minutely described and beautifully figured by Dr. de Man (7).

With the discovery by Alexander Braun (8) of a Chytridian parasite on the fresh-water Alga *Hydrodictyon*, and the publication, in 1855, in the paper quoted, of some twenty species of *Chytridium*, many of them inhabiting fresh-water Algæ, a new field of research was opened to cryptogamists. In the same year Bail (9) extended our knowledge of the subject, and Cienkowski (10), a little later, described a *Rhizidium* parasitic on *Conferva glomerata*. Cohn (11), in a remarkable paper on the physiology of the Florideæ, pointed out, in 1867, that *Chytridia* had been mistaken for fruits in certain marine Algæ, and thus extended the domain of this group to the sea. He was soon followed by Magnus (12), who made known other forms, of which the following may be looked for by students of our native Algæ, viz., *Chytridium tumefaciens*, in species of *Ceramium*; *C. sphacelarum*, in *Cladostephus spongiosus*; and *Sphacelaria cirrhosa* and *C. plumulæ*, in *Callithamnion*. Kny (13) next described another form—*C. olla*—in the fresh-water *Œdogonium rivulare*, and was succeeded by Nowakowski (14) and Professor Perceval Wright (15) with further records. In the meantime Pfitzer (16) described an interesting, novel parasite on *Closterium*, viz., *Ancylistes closterii*, which is of such singular character that it is commonly reckoned by itself as the type of a group, the Ancylisteæ, unless one puts with it *Lagenidium*, of which Zopf (17) described a form inhabiting *Spirogyra*, though there is now a disposition to include these forms under Chytridiaceæ. These Chytridiaceæ are a group of Fungi of aberrant type, and it is still debatable whether they form one natural family, or are better considered a convenient assemblage of forms with affinities in various directions among Fungi, or even with Protococcaceæ among Algæ. Both views claim a good deal of support, but much remains to be done in working out the life-histories. They are parasites in the tissues of marsh and aquatic plants, including aquatic Fungi such as *Saprolegnia*, and are reproduced by the production of swarm-spores in sporangial cells of definite shapes. These swarm-spores are provided, as a rule, with only one cilium, and they develop without well-marked intermediate stages into fresh sporanges. Some forms have resting spores which develop in similar fashion. They are all exceedingly minute, and easily find lodgment in the cells of their hosts, on which they produce both destructive and deforming effects. An excellent general account of their life-histories and operations will be found in De Bary's *Vergleichende Morphologie und Biologie der Pilze*, of which the Clarendon Press has published an English translation by Garnsey and Balfour, while the latest systematic accounts of them (both well illustrated) are by Alfred Fischer in *Rabenhorst's Kryptogamen-Flora*, pt. i., vol. iv., lieferungen 45 and 46 (1892), and by Schroeter in Engler and Prantl's *Naturl. Pflanzenfam.*, lieferung 76, p. 64. Not the least interesting circumstance about them is their occurrence in the sea, since it is well known that even such aquatic Fungi as the *Saprolegnia* of the salmon-disease find immersion in salt-water rapidly

fatal. A few other Fungi, such as certain parasites on *Zostera*, a marine flowering plant, have been recorded with some doubt so far as their actual growth in sea-water is concerned.

This parasitism of Fungus on Alga naturally suggests the case of the lichens, where, however, both classes of organisms dwell together in amity and their relations are symbiotic rather than parasitic.

Professor Perceval Wright (18) described in 1879, in an admirable paper, "a new species of parasitic green Algæ belonging to the genus *Chlorochytrium* of Cohn," which he found inhabiting the fronds of *Schizonema*, *Polysiphonia*, &c. A consideration of this form opens fresh ground, since it, like the type form of *Chlorochytrium*, inhabiting duck weed, described by Cohn (19) in an interesting paper on parasitic Algæ, does not appear to be a case of true parasitism, but rather of that sort called by Klebs "Raumparasitismus," in which the host furnishes merely lodging without board to the intruder. There are many such cases among the lower Algæ, and their bearing on symbiosis and on the parasitic habit itself is an instructive one. A large number of Algæ (especially marine forms) live as epiphytes on larger Algæ, and, indeed, constantly select the same species of host and the same part of its thallus. Many of these deserve special investigation, since cases occur in which the rhizoids of the epiphyte (and even wedges of its tissue, like haustoria) penetrate the host, and if not actual burglars are certainly in a very compromising position. We have an example of more than mere "Raumparasitismus" (we thank Klebs for that word) in *Phyllosiphon arisari*, a siphoneous Alga which lives in the intercellular spaces of the leaf of *Arisarum vulgare* and even consumes the chlorophyll in the adjacent cells. Other Algæ are known to inhabit the tissues of flowering plants, of *Azolla* and of Muscineæ, but the consideration of these is at present leading us away from the subject. *Chlorochytrium* (which may be put among the Protococcaceæ) furnishes us at any rate with a case of symbiosis, so far as shelter is concerned, between Algæ, and the line of investigation so successfully pursued by Professor Perceval Wright is one that phycologists may be confidently incited to follow.

Finally, and of special interest, are certain tubercles on the fronds of Florideæ described last year by Dr. Schmitz (20) as caused by Bacteria. The numerous cases of error in attributing the causes of diseases to Bacteria may make one unduly cautious in accepting statements of this kind when the possibility of the Bacteria being merely *post hoc* has not been absolutely excluded by the evidence of inoculation experiments. Dr. Schmitz, however, is not the man to make rash statements, and his research is a very noteworthy one, and one, moreover, that suggests other matters of interest in regard to Bacteria.

I am convinced that the subject of the parasites of Algæ—as well as that of parasitic Algæ—is but in its infancy, and in the hope that workers may be attracted to a field of research of great difficulty,

out with rich promise of reward, I have appended a list of papers which, while it does not claim to exhaust the literature by itself, will yet, with the books referred to in the text, guide the student to all that is known of the subject, and to collateral matters of interest.

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GEORGE MURRAY.

VI.

The Underground Waste of the Land.

GEOGRAPHERS and geologists are alike interested in the origin of the physical features of the land; but their sympathies become united only when we treat of the actual processes of sculpture. To the geologist the evolution of scenery is an exceedingly complex subject, for many of the features marked out in ancient epochs have been buried up by subsequent sediments and afterwards revealed by denudation. In the long and varied history of the subject the geographer is apt to manifest impatience, for he cares little about the age of the rocks, so long as he understands their relation to the form of the ground, and the influences that have contributed to produce the present shape of hill and dale, lake and river.

In this country the waste along our coasts, especially along the eastern and southern shores, is manifest; but when we study the inland features and find evidence of so many ancient earthworks, it would seem that the surface of the land has been but little modified during the past four or five thousand years.

The power of rain and rivers in wearing away the surface of the land is scarcely realised until statistics are presented to us of the amount of solid matter annually carried to sea by our rivers. From some areas in England and Wales as much as 150 (or even 200) tons per square mile may be removed each year, but the result is imperceptible, for it means a lowering of the general level of the land of about one-tenth of an inch in a century.

More conspicuous are the local evidences of destruction that may be seen in the occasional landslip and in the screes of fragmentary rock at the foot of crag and mountain. The material is dislodged by the mechanical action of frost and rain, and in some situations, as along the Pass of Brander, below Loch Awe, it is carried away by torrential streams, and the rounded fragments go to form beds of gravel. Again, after heavy rains, the turbid streams in the lowlands plainly indicate the kind of denudation that is taking place.

The loss of material that is carried away in solution is rarely made manifest, except in the case of caverns, and by sinkings of the ground in limestone-areas; or by the artificial removal of brine from the salt regions of Cheshire, whereby great subsidences have been caused. Even the Bath hot-springs, which do not contain a very

large amount of mineral ingredients compared with some other saline waters, daily carry away between three and four tons of solid matter, and the removal of this must cause cavities. Perhaps the landslips that have occurred from time to time at Bath may be to some extent influenced by subterranean movements, caused by the filling-in of cavities by the overlying strata. The waters of Burton-on-Trent may be cited in reference to chemical erosion, and it has been estimated that about 150 tons of gypsum are annually imbibed in potations of Burton beer.

In the more hilly and mountainous regions, where bare rocks frequently jut out at the surface, the evidences of destruction are apparent. Among the gentler hills and vales of the midland and southern counties, the evidences of erosion are to be judged mainly by the solid matter carried away by the streams. The matter held in solution is due chiefly to subterranean erosion; that held in suspension is due almost entirely to superficial erosion. The hills are, for the most part, composed of limestones, sands, and sandstones, through which the rain-waters may percolate, until arrested by a band of clay, when they issue as springs. Hence the hills are not so much subject to superficial denudation as are the vales, for these lie mainly in tracts of clay that are directly acted upon by the streams that flow across them. The main features of hills have, therefore, remained permanent for long ages, though their general level may have been continuously, if imperceptibly, lowered.

The question arises whether the underground erosion may not be partly mechanical, even if it be mainly chemical. Underground waters that flow on the top of a mass of clay must form channels in that material. The permanence of springs that issue here and there along the foot of escarpments, indicates plainly that the underground waters follow definite courses; and the actual outlet of such springs may be found at varying levels beneath the plane of division that separates the porous and impervious strata, because the springs have eroded their channels in places, perhaps, to depths of five or six feet.

Referring to the Central Himalayan region, Mr. C. L. Griesbach (4) mentions a limestone which rests conformably on calcareous shales, and this, like most limestones, is much jointed; consequently, all the drainage finds its way through the joints into the underlying shales. These become disintegrated, and are gradually carried away, while the thick limestone band sinks down to the level which was formerly occupied by the shales.

In an article in which I discussed the origin of the Scenery of Norfolk, I remarked that on the clayey strata the rainfall must accumulate or flow away at once towards lower levels; on the sandy and gravelly strata it will sink down until arrested by impervious beds beneath. Hence the earliest exposed channels no doubt commenced on the clayey areas that formed the surface. Beneath the

gravelly and sandy tracts, the rainfall formed subterranean courses, flowing, in some instances, even out to sea in that way, as we now witness in the case of numerous springs issuing from our cliffs on the coast. There deep channels or "chines" are in time formed, and the gravelly accumulations being ultimately cleared out, the clayey strata are exposed. In one case, I noticed a narrow gorge in the Contorted Drift, 9 feet deep and 2½ to 3 feet wide—like a miniature cañon—at the bottom of one of these chines. Thus streams flowing for a time underground, and carrying away material from below, will cause the surface to sink.

From these and other considerations we may be able to understand how some of our great sheets of gravel and sand, like those of the neighbourhood of Holt and Cromer, have become isolated hills, with ramifying spurs; for although some of the larger sheets of gravel may have been deposited in patches, yet the surrounding strata have been denuded, and they themselves have been broken up into smaller patches or outliers. When once this has taken place, the surface-features of these sandy and gravelly tracts may remain for long periods much the same, although the level of the whole may be slowly reduced by springs carrying away material from the lower portions of the strata at their junction with clayey beds beneath (3).

Subterranean erosion is suggested by the irregular channels that are often met with on the surfaces of clays that lie beneath Valley Gravel. On the Lias Clay at Bath, on the Oxford Clay at Oxford, and on the London Clay in the Thames Valley, we find such channels beneath the gravel; and in many instances the stones filling the channels lie with their longer axes more or less upright. These appearances have sometimes been attributed to the action of land-ice, or to the movements of thawing and slipping soil.[1] Without questioning that these explanations may be true in certain cases, I think the possibility of erosion by streams has often been overlooked.

On the Dorsetshire coast, between Lyme Regis and Bridport, gullies similar to those noted on the Norfolk coast may be seen in the Lias clays, and these are formed by springs that issue from the porous Cretaceous Beds above. Conybeare, in 1840, in his explanation of the great Landslip of Dowlands and Bindon, remarked that where the loose sands of the Upper Greensand rest on the Lias clays, and are exposed in the cliffs, "copious land springs will gush forth, and carry away in different seasons greater or less quantities of the loose material through which they flow; and thus, in process of time, the superincumbent rock will become partially undermined."

Some of the landslips on Portland, and the great gullies or fissures that affect the limestone-rocks, may probably be attributed

[1] *See* Letter of Darwin to Professor James Geikie (1876), *Life and Letters of Darwin*, ed. 2, 1887, vol. iii., p. 214.

to this undermining action of land-springs, and the consequent disruption of the strata.

Thus the level of the land may be lowered by springs carrying away sand, and also by their eroding channels in the clays over which they take an underground course. We may also discern a way in which outliers have been produced, many of which, like Glastonbury Tor and Brent Knoll in Somersetshire, appear to owe their preservation to a gentle, basin-shaped structure in the arrangement of their strata. Mr. T. V. Holmes has pointed out that some of the outliers of Bagshot Beds in Essex probably owe their existence to similar causes.

Minor instances of erosion beneath clay may sometimes be noticed. Thus in seasons of drought a superficial layer of clay may contain deep cracks, and bands of limestone three or four feet from the surface may bear evidence of erosion by carbonated water. Joints in limestone that have been widened into fissures by chemical erosion are often filled up with clayey material from beds above, and when the rocks are much fissured and eroded, small dislocations sometimes occur that produce a kind of faulting which does not affect the underlying strata. In these ways the level of the land is slightly lowered in many places.

Lyell (1) has referred to the depression of "submarine forests" by the drainage of peaty soils on the removal of a seaward barrier that formerly prevented the escape of the waters. His attention was drawn to the subject by observations published by the Rev. Dr. Fleming; and Lyell applied the explanation to a case at Bournemouth. There "the sea, in its progressive encroachments, eventually laid bare, at low water, the foundations of this marshy ground; in this case, much of the sand, of which these foundations were composed, might have been washed out by the rapid descent of the fresh water through them, at the fall of the tide."

John Cunningham (2), as pointed out by Mr. G. H. Morton (6), gave a somewhat similar explanation of the submarine forest of Leasowe; and very recently Mr. William Shone (5) has drawn attention to the subject in a paper referring to the same district. It must be confessed that these suggestions have yet to be substantiated.

Nevertheless, the subject of subterranean erosion by mechanical agency is one to which more attention may profitably be directed. The observations here recorded tend to show that the amount of erosion, whether mechanical or chemical, great as it must be, is hardly perceptible except locally. Our main features are of great antiquity, and, although the present forms of escarpments are due to subaërial agents, yet but a small amount of rock (a mere "notch") has been removed in this way compared with original extent of the strata. Into that particular subject it would, however, be undesirable now to enter, my object being simply to draw further attention to the question of underground erosion; and to point out how it is

that the general level of the land may be lowered by the material removed in solution and in suspension by springs, streams, and rivers, while the surface-features of hills and plateaux exhibit so little change.

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HORACE B. WOODWARD.

VII.

Owen.

(*Concluded from page* 30.)

IV.—SIR RICHARD OWEN'S RESEARCHES ON THE VERTEBRATA.

SIR RICHARD OWEN'S contributions to knowledge of the vertebrate animals, both living and extinct, are so vast, and relate to so many details of anatomy and zoology, that it is impossible in a brief notice to do more than survey the broad features of his work. He was placed, it is true, amid facilities for research that have perhaps not been equalled either before or since; he not only benefited by the labours of John Hunter, and had the entire collection of the Royal College of Surgeons at his disposal, but for a long period was favoured with the almost exclusive right of dissecting the various animals dying in the Zoological Gardens. Nevertheless, none but an enthusiast gifted with Owen's great intellect and indomitable perseverance could have taken the full advantage of these facilities; and the mass of new facts and detailed observations in Comparative Anatomy and Zoology recorded in the Hunterian Professor's publications, exceeds even the work of Cuvier himself. His various catalogues of the collections in the Royal College of Surgeons, his three volumes on the *Comparative Anatomy and Physiology of Vertebrates*, and his numerous memoirs published by the Royal, Linnean, and Zoological Societies of London—all must be tested to be appreciated; and we fear there are few naturalists of the newest school who refer to these old classics to the extent that they deserve.

Owen's detailed memoirs and descriptions, however, require laborious attention in reading on account of their nomenclature and modes of expression; and there is, perhaps, some reason that those now engaged in research should confine their attention to works of reference in more familiar language. At the same time, it must be remembered that Owen was the pioneer in a concise anatomical nomenclature; that many of our most familiar terms of everyday use are due to him; and that if the "laws of priority" are ever enforced as regards anatomical terms, his influence on accepted nomenclature will become still more conspicuous. It is, indeed, to be regretted that Owen could never be induced to follow, at least to some extent, the new school of anatomy and zoology that arose with the epoch-

making researches of Von Baer and Rathke in embryology; and it is precisely that unwillingness to depart from the philosophical ideas of his earlier life that has led to Owen's temporary eclipse by the present generation.

This marked disregard of embryology as the essential adjunct, even if not the key, of comparative anatomy, is all the more surprising, since so large a proportion of Owen's researches on vertebrate animals were devoted to the fossil remains of past ages. If any phase of biological research can benefit by embryology, that is assuredly palæontology; and it is strange to have to record that not only did Owen fail to appreciate this fact, but that he absolutely ignored some of the most striking memoirs in which an attempt is made to utilise modern methods, and discover the successive stages through which any particular type of animal has passed during geological time. His statements on the succession of genera and species, and their possible derivation one from another, were always vague, and capable of more than one interpretation; and though there is not much doubt he leaned towards the views of Geoffroy St. Hiliare, and those who believed in the evolution of life, his work, for the most part, is eminently Cuvierian—a laborious description of the facts, with a detailed discussion that rarely extends beyond strict comparative anatomy and the phenomena of geographical or geological distribution. Only on two occasions[1] does he appear to have attempted any broad philosophical deductions, and, even in those cases, it is not quite clear how much he admits. He was perfectly well aware that the facts of progression noticed by the anti-evolutionist Agassiz among fishes were equally conspicuous among the higher vertebrates[2]; but he contented himself with the bare statement that "the inductive demonstration of the nature and mode of operation" of the laws governing life would "henceforth be the great aim of the philosophical naturalist."

Owen, in fact, was Cuvier's direct successor, and apart from his striking hypotheses to which Dr. Mivart has referred,[3] it is in this character that he has left the deepest impression upon biological science. Extending and elaborating comparative anatomy as understood by Cuvier, Owen concentrated his efforts on utilising the results for the interpretation of the fossil remains—even isolated bones and teeth—of extinct animals. He never hesitated to deal with the most fragmentary evidence,[4] having complete faith in the principles established by Cuvier; and it is particularly interesting in the light of present knowledge to study the long series of successes and failures that characterise his work. However unwittingly, Owen may

[1] References to horse in "Anat. and Physiol. Vert.," vol. iii., p. 791 (1868), and to crocodiles in *Quart. Journ. Geol. Soc.*, vol. xl., p. 157 (1884).

[2] "Palæontology," ed. 2, p. 444 (1861).

[3] NATURAL SCIENCE, vol. ii., p. 18.

[4] Cf. *Anthrakerpeton crassosteum*, Owen, *Geol. Mag.*, vol. ii., p. 6, pls. i., ii. (1865).

be said to have contributed most to the demolition of the narrow Cuvierian views; when dealing with animals closely related to those now living, his correctness of interpretation was usually assured—when treating of more remote types he could do little more than guess, unless tolerably complete skeletons happened to be at his disposal. The fragmentary thigh-bone of *Dinornis*, brought to him in 1839, sufficed for the great anatomist to demonstrate that gigantic flightless birds had once existed in New Zealand; and the single fragment of the end of the lower jaw of *Diprotodon* from New South Wales was enough to enable him to conceive the former existence of that huge wombat-like animal in Australia. When, however, the molar teeth and femur of *Diprotodon* were first discovered, Owen failed to recognise their relationships and described them as evidence of a "Dinotherioid or Mastodontoid pachyderm" in Australia; and the restoration of the "great horned lizard" of Queensland (*Megalania*) has proved even more unfortunate, the backbone only belonging to a lizard, the head and tail to a tortoise, and the toes to a marsupial quadruped. The supposed tooth of a monkey from the Eocene of Woodbridge soon proved to belong to a primitive hoofed animal; and there is no longer any doubt that the comparison of *Stereognathus*, from the Oolites, with the Ungulata is based upon a complete misapprehension. In short, Owen's work on fragmentary fossils has demonstrated that the principles of comparative anatomy are very different from those inferred by Cuvier from his limited field of observation; and the discoveries of Leidy, Marsh, Cope, Scott, and Osborn in America have finally led to a new era that Owen only began to foresee clearly in his later days.

Throughout this work, one of the most striking features was the persistence with which Owen followed each subject until he had exhausted all available material. Impressed, in the earlier part of his career, with the researches of Retzius and others on the minute structure of teeth, he discussed and extended these observations until he had studied in detail the teeth of every known division of the vertebrates, living and extinct; and thus was produced his unrivalled work on *Odontography* (1840-45). He began his researches on British fossil mammals, birds, and reptiles, also, by contributing exhaustive summaries of all known material to the Reports of the British Association; and, although his results were usually published in small instalments in scientific serials, he nearly always arranged that the various groups of papers should follow in connected sequence, and in many cases he had them reprinted in the form of successive sheets of separate works, which appeared as *Researches on the Fossil Remains of the Extinct Mammals of Australia, with a notice of the Extinct Marsupials of England* (2 vols., 1877), *Memoirs on the Extinct Wingless Birds of New Zealand* (2 vols., 1879), and *A History of British Fossil Reptiles* (1849-84). This method is convenient for purposes of reference, but it naturally led to continual disagreement between the author of the memoirs and

the councils of the societies from whose publications they were extracted.

To refer now more particularly to some of Owen's principal contributions to knowledge of the vertebrata, we have first to allude to the fishes, which did not attract so much notice as the higher groups. Besides his detailed descriptions in the Hunterian lectures and catalogues, however, there are some of Owen's writings that will ever remain memorable. His description of the African mud-fish *Protopterus*, for example, laid the foundations for the recognition of the Dipnoi by Müller, and it is interesting to note that Owen emphasised the resemblance between the teeth of this fish and the fossils named *Ceratodus* long before anything was known of the affinities of the latter. He perceived the direct serial connection between the teleostean and ganoid fishes, and grouped them in one sub-class, the Teleostomi. Among fossils, too, he made several advances, and to him we owe the first information of the complex structure of the teeth of the Holoptychian fishes named *Dendrodus*—a structure, as Owen pointed out, only paralleled in the terrestrial extinct Labyrinthodonts.

The discovery of the remarkably complex structure of the last-mentioned teeth, which Owen referred to a group of animals termed Labyrinthodonts, was one of his earliest contributions to knowledge of the extinct cold-blooded air-breathers. On discussing some portions of the skeleton later, he concluded that these animals were probably amphibia, and it is only quite lately that much evidence has been brought forward to indicate their higher rank. At the same time, Owen can scarcely be held responsible for the great frog-like "restoration of *Labyrinthodon*" that is commonly ascribed to him; he merely interpreted the fragmentary bones as best he could, and the monstrosity just referred to was the unjustifiable work of a "populariser" of scientific investigation.

Among undoubted reptiles, Owen's first important triumph was his recognition of the great group of Mesozoic land-reptiles, to which he gave the now-familiar name of Dinosauria. Hermann von Meyer, it is true, had already expressed some vague ideas on the subject of what he afterwards termed the "Pachypoda"; but Owen, with the assistance of Mantell and Buckland, was the first to arrive at so much precision as could be attained in those days, and he continued to make the principal contributions to our knowledge of Dinosaurs for a period of nearly 40 years; describing *Iguanodon*, *Hylæosaurus*, *Cetiosaurus*, *Omosaurus*, *Scelidosaurus*, *Megalosaurus*, and more or less fragmentary remains of other genera. Even to the end, however, Owen failed to appreciate some of the fundamental characters of these animals,[5] and there is now not much doubt (reasoning from

[5] *Cf.* Restoration of skull of *Megalosaurus* in *Quart. Journ. Geol. Soc.*, vol. xxxix., p 340 (1883).

discoveries of complete skeletons in America) that they belong to more than one order of the reptilian class.

The Anomodontia—those curious early Mesozoic reptiles combining in their skeleton certain features now only known in amphibia and mammalia—were also first recognised and defined by Owen, who described a great number of forms from the Karoo deposits of Cape Colony, beginning with *Dicynodon* in 1845, culminating in his exhaustive *Catalogue of the Fossil Reptilia of South Africa* (British Museum, 1876), and even further pursued in later papers. The discovery of *Rhynchosaurus* in the English Keuper was also the first evidence of the order recognised by later observers as the Rhynchocephalia. Moreover, the most elaborate series of descriptions of the Ichthyosauria and Plesiosauria are due to Owen; and he has made known nearly all the principal British types of fossil Chelonia and Ophidia hitherto described. His memoir on *Dimorphodon* marked a great advance in knowledge of the Pterosaurian skeleton; and his discovery of the reptilian nature of *Placodus* and *Stagonolepis* (previously regarded as fishes) was also an important step.

Among contributions to the anatomy of birds, we may specially refer to Owen's classical memoir on the apteryx, and his further notices of the anatomical characters of the flamingo, pelican, gannet, and hornbill, all published by the Zoological Society. His important series of memoirs on the osteology of extinct birds, however, issued by the same society, excel even his work on living forms, and we need only recall his descriptions of the dodo and great auk, besides his numerous memoirs on the Dinornithidæ, on *Aptornis*, and on *Notornis* from New Zealand. From the palæontological standpoint, too, Owen's monograph on *Archæopteryx*—the long-tailed, toothed bird from the Bavarian Lithographic Stone—is an epoch-making work. Finally, he described *Odontopteryx*, *Argillornis*, and *Dasornis* from the London Clay.

Among recent mammalia, the more striking of Owen's contributions relate to the monotremes and marsupials, and the recognition of the two natural groups of typical Ungulata—the odd-toed (Perissodactyla) and the even-toed (Artiodactyla); and mention ought also to be made of his studies of the various apes. His attention seems to have been first directed to the extinct mammalia when he undertook the description of the fossils collected by Darwin in South America during the voyage of the "Beagle." *Toxodon* was then described, and thus provided the first clear evidence of an extinct generalised hoofed animal; according to Owen, indeed, this was a "pachyderm, with affinities to the Rodentia, Edentata, and Herbivorous Cetacea." *Macrauchenia*, erroneously associated with the camels, was also made known, and there were descriptions of new gigantic sloths, *Mylodon* and *Scelidotherium*. Almost at the same time, too, Owen proved that *Megatherium* was not an armoured animal, but that the dermal plates often ascribed to it really per-

tained to a non-banded armadillo, to which he gave the name of *Glyptodon*.

While dealing with the South American extinct mammals, Owen was also occupied with bones and teeth from the caverns of New South Wales, discovered by Sir Thomas Mitchell, and thus he laid the foundation of the long series of memoirs on the extinct mammals of Australia, for which he obtained materials from Dr. George Bennett and many other indefatigable correspondents. Moreover, in the midst of this activity, he was collecting materials for an exhaustive work on the British fossil mammals, and his charming volume on *British Fossil Mammals and Birds* appeared in 1846.

Owen also took part in the discussion on the supposed mammalian jaws discovered in the Stonesfield Slate, which attracted the notice of Cuvier and many contemporaries from 1824 onwards; and it was Owen who gave the first extended account of the Purbeck Mammalia (collected by Mr. Beckles) in his *Monograph of the Fossil Mammalia of the Mesozoic Formations* (Palæont. Soc., 1871). The only known Lower Mesozoic mammalian skull was also described by Owen from the Karoo formation of Orange Free State in 1884 (*Tritylodon*). The Cetacean fossils from the Red Crag formed the subject of a brief *Monograph of the British Fossil Cetacea from the Red Crag* (Palæont. Soc., 1870); and it must not be forgotten that Owen was the first to recognise the affinities of the archaic *Zeuglodon*.

Limits of space forbid more. These are merely a few of the more salient features of Owen's contributions to our knowledge of the Vertebrata. We need only add that it is very largely owing to the prompt manner in which he interested himself in, and dealt with every new fossil submitted to him, that the British Museum and the Royal College of Surgeons are almost unique in their rich collections of Vertebrate Palæontology.

A. SMITH WOODWARD.

VIII.

The Restoration of Extinct Animals.[1]

AS the writer very truly remarks in the volume before us, the general public most certainly does not appreciate dry bones, more especially when they are found in the detached and frequently imperfect condition so common in the older formations of this country. With the laudable endeavour to excite a more general interest in the monsters of a former world, he has accordingly set himself the very difficult task of endeavouring to clothe them with skin and muscle, and to set their portraits, as thus restored, before the eyes of a wondering public. Needless to say, in such a task everything depends upon the skill of the artist and his knowledge of anatomy, and as we believe that Mr. Hutchinson is neither an artist nor an anatomist, such criticisms as we have to make on his restorations will apply chiefly to the draughtsman, who, we trust, is as pachydermatous, for the nonce, as the animals he portrays.

The idea of restoring extinct animals so as to present some semblance—we do not care to speculate how remote—to their original form, is no new one, dating at least as far back as Cuvier, by whom very fine restorations were made of the Palæotheres and Anoplotheres of the Paris Basin. Later on Buckland and Conybeare attempted the restoration of the Ichthyosaur and Plesiosaur, and, indeed, so successfully that their models now require but comparatively little modification to bring them up to date. In all these cases, as well as in that of the South American Megathere, the restorers had more or less nearly complete skeletons of the animals in question before them, and they, accordingly, could not well wander very far from the truth. The same remark will apply to the restorations which have been from time to time attempted of the various large mammals of the Pleistocene and Pliocene. When, however, we come to tell the tale of what has been previously done in the way of restoring the giant land reptiles of the Secondary epoch, we have nothing to record but disaster and confusion. This is due to the circumstance that it was formerly considered possible to restore an animal correctly from a single limb or bone. Now, however, we know better, and are fully

[1] "EXTINCT MONSTERS. A Popular Account of some of the Larger Forms of Ancient Animal Life." By Rev. H. N. Hutchinson. With Illustrations by J. Smit. London: Chapman and Hall, 1892. 8vo. Pp. xx. and 254. Price 12s.

aware that the powers of the anatomist—great as they undoubtedly are—are not equal to the task of making anything approaching a correct restoration from such materials.[2]

For instance, what sort of a creature would have been the result of attempting to restore the Macrothere from the foot-bones which were long supposed to be the only portions of the skeleton known, although its teeth and jaws lay all the time in the national collection—in a case half the length of the gallery from the one containing the former?

This overwhelming confidence in what the anatomist could be legitimately supposed to effect led to the grotesque so-called restorations of the Iguanodon and other Secondary reptiles set up many years ago at Sydenham, where, we believe, they still remain to delude an unsuspecting public. Needless to say, the creatures thus modelled in plaster were monsters in every sense of the word, and about as unlike anything that ever existed in heaven or earth as they well could be.

Now, however, *nous avons changé tout cela*; and all those who know anything at all about the subject are fully aware that it would be idle to think of restoring any monster of a totally extinct type without having, at least, its skull, the bones of both fore- and hind-limbs, and some portion—the more the better—of its backbone. In this country, and Europe generally, as a well-known palæontologist once remarked, we prefer (from necessity) to construct our Dinosaurs piecemeal—and nice little squabbles we sometimes get into over these aforesaid pieces; and it would accordingly, with one or two exceptions, have been long before we should have been justified in attempting their full restoration, even if this were ever possible. Fortunately, however, America has stepped into the gap, and given us Dinosaur skeletons by the dozen; so abundantly, indeed, they may almost be said to be a drug in the market. We have horned Dinosaurs, four-legged Dinosaurs, two-legged Dinosaurs, and mail-clad Dinosaurs, all and every one of which are creatures more like the phantoms of a dream than any we should naturally have thought of as denizens of this world of ours.

Hitherto we have known these marvellous creatures only by their skeletons; and although these are, perhaps, fully sufficient for the scientific student, yet it is quite clear that they do not arouse the enthusiasm of the public, by whom they are doubtless not "understanded." Now, however, thanks to our author and his artist, we have them depicted in at least some semblance of their original guise, the artist having taken the skeleton as his model in each case, and clothed it in flesh and skin as best he might.

When we take into consideration the extreme difficulty of this task, we cannot but think that the draughtsman has acquitted him-

[2] We were surprised to see the other day in the *Times* the statement that Sir R. Owen restored the Megathere from a single bone.

FIG. 1.—RESTORATION OF *Triceratops*

self well, and more than well; and any criticisms that we may have to make must be looked upon rather as suggestions than as indicating a carping spirit.

One of the most striking restorations in the book is that of the Horned Cretaceous Dinosaur forming the frontispiece, which we are enabled to reproduce (Fig. 1). Now, so far as regards the head and body, there is little room for criticism, but the case is very different when we come to the limbs. In the first place, we are led to ask why the artist took for his model—as we are fain to suppose he did—an Ungulate Mammal instead of a Crocodile in his restoration of the hind-limbs. That is to say, we wish to know why the upper segment of the hind-limb (femur) is included in the common integument of the body, instead of being, as in the Crocodile, free. We should, indeed, have thought it much safer to follow the crocodilian model in this respect. Then, again, we have greatest doubts as to whether the creature ever had the prominent Ungulate-like heel with which it is represented, as, on turning to the figure of its skeleton on page 166, we find there is no backward projection of the calcaneum, so that here also the departure from the crocodilian model seems unjustifiable. In the fore-limb it is, further, quite evident that the joint which appears intended to represent the wrist is placed much too high up; and we also doubt if any reptile could possibly have had the "action" in the fore-limb which the forward bend of this joint on the right side is clearly intended to represent. In all these points the want of an anatomical knowledge on the part of the author wherewith to check the exuberant fancy of his artist is only too apparent; and the former would certainly have been well advised had he submitted the sketches to some person well versed in the osteology of reptiles before having them photographed.

We may remark here that the author calls the reptile in question by the name *Triceratops*, but he should have been aware that this is certainly not its proper title; and we notice all through the book a lamentable want of care in this respect, the names assigned by one particular palæontologist to the animals figured being taken without the least enquiry as to whether they are correct. We should have thought, moreover, that in a popular work the insertion of specific names in the case of these giant reptiles was perfectly unnecessary, and only too likely to make it repellent to the unscientific public. Then, again, the number of generic terms introduced into the text is, to our fancy, far too numerous—more especially when many of them are probably synonyms. For instance, it would have been far better to allude to the skull figured on page 79 as that of a Carnivorous Dinosaur, rather than as *Ceratosaurus*, seeing that both Professors Cope and Baur are confident that it belongs to *Megalosaurus*. The author is also, in some cases, somewhat careless as to sources from which he derives his figures, and we may remind him that the skeleton figured on page 75 is *not* after Marsh.

While on the subject of Dinosaurs, we must take exception to the author's throwing any doubt (page 62) on the relationship existing between these reptiles and birds, which we venture to consider one of the best established zoological facts. Few, however, at the present day, would be disposed to assert that the Ratite birds (p. 63) —*qua* Ratites—are the direct descendants of Dinosaurs. The author is still more "flabby" with regard to the relationship between Dinosaurs and Crocodiles (p. 63), which is likewise a well-established fact; and if his own want of anatomical knowledge did not permit his perceiving their connection, he might at least have followed those who are entitled to speak with some authority on the subject. Instead of this, he goes out of his way to say that, while such connection may be "quite possible, and even probable," it cannot be considered certain.

To resume our survey of the plates, the restorations of the Brontosaur and Megalosaur (pls. iv., vi.) appear to us decidedly more successful than that of the Horned Dinosaur; although in the Megalosaur the fore-limbs are not quite satisfactory. In both these figures the objectionably prominent heel given to the Horned Dinosaur is wanting. As regards the Iguanodon (pl. vii.), it appears to us unnatural that, while the elbow-joint is represented as completely free from and below the body, the thigh is almost entirely enclosed in the integument; and we have also doubts whether the creature was really so short-legged as it is represented. When we contrast with it the figure of the Scelidosaur (pl. viii.), we are, indeed, surprised to find such a marked difference made between the two animals in these respects, and we are certainly not prepared to admit that, while the one was digitigrade, the other was plantigrade. Anyway, the different position of the elbow-joint in the two forms clearly shows that at least one of the restorations must be incorrect in this particular. With regard to the restoration of the armour in the Scelidosaur, we are aware that the artist has followed an attempted reconstruction of the skeleton, but, nevertheless, the huge spines on the shoulder-blades do not appear altogether natural. In putting the creature in an upright posture, we have, however, every reason to believe that the artist, in spite of certain criticism to the contrary, is fully justified. Judging from the skeleton of the so-called Stegosaur, we are inclined to think that in the restoration (pl. ix.) the hind-quarters and limbs do not give any adequate idea of their immense height and power as compared with the fore-parts.

Of the other Secondary reptiles, such as the Ptérodactyles, Plesiosaurs, Ichthyosaurs, and Mosasaurs, the restorations appear, on the whole, as satisfactory as the materials on which they are based will admit. It was, however, unfortunate that the specimen of an Ichthyosaur referred to in an appendix was not described in time to admit of its forming the basis for the plate of that group. In treating of the Plesiosaur on page 50, we think it might have been well to

modify the statement that it was so named "in order to distinguish it from the *Ichthyosaurus*, and to record the fact that it was more nearly allied to the lizard than the latter." What exact signification the author may attach to the term *the lizard*, we are, of course, quite unaware, but the sentence as it stands is certainly misleading to the uninstructed. Again, on page 58, we are totally at a loss to imagine what special resemblance the Plesiosaur presents to "the strange Ornithorhynchus" (with which term, of course, all the readers of the book would be perfectly familiar!). If the author had written Ichthyosaur instead of Plesiosaur, there might have been something in the statement.

Before leaving the subject of reptiles, we should like to know why the author retains the name *Colossochelys* for the giant extinct Indian Tortoise, seeing that it has been conclusively shown to be inseparable from *Testudo*; and we also think he would have been better advised had he made no mention of the grossly-exaggerated restoration of its shell which still disfigures the fossil reptile gallery of the British Museum. Indeed, the whole fable of the mythological Indian tortoise, which Falconer sought to identify with this fossil, might well have been consigned to the oblivion it merits.

Two plates are devoted to birds, the one representing the toothed *Hesperornis* of the North American Cretaceous, and the other two of the New Zealand Moas. In representing *Hesperornis* with a steganopodous foot, that is to say, with the hallux connected with the other toes by a web, the artist has certainly no justification, this feature being at variance with the colymbine affinities of the genus. Here, then, we again meet with an instance where the author's want of acquaintance with the most ordinary facts of zoology has rendered him unfit for the task of supervision. We do not, of course, mean to assert definitely that this bird may not have had all the toes joined by membrane; but since its osteology is so close to that of *Colymbus*, it is probable that the hallux was separate, and it should have been restored accordingly. That the author adopts the view that the extermination of the Moas took place during the period that man has inhabited New Zealand, is evident from his plate; we regret, however, to find that he has not seen fit to follow the view of all living authorities on the subject that the two species of these birds selected for illustration belong to widely different genera.

In regard to mammals, we have not much to remark. One of the most striking illustrations is that of the Glyptodont in plate xviii. (herewith reproduced), where the proper form of the tail is given. Readers of the work are, however, likely to be somewhat puzzled on finding another member of the same genus depicted on page 174 with a totally different tail; and in common fairness they ought to have been informed that in this case the tail belongs to a totally different animal from that to which the carapace pertained. We do not, more-

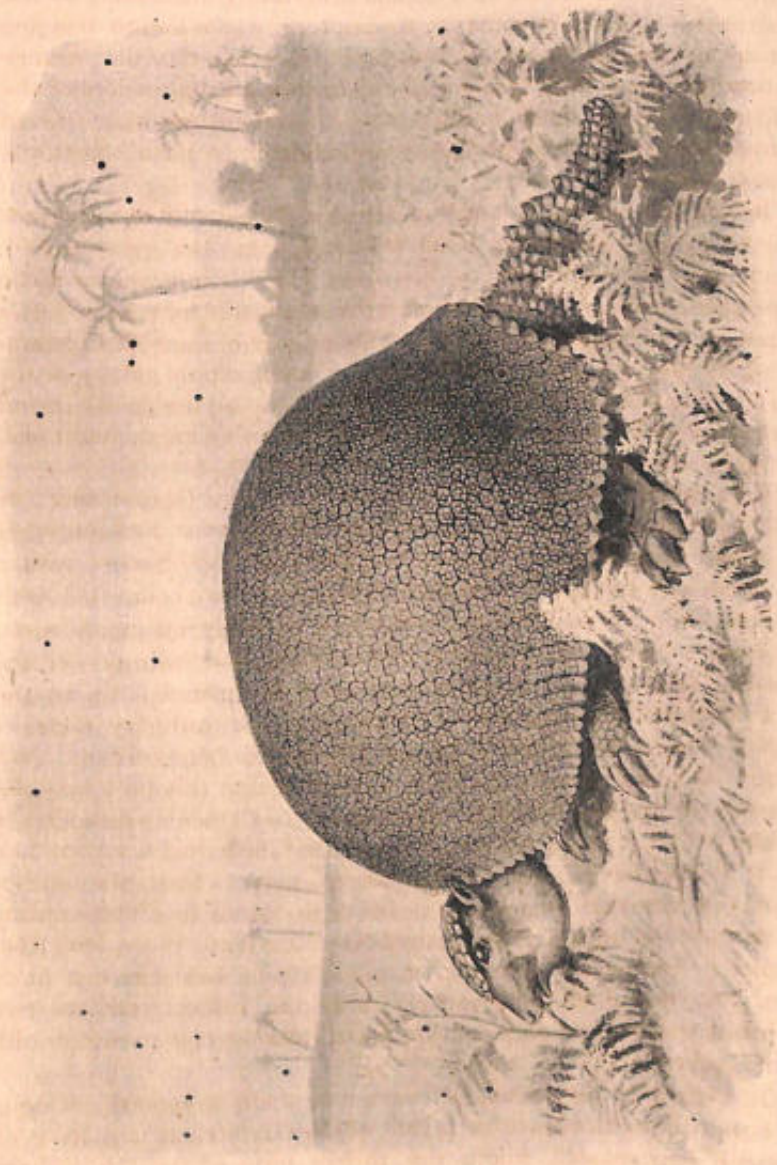


FIG. 2.—RESTORATION OF *Glyptodon*.

over, quite like the term armadillo applied to the Glyptodonts, especially as its original signification implies diminutive size; and surely, in face of the multitude of uncouth terms used by the author, there could have been no objection to the use of the name Glyptodont for these animals. The Mammoth is rightly restored on the lines of its near relative, the Indian Elephant, but surely the artist might have selected a better example of the latter animal as a model than the ill-bred, long-limbed, and small-bodied brute he has depicted. Why, for example, did he not take the figure of a "Koomeriah" Elephant from G. P. Sanderson's "Wild Beasts of India"? On page 199, the author enters into a comparison between the habits of the Mammoth and the African Elephant, and infers that the former uprooted fir-trees for food. He forgets, however, that the African and Indian Elephant have quite different teeth and quite different food, and that the Mammoth resembled the latter in the structure of its molars. Bearing in mind the great external difference between the Indian and African Elephant, of which no indications are given by their skeletons, it must be admitted that the restoration of the American Mastodon is a mere fancy sketch; and we quite fail to see the advantage of attempting the restoration of a member of any existing genus of animal unless its exact specific characters are, more or less, exactly known. The restoration of the Woolly Rhinoceros (which the author, as usual, calls by its wrong scientific name) appears to be rightly modelled, so far as the head is concerned, on the lines of the African Square-mouthed Rhinoceros. The artist has, however, made the beast "square-mouthed" with a vengeance, and has put the front horn too far back; and he would have done better if he had kept close to the figure of the head of the living species he drew a few years ago for the *Proc. Zool. Soc.* Moreover, the beast is too low at the withers, and has an altogether "sheepish" expression, which does not convey the proper idea of such a magnificent monster.

In the Indian Sivathere depicted in plate xvi., we note that the tips of the antlers are represented as curving forwards and inwards, whereas in the figure of the skull on the page facing the plate their direction is backwards and outwards. The restoration of the Uintathere (miscalled *Tinoceras*) in plate xiv., and that of the Titanothere (wrongly called *Brontops*)[3] in plate xv., appear to be fairly satisfactory, the feet of the one being formed on the Proboscidian, and those of the other on the Rhinocerotic model, while both have rhinoceros-like heads. Why both the plates should be lettered "new" extinct quadrupeds, we rather fail to see; and we had hoped the latter term had received its quietus.

[3] The author ought to be aware that, if he admits such terms as *Brontops* and *Tinoceras* to generic rank (if, indeed, they are not synonyms pure and simple), he has not the shadow of justification for including the Woolly Rhinoceros in the genus of that name.

On the whole, we think that, considering the difficulties of the task before them, both author and artist have produced a very creditable volume, and one which will not fail to arouse a considerable amount of popular interest. Should a new edition be called for, we think the author would do well to be somewhat less free in his use of uncouth and unfamiliar terms (*e.g.* heteroclite, p. 35) and technical names. Moreover, in many places his literary style and the conformation of his sentences might be altered for the better. For instance in a half-page paragraph of six sentences (p. 18) we find no less than three of these beginning with the word "But" and two with "In"; while the clumsiness of some of the sentences themselves would be rather hard to beat.

SOME NEW BOOKS.

INTRODUCTION TO PHYSIOLOGICAL PSYCHOLOGY. By Dr. Theodor Ziehen. Translated by C. C. Van Liew and Dr. Otto Beyer. 8vo. Pp. 284. London: Swan Sonnenschein & Co., 1892.

THE original German work, of which this is a translation, forms a useful introduction to the subject. It is short, it is written vivaciously, it is usually clear and well-arranged, always interesting; it is not a compilation, but the work of a man who has thought out the whole subject for himself. The doctrine is by no means free from exception, but it is set forth vigorously and consistently. In the present state of the subject, any short book is almost necessarily dogmatic in character. Perhaps the few polemical passages were better omitted altogether, as likely to create an unfair impression when not supported by proofs.

Professor Ziehen belongs to the small but able body of German psychologists who follow the model of the English psychology of association, and seek to improve upon their model. The latter half of the book attempts to show how associative processes account for the complexity of all mental phenomena above sensation. But the chief value of the work lies in the consistent and thoroughgoing attempt to exhibit in detail the physiological processes which underlie the psychical. Without this procedure, hypothetical as it is at present, we are unable to explain mental acts; and at the same time this method clears up many difficulties, such as those of unconscious mental states.

Professor Ziehen proceeds upon the hypothesis that ideas are deposited in different elements of the cortex from the corresponding sensations, though he is well aware that another hypothesis is possible. Unfortunately, in the very chapter on the association of ideas, his usual clearness deserts him. He does not explain clearly the relation of association by contiguity to association by similarity, and he is confused in dealing with the difficult question of whether the first kind of association is simultaneous or successive. This chapter is one of the less satisfactory parts of the book. His classification of actions into reflex, automatic (he uses this unhappy word in yet another sense) and actions proper, is well worked out, but it presents many difficulties. He has to class instincts along with reflex and automatic acts (*i.e.*, reflexes modified by intercurrent sensations—why sensations?) as purely unconscious. The first half of the book is an account of the various sensations, and of generalisations, like those of the specific energy of nerves and of Weber's law. A fuller account of colour-blindness and of Hering's hypothesis would be an advantage. The psychophysical methods are treated very inadequately, even making allowance for the difficulty of discussing them in a short book; and the treatment of the method of right and wrong cases (which the translators, following Professor Ladd, absurdly call the method of correct and false (mistaken) cases) is apt to be misleading. Still, on the whole, the selection made from the multitude of data is a good one for elementary purposes, and the statement is clear,

with one or two exceptions (*e.g.*, the notice of Cheselden's patient, on p. 81, and the account of Goldscheider's investigations of the sense of motion, p. 69).

A useful and able book like this, if it was to stimulate the growing interest taken in the subject in England, needed to be very well translated. Professor Ziehen has been unfortunate in his translators. They have obviously taken pains, but they do not appear well qualified for the task, either by complete mastery of the English language, or by close familiarity with the subject itself. They have made the initial mistake of abandoning the fluent, personal, lecture form of the original, for a buckram, awkward, impersonal form. The translation is, as a whole, clumsy and disfigured by inelegancies, and one is always reminded that the work is a German one. There are many strange words, such as taction, obtusion, appertinent, respective, literature, innerved, impeditive ("a very impeditive sheath of caoutchouc" for "a close-fitting indiarubber tube"), synchronically, sensual for sensuous (*e.g.*, "sensual vivacity"), to dampen for to damp. As Goldsmith at a famous dinner-party said of the phrase "happy rebellions"—we have not the phrase. The word "impart" is used freely; a stimulus "imparts" a sensation, one sensation even "imparts" another; a cortical centre "imparts" a motion—where produce or discharge is the accepted term. Magnitudes are "projected" on an axis of abscissas, instead of being "measured off." Very often the particles get wrong, and the order of words destroys the emphasis. On page 124, by translating "höchstens" by "at least" instead of "at most," they make the passage unintelligible, and many other such confusions occur (*e.g.*, on pages 215, 245, 44.).

More serious mistakes arise from unfamiliarity with the subject. Awkward or unusual names are used, gustatory bulbs (instead of taste-buds or taste-bulbs), zone of Rolando for the motor area, threshold of distinction (which should surely be "difference"), faradic electricity, synovial duplicatures, auditory bones. In some cases the translators improve on their author, and make him blunder; the description of chemical stimuli on p. 37 is absurd in the translation; on page 102 they blunder gratuitously over the connection of the hemispheres with the different sides of the field of view; in the same chapter the nodal point of the eye is described as the point at which the rays (*i.e.*, from a single point) intersect, not a word of which of course, is in the original. The "well-known blind patient of Cheselden" is actually rendered "the well-known Cheseldens, who was born blind." It is amusing to find, in a work intended for English readers, "the then youthful English statesman, Gladstone." The mistakes occur principally in the earlier, more physiological, half of the book; the later half is fairly readable, but it, too, contains both blunders and inelegancies. On the whole, the translation must be pronounced very unsatisfactory, and contrasts glaringly with the excellent translation of Höffding's "Outlines," which is a boon to the English student. It speaks much for Professor Ziehen that he is interesting in spite of his translators.

S. A.

Public Health Problems. By John F. J. Sykes, B.Sc., &c. [Contemporary Science Series.] London: Walter Scott, 1892. Price 3s. 6d.

The peculiarity of this book is, that the author, to use his own words, has attempted "to cast a reflective and suggestive line of thought

into the volume." With this idea before him, he deserts the ordinary empirical method of the text-books, and attempts to group the "Problems" and the Ideas which are prevalent in Biology at the present time. In the later part of the book, especially in Part III., where the author falls back on the Empirical method, and takes up, in this fashion, subjects such as "Quarantine," "Isolation," &c., we have him at his best. Here he discusses the subjects in an entirely practical manner, and does not attempt to justify his conclusions on other than practical grounds.

It is with the earlier part of the book that we are inclined to find fault. The conception of evolution has hitherto not been found of much service in the sciences of Physiology and Pathology. It is chiefly of value to Morphologists. To try to introduce it into Hygiene, which depends on Physiology and Pathology for the basis of its doctrines, is a much more serious matter than can be undertaken in a small text-book. The chapter on "Heredity," for example, is not very conclusive. The author tells us that in Pathological changes the organism passes beyond the limit of Physiological adaptation. In this fact we are to find the origin of Disease and Death. The extreme character of Pathological modifications produces such an effect on the germ-plasm that they are inherited more readily than Physiological modifications (13). To supplement this position, which he does not prove, he tells us how we may hope to obtain "amelioration in the fitness of the generations yet to be born." Sufficient knowledge is to be imparted to the rising generation that when the time comes the members of it may select mates so as "to counteract injurious or to supplement deficient characteristics." "Indirect as it may appear, the individual possesses distinct power of adaptation over the offspring. The weakness of a system, such as the nervous or respiratory, in the one parent may be counteracted by the other, and any neglect to take cognisance of such a weakness on both sides more surely results in disease in the children" (25).

How is this "counteraction" brought about? It is surely not by both sides taking cognisance the neglect of which is here blamed. The author's belief in education is unbounded, and leads him to the following extraordinary conclusion: "For the future citizen the earliest teaching of the school must be how to live healthily and have healthy offspring," p. 28. The rest of Part I. claims more or less similar criticism. The style in which it is written is awkward, as if the author were unfamiliar with the subjects. Occasionally, as in the account of carbonic acid, the author shows himself unacquainted with recent work upon the questions at issue.

ETHNOLOGY IN FOLK LORE. By G. L. Gomme. ["Modern Science" Series, edited by Sir John Lubbock.] 8vo. Pp. 200. London: Kegan Paul, Trench, Trübner & Co., 1892. Price 2s. 6d.

THIS volume, by the president of the Folk-Lore Society, aims to set forth the principles upon which the subject may be classified, in order to arrive at some of the results which should follow from its study. Old races disappear while old customs last—carried on by successors, but not necessarily by descendants. Many customs and beliefs exist uselessly in the midst of civilisation, but their true meaning may be gathered if they can be traced to other countries where they occur in harmony with the manners and ideas of the people. The author maintains that the records of uncivilisation are as real as those of

civilisation, and that both belong to the same geographical area. Historians often ignore the less pleasing of the two records, and magnify the more pleasing. For instance, the records of life in various parts of London at the present day are painfully different, so are those in many parts of the Hebrides. Some of the conclusions of the author may be startling to those who have given no attention to the subject. He says:—"It would appear, then, that cannibal rites were continued in these islands until historic times; that a naked people continued to live under our sovereigns until the epoch which witnessed the greatness of Shakespeare; that head-hunting and other indications of savage culture did not cease with the advent of civilising influences, that, in fact, the practices which help us to realise that some of the ancient British tribes were pure savages, help us to realise also that savagery was not stamped out all at once and in every place, and that, judged by the records of history, there must have remained little patches of savagery beneath the fair surface which the historian presents to us when he tells us of the doings of Alfred, Harold, William, Edward, or Elizabeth." We wonder why the author refrains from adding that "little patches of savagery" continue to manifest themselves alongside of the "advanced guard of the nation."

MAN AND THE GLACIAL PERIOD. By Professor G. Frederick Wright. [International Scientific Series.] Pp. 385. London: Kegan Paul, Trench, Trübner and Co., 1892. Price 5s.

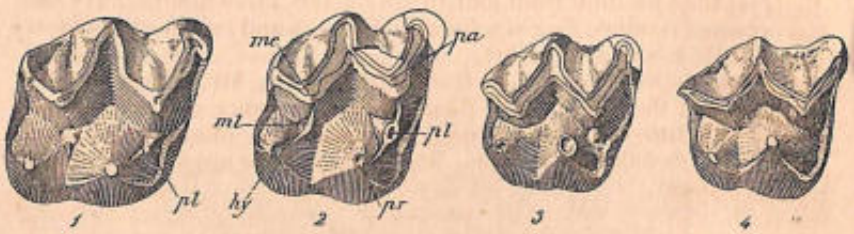
THE new volume by Professor Wright is somewhat disappointing. Another book has been added to the fast-growing literature of the Glacial Period, but we cannot feel that any real advance has been made in our knowledge of the subject, or that the author has even given us a good *résumé* of what is already known. No doubt to most European readers the account of the American Glacial deposits will be new; but so many of the statements made have been challenged by competent American authorities, that we should hesitate to recommend the book. In face of the explicit repudiation of all responsibility by the United States Geological Survey, of which Professor Wright was formerly an assistant, it will be safer to wait till a larger area has been properly examined.

The title of Professor Wright's book scarcely leads one to expect that less than a sixth of the volume has anything to do with the antiquity of man, the rest being taken up with stock subjects, such as glaciers and glacier motion, ancient glacial deposits of various parts of the world, the cause of the Glacial Period, &c. If this section were well done, we should not object, but it shows an imperfect knowledge of the literature, and an inability always to select good authorities for districts with which the author is not personally acquainted. As to the genuineness of the mortars, and the clay image, stated to have been found at great depths in the Western States, we prefer to suspend our judgment till American geologists are satisfied. We have had a considerable experience of the miscellaneous articles said to be found in mines and quarries, and without in the least suggesting bad faith on the part of the finders, we may remind our readers of the living toads discovered in coal, of the horse-bones or garden snails found in the Chalk, and of the miscellaneous articles unintentionally dropped down deep borings, to be brought up again by the boring tool.

A MEMOIR ON THE GENUS *Palæosyops* AND ITS ALLIES. By C. Earle. Extract from Journ. Acad. Nat. Sci. Philadelphia, vol. ix., pp. 267–388, pls. x.–xlv. (1892).

WE are glad to welcome as a comparatively new worker in mammalian palæontology Mr. Charles Earle, who in the finely illustrated memoir before us has produced a monograph which will form a fitting companion to those written by Professors Scott and Osborn on other groups of Ungulates. The systematic and methodical work now being undertaken by these three gentlemen on the fossil mammals of the United States is of the highest value and importance, since by this means alone can American vertebrate palæontology be rescued from the confusion with which it is beset through the over-zeal of describers anxious to procure priority for their own names.

The group Mr. Earle has set himself to monograph is one of peculiar interest, since it forms part of a family of Perissodactyle Ungulates which has no European representatives. *Palæosyops*, it may be observed, included animals of about the size of a tapir, with a skull of somewhat similar type, and the same number of digits to the feet. They had, however, upper molar teeth of a totally different type, which is described by Mr. Earle under the name of *buno-selenodont*; that is to say, while the inner pair of columns formed simple cones,



Right Upper Molar Teeth of *Palæosyops* (1, 3), *Limnohyops* (2), *Telmatotherium* (4). *pa.* paracone; *mc.* metacone; *pr.* protocone; *hy.* hypocone; *pl.* protoconule; *ml.* metaconule.

the outer pair were flattened and crescent-like. Molars of almost identical structure occur in the European genus, *Chalicotherium*.

Palæosyops, together with some nearly allied forms to which distinct names have been applied, occurs in the upper portion of the Middle or Bridger Eocene; while in the Lower Bridger they were preceded by a nearly allied but more generalised form known as *Lambdotherium*, which was probably very close to, if not the actual ancestor of the group. These forms are characterised by possessing the full typical dentition,[1] and by the upper premolar teeth being less complex than the molars, while the skull was devoid of bony horn-like appendages, and the third trochanter of the femur fully developed. Professor Cope regarded these forms as constituting a family by themselves; this view was, however, disputed by Dr. Schlosser, who proposed to include them in the same family with the larger forms from the Miocene, known as *Titanotherium*.

The latter view is supported by Mr. Earle, who shows that the two groups can only be distinguished by the circumstance that *Titanotherium* and its allies have at least some of the upper premolars as complex as the molars. In the gigantic *Titanotherium* of the Miocene, the skull was provided with enormous bony horn-like

[1] In the one named species of *Lambdotherium*, the first lower premolar is absent.

appendages, doubtless sheathed in true horn during life; several of the upper premolars were as complex as the molars, the incisor teeth were reduced in number or wanting, and the femur had nearly lost its third trochanter. *Diplacodon*, of the upper or Uinta Eocene, forms, however, as might have been expected from its geological horizon, a perfect connecting link between *Titanotherium* on the one hand and *Palæosyops* and its allies on the other. In this genus the skull has no horn-like appendages, while only the last of the upper premolars resembles the molars, and the typical three pairs of incisors are retained.

The evolutionary development of the *Palæosyops-Titanotherium* line may, therefore, now be regarded as worked out as fully as that of the Rhinoceroses, and it is curious to notice how closely the two groups follow parallel courses in this respect. Thus, in both there has been a development of horn-like appendages to the skull; in both the premolars have tended to assume a molariform structure, while in both the last upper molar has more or less completely lost the hinder of its two inner columns; then, again, the more specialised forms in each group have the front of their jaws edentulous. On the other hand, while the most specialised Rhinoceroses have acquired high-crowned (hypsodont) molar teeth, and have reduced the number of digits on the fore-limb from four to three, the Titanotheres have still low crowned retained the (brachyodont) molars and tetradactylous fore-feet of their Eocene ancestors.

So far as we can gather from his memoir, Mr. Earle seems to consider that the unguiculate *Chalicotherium*, in spite of the similarity in the structure of its molar teeth, has no sort of affinity with *Palæosyops* and *Titanotherium*. This, however, we are not at present prepared to admit.

R. L.

THE STUDENT'S HANDBOOK OF PHYSICAL GEOLOGY. By A. J. Jukes-Browne, B.A., F.G.S. 2nd edition. Pp. 666. London: George Bell & Sons, 1892. Price 7s. 6d.

THE new edition of Mr. Jukes-Browne's Handbook is too bulky, and would be improved by judicious compression. We would suggest, for instance, that it is quite unnecessary to say so much about the internal state of the earth, considering how little is known, and how likely to mislead is an appearance of knowledge. Already the dogmas of the rigidity of our globe and the stability of the earth's axis have been rudely shaken, and even the mathematicians hesitate. Is it really necessary, also, to ask the unfortunate student to master such terms as *metatropy*, *metataxis*, and *metacrasis*, and to understand what they mean? No examiner is likely to expect a knowledge of a number of technical expressions which he does not himself use, and which are not current coin.

We draw attention to these slight imperfections, for it is evident that the book meets a want, and will have an established place as a class-book for students, if it can be kept within reasonable limits as to size and price.

ATLAS DES ALGUES MARINES. By Paul Hariot. Pp. 51. 48 Plates. Paris: Klincksieck, 1892. Price 12 francs.

THIS volume of the *Librairie des Sciences Naturelles* aims at guiding the young phycologist to a knowledge of the more common seaweeds. On the 48 plates there are represented 110 species (not 108 as stated on the title-page), easily collected on the shores of France, and, with

one or two exceptions, it may be added on the shores of Britain. The plates are photographic reproductions of dried and mounted specimens, and are printed in appropriate colours (green, brown, and red). They will certainly be a greater help to the student of seaweeds, after these have been dried, than to the student of them who begins by trying to name the plants fresh from the pools. This is inevitable, however, and nearly every botanist has had the experience of being utterly puzzled by a plant in the field or garden which, on being dried, has revealed itself as an old herbarium friend. The text consists of excellent directions for collecting and preparing specimens, and of short and good descriptions of the Algæ figured, with a glossary of the few unavoidable scientific terms used. M. Hariot is a phycologist of distinction, and has brought accuracy and good sense to his task. Though the plates are not things of beauty, they are sufficient for their purpose, and the choice of species figured is good, in the greatest number of cases, for Britain. It will prove useful to beginners in this country who are familiar with French.

ENGLISH BOTANY. Supplement to the Third Edition. Part III. (Orders xxvi.—xl. Compiled and illustrated by N. E. Brown. London: George Bell & Sons, 1892. Price 5s.

PARTS I. and II. of this Supplement have already been noticed; Part III. brings us to the end of Dipsaceæ, and includes also the remainder of Rosaceæ, Onagraceæ, Cucurbitaceæ, Crassulaceæ, Saxifragaceæ, Umbelliferæ, Rubiaceæ and Valerianaceæ. Mr. Brown's connection with the work ceases with the present number, its completion falling to the lot of Mr. Arthur Bennett. With Part III. is published the preface, in which the scope of the work and the rules adopted in nomenclature are explained; no authorities for genera are accepted that date farther back than the year 1735, when Linnæus published the first edition of his "Systema Naturæ." Authorities for species do not date farther back than 1753, when Linnæus established the binomial system in the first edition of his "Species Plantarum." "It seems an act of gross folly," says the writer, "to apply the binomial system—as has been attempted in America—to dates before that system was in existence," and we heartily agree with him.

We also commend the expression of his view on the hybrid-making epidemic. No less than seven and twenty names follow the genus *Epilobium*; they are headed "Hybrids?" and "are supposed to be natural hybrids, and are considered as being intermediate in character between their supposed parents." The differences, however, "between the supposed hybrid and the species it most resembles being no greater and sometimes not as great as may often be found between individuals in a bed of seedlings from one plant." Mr. Brown sees no use in inserting in our floras descriptions of such plants. Coloured plates are given of *Pyrus rotundifolia* var. *decipiens*, *P. intermedia*, *P. pinnatifida*, *P. semi-pinnata*, *P. cordata*, and *Silenum curvifolia*.

MESSRS. WILLIAM WESLEY & SON have issued a list of the transactions of Scientific Societies, Periodicals, and Serials, including a nearly complete enumeration of the various scientific journals that have at different times been published in Britain. It is interesting to note how few of those devoted to popular exposition have survived for more than two or three years.

OBITUARY.

JOHN OBADIAH WESTWOOD.

BORN DECEMBER 22, 1805. DIED JANUARY 2, 1893.

LAST month we chronicled the death of the veteran entomologist, Mr. H. T. Stainton, and now we have to record the passing away of a yet more venerable figure among British students of insect life, Professor Westwood, of Oxford.

Westwood's native town was Sheffield; there, and at Lichfield, whither his family afterwards removed, he was educated at private schools. A strong taste for natural history, a marked aptitude for drawing, a love for Lichfield Cathedral and its services, were characteristics of his boyhood, and all three bore fruit in his after-life. On leaving school Westwood was articled to a solicitor in London; he afterwards became a partner, but soon relinquished law to give himself to his chosen studies—entomology and ecclesiastical art. Known to naturalists throughout the world by his work on the former subject, he has acquired a wide reputation among archæologists by his descriptions and beautifully executed copies of ancient Christian MSS., and illuminations, ivories, and inscribed stones. His archæological work was carried on concurrently with his entomology, from the publication of the *Palæographia Sacra Pictoria* (1845) to the *Facsimiles of the Miniatures and Ornaments of Anglo-Saxon and Irish MSS.* (1868), the *Catalogue of the Fictile Ivories in the South Kensington Museum* (1876), and the *Lapidarium Walliæ* (1876-9). It is to his beautiful draughtmanship that we probably owe the combination in Westwood of two such apparently dissimilar lines of study as archæology and entomology.

His entomological writings date from 1827, when he began to contribute papers on various orders of insects to different journals and to the publications of the Linnean and other Societies. In 1837 he published a new edition of Drury's figures of exotic insects, for which he wrote descriptions. In 1838 appeared his *Entomologist's Text-Book*, to be followed in the two following years by the two volumes of the *Introduction to the Modern Classification of Insects*.

This latter work is one of the classics of British entomology, and no later work on insects generally, published in England, has been able to supersede it. Many of its speculations seem strange to naturalists reared in modern days, as the arrangement of the insect

orders in two "circular" groups, a mandibulate and a haustellate group of five each, in accordance with the theory of Macleay that all animal orders should fall into such an arrangement. Westwood was, however, careful to state that he did not regard the Macleayan scheme as final, but considered it as a step towards a truly natural arrangement; he did not adopt it in dividing the orders into tribes or families. The *Modern Classification* was illustrated by careful figures of the distinctive parts of representative insects of each family or important genus, and is specially valuable in containing figures of larvæ, for which the student looks in vain in most recent systematic works on single orders, except those on the Lepidoptera and Hymenoptera.

In 1841 appeared the *British Butterflies*, to which work Westwood contributed the descriptions and Humphreys the plates. A similar association of the two authors produced *British Moths*, in two volumes, in 1843–5. In the latter year appeared also Westwood's *Arcana Entomologica*, in two volumes, containing descriptions and exquisite illustrations, by himself, of new and rare insects; the *Cabinet of Oriental Entomology*, also remarkable for the beauty of its plates, appeared in 1848. Westwood's next great work was the *Genera of Diurnal Lepidoptera*, in which he was associated with Doubleday and Hewitson; the latter was responsible for the illustrations to both volumes, Doubleday wrote the first, and Westwood the second, which was published in 1850–2, and comprised part of the Nymphalidæ, the Brassolidæ, Satyridæ, Libythæidæ, Erycinidæ, Lycænidæ, and Hesperidæ. This monograph was a sterling contribution to the classification of butterflies, and reduced to order the chaotic assemblies of species of earlier writers. About this time, Westwood drew the illustrations for Walker's unhappy work on the *British Diptera*. The Royal Society bestowed on him a Royal Medal in 1855. In 1859 appeared his *Catalogue of Phasmidæ in the British Museum*, an excellent monograph of that most interesting group of Orthoptera.

Westwood's connection with Oxford now began. In 1858, Rev. F. W. Hope presented to the University a valuable collection of insects, including some purchased from Westwood, who was appointed curator of the Hope Museum, which had thus been formed. The University was at that time without a Professor of Zoology; the munificence of Hope endowed a chair, to which he was to make the first nomination, and, in 1861, he appointed Westwood.

The new Professor, who had had no University career, received an honorary M.A. from Oxford, and was introduced to Magdalen College, of which, in 1880, he became an Honorary Fellow. For more than thirty years, therefore, he has been a familiar figure at his adopted University, of which, at the time of his death, he was probably the oldest resident. Shortly after his appointment he was associated with Mr. Spence Bate in a monograph of the *British Sessile-*

Eyed Crustacea, one of his very few zoological works beyond the limits of the Insecta. At this time the zoological world was convulsed with the controversy which followed the publication of the *Origin of Species*. In the year that Westwood was appointed to his chair, the British Association met at Oxford, and the famous debate between Professor Huxley and Bishop Wilberforce took place. Westwood, like most systematic naturalists of his day, was strongly opposed to the new views; he attacked the Darwinian theory in a few papers and letters, but prolonged controversy seemed distasteful to him. He went on steadily with his systematic work, and, while he continued to the end firm in his convictions, he saw ere his death a biological school established at Oxford whose teachers owe their inspiration to the work of Darwin. He lived, also, to see Oxford theologians welcome evolution as a valuable contribution to religious thought.

The collection under his charge at Oxford, second only to that of the National Museum—and in some groups superior—now occupied his attention, and in 1874 the new and rare insects of the Hope Museum were described and figured in the magnificent work known as the *Thesaurus Entomologicus Oxoniensis*. In the same year he issued a second edition of the *Butterflies of Great Britain*, in which the plates, as well as the text, were from his own hand. In the last few years of his life he returned to the study of the Orthoptera, and published, in 1889, his final great work, the *Revisio Insectorum Familiæ Mantidarum*.

Besides the books enumerated, numberless papers on entomological subjects by Westwood have appeared in scientific journals and transactions during the last sixty-five years. His contributions to the science of insect-life and structure were confined to no special group; the literature of every order has been enriched by his researches. Nor did he confine his labours to the purely scientific aspect of entomology; in many articles contributed to the *Gardener's Chronicle* he elucidated the history of the insect enemies of the cultivator of the soil. At the present time, when extreme specialisation threatens to become more and more the habit of systematic naturalists, it is well to remember that Westwood has shown that it is possible to do a vast amount of thoroughly good work without unduly narrowing the range of study. To his admirable personal qualities, all who had the pleasure of his acquaintance bear hearty witness.

G. H. C.

JOHN STRONG NEWBERRY, M.D.

BORN DECEMBER 22, 1822. DIED DECEMBER 7, 1892.

A PIONEER and honoured leader in North American Geology has just passed away in the person of Professor Newberry, of New York. Born seventy years ago at New Windsor, Connecticut, and removed in early childhood to the then newly-founded Cuyahoga Falls City, in Ohio, he was educated for the medical profession,

graduating in 1846 at the Western Reserve College, and later at the Cleveland Medical College in 1848. In 1849–50 he travelled in Europe, and in 1851 entered upon private practice as a physician at Cleveland, Ohio.

Newberry, however, was imbued from childhood with a taste for Natural Science, and the routine of a medical practitioner soon proved irksome. In 1855 he determined to devote himself to purely scientific investigation, and during that year he undertook the duties of surgeon, geologist, and naturalist to the Government expedition under Lieut. Williamson, organised to explore the country between San Francisco and the Columbia River. This resulted in his first published work on *The Geology, Botany, and Zoology of North California and Oregon*. In 1857–58 Newberry was one of the original explorers of the now well-known cañons of the Colorado River, and, still later, he prosecuted geological researches in New Mexico, Arizona, and Utah.

Newberry's distinguished services were soon rewarded, and in 1866 he became Professor of Geology in the School of Mines, Columbia College, New York. Two years later he also received the appointment of State Geologist of Ohio, and thenceforward his more systematic work began. The reports of the Geological Survey of Ohio, published under his direction, contain not merely stratigraphy and economics of local interest, but also detailed monographs on palæontology of fundamental importance. The descriptions of the Devonian and Carboniferous fishes and plants were contributed by Newberry himself; and these, with subsequent writings on the same subject, led to an entirely new view of the gigantic "placoderm" fishes of the Palæozoic period. A large and unique collection accumulated, and this the Professor placed in his museum at the Columbia College.

While actively engaged in lecturing and teaching until two years ago, Professor Newberry continued his favourite studies of the Palæozoic and early Mesozoic fishes and plants with so much success that in 1888 and 1890 he was able to issue two great monographs under the auspices of the United States Geological Survey. The first volume relates to the Triassic fishes and plants of New Jersey and Connecticut, and the second deals with various Devonian and Carboniferous fishes, each containing a synopsis of earlier researches, combined with new figures and many observations not previously published. Though somewhat antiquated in their style of treatment of the matter, and not remarkably systematic, these two works will ever remain standards for reference, and the mine of clearly-enunciated facts they contain will form an enduring monument of the author's industry and acumen. Even until the last, Dr. Newberry was contemplating a supplementary volume on the Palæozoic fishes, and he also had nearly ready for issue a monograph on the Cretaceous flora of New Jersey.

Dr. Newberry was one of the founders of the United States National Academy of Sciences, and occupied the Presidential chair of the New York Academy of Sciences from 1867 until 1891. In 1867 he presided over the meeting of the American Association at Burlington. In 1883 he became a Foreign Member of the Geological Society of London, and in 1888 he received the Murchison Medal awarded by this Society. Whether in his quiet home at New Haven, or in the Museum of Columbia College, or wandering abroad, the privilege of meeting Dr. Newberry was one to be cherished. He was truly esteemed by all who came in contact with him, and the memory of his friendship will long be treasured, both by his pupils and fellow-workers.

THOMAS DAVIES.

Born December 29, 1837. Died December 21, 1892.

BY the death of Mr. Thomas Davies, Senior Assistant in the Mineralogical Department of the British Museum, Mineralogy in this country loses one of its most accomplished students. He was the son of the late Mr. William Davies, for forty years connected with the Geological Department of the same Museum, and began his career as third-class attendant under Professor Maskelyne in 1858. For several years Davies was the only member of the Museum staff deputed to assist the Professor in arranging the collection of minerals after its separation from the Geological Department, and he rapidly acquired the foundation of that remarkable knowledge of mineral species for which he became so noted in later years. In 1862 he was promoted to the rank of transcriber, and in 1880 he received the well-merited reward of appointment to a senior assistantship. Mr. Davies was a prominent member of the Mineralogical Society, acting for some years as Editor of the *Mineralogical Magazine*, and later filling the office of Foreign Secretary. Besides mineralogical notes, he published several contributions to the petrology of the older rocks, and in 1880 he was awarded the Wollaston Donation Fund by the Geological Society of London.

MARTIN SIMPSON.

Born 1799. Died December 31, 1892.

BY the death of Martin Simpson, of Whitby, at the advanced age of 93, Yorkshire loses one of its earliest geological explorers. He was a young man when Young and Bird published their "Geological Survey of the Yorkshire Coast" (1822); he was associated with Bean, Williamson, and John Phillips in their early work on Yorkshire geology; and later on rendered assistance to Tate and Blake. The Whitby Museum was established in 1823, and Simpson, in 1837, was

appointed Curator, also assuming duties as Lecturer on Natural Science to the Literary and Philosophical Society.

In 1843, Simpson published a short Monograph giving descriptions of more than 100 Ammonites of the Yorkshire Lias; and in 1855 he published descriptions of all the known fossils of the Yorkshire Lias, and an outline of the Geology of the Coast. A second edition of "The Fossils of the Yorkshire Lias" was published in 1884, while his "Guide to the Geology of the Yorkshire Coast" reached a fourth edition in 1868. As Simpson's descriptions of his fossils were not properly defined, only about 20 of his original species of Ammonites are now accepted.

In 1884 the Murchison Geological Fund was awarded by the Geological Society to Simpson, and he then remarked that he felt the more honoured as he had received no assistance or encouragement from his fellow-townsmen.

WE also have to record the death of BENJAMIN VETTER, Professor of Zoology at the Polytechnic in Dresden; of the veteran Russian mineralogist, N. I. KOKSCHAROW, author of the *Beiträge zur Mineralogie Russlands*; and of H. F. BLANFORD, F.R.S., late Chief of the Meteorological Department in India.

WE are happy to state that the announcement of the death of DR. D. STUR appearing in certain French and English papers, is founded on the mis-translation of a German paragraph. The late Director of the Austrian Geological Survey is now enjoying a State pension.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

PROFESSOR F. W. HUTTON, F.R.S., has been appointed curator of the Canterbury Museum, New Zealand, and Lecturer on Geology in the University College.

MR. WALCOT GIBSON has been appointed an assistant geologist on the Geological Survey of Great Britain. He received his geological training under Professor Lapworth, and has since travelled in the regions of the Transvaal and Uganda. An important paper on the gold-bearing and associated rocks of the Southern Transvaal was communicated by Mr. Gibson to the Geological Society of London in 1892.

IT is proposed to establish an Agricultural College for Kent and Surrey, under the name of the "South-Eastern Agricultural School and College." A scheme for the conversion of Wye College to this purpose has been submitted by the Charity Commissioners to the Committee of Council on Education.

THE Colleges of Aberystwith, Cardiff, and Bangor are just completing the preparation of a draft charter for the proposed degree-conferring University for Wales. This was discussed at a conference at Shrewsbury on January 6, but it does not apparently meet with much favour among those best acquainted with the requirements of higher education in the Principality. While nominally founding only one University, some fear that it will raise each of the three constituent colleges independently to that rank.

THE COUNCIL of the British Institute of Preventive Medicine is appealing for donations towards the cost of erection of a suitable building. The circular is signed by Sir Joseph Lister (Chairman), Sir Henry Roscoe (Hon. Treasurer), and Dr. Armand Ruffer (Hon. Secretary), and is accompanied by a long list of donations already received. The sum of £20,000 has been promised to the funds of the Institute by the trustees of the late Mr. Richard Berridge, on condition that a further sum of £40,000 is raised for land and buildings.

UNDER the presidency of H.R.H. the Prince of Wales, a committee has been formed to arrange for a suitable memorial of the late Sir Richard Owen. Sir James Paget, Sir William Flower, and Mr. W. Percy Sladen (Secretary, Linnean Society) have consented to act respectively as Vice-Chairman, Treasurer, and Secretary. A meeting was held in the rooms of the Royal Society of London, Burlington House, on January 21, when the first list of subscriptions was read, and it was resolved to place a full-length marble statue of the late Superintendent of the Natural History Departments of the British Museum in the hall of the Museum at South Kensington.

AT the meeting of the Bristol Town Council on January 2, the gift of the Bristol Museum and Library was duly accepted on behalf of the citizens. The resolution was carried unanimously amid much enthusiasm.

A NATURALISTS' Field Club has been founded at Limerick, and has joined the other Irish Societies in adopting the *Irish Naturalist* as its organ.

THE Cambridge Entomological Society has extended its scope so as to include other branches of Natural History, and will henceforth be known as the Cambridge Entomological and Natural History Society. The Society is not confined to members of the University, and at the present time is in a more flourishing condition than it has been for some years past. The secretary is Mr. W. Farren.

THE medals and funds at the disposal of the Council of the Geological Society of London will be awarded as follows, at the Annual Meeting on February 17:—Wollaston Medal, Professor N. Story Maskelyne, and Fund to Mr. J. G. Goodchild; Lyell Medal to Mr. E. T. Newton, and Fund divided between Miss C. A. Raisin and Mr. Alfred N. Leeds; Murchison Medal to the Rev. O. Fisher, and Fund to Mr. G. J Williams; Bigsby Medal to Professor W. J. Sollas.

AT the meeting of the Mineralogical Society of Great Britain on January 17 a biographical notice of the late foreign secretary, Mr. Thomas Davies, was read, with the announcement that a memorial fund would be collected for the benefit of the widow and family of the deceased. Professor Story Maskelyne is president of the committee, Dr. Hugo Müller is treasurer, and Mr. H. A. Miers has undertaken the duties of secretary. Donations may be forwarded to Dr. Hugo Müller, F.R.S., 13 Park Square East, Regent's Park, London, N.W.

A NUMBER of Conchologists residing in and around London have decided, if possible, to found a society devoted exclusively to the study of Mollusca, with its headquarters in the Metropolis. It is suggested that monthly meetings should be held from November to June, for the exhibition of specimens and the reading and discussion of papers, which it is intended should in due course be published and distributed to members. It is not proposed to form a library or a collection—at all events for the present. It is estimated that a subscription of 10s. 6d. per annum would be sufficient to effect these objects (no entrance fee being charged to those who join in the course of the first year). A circular has been issued by Mr. E. R. Sykes (of 13 Doughty Street, London, W.C.), who will be glad to hear from all who are willing to further the object in view. In the preliminary list of supporters we observe the names of Colonel Beddome, Lieut.-Colonel Godwin-Austen, Messrs. Cosmo Melvill, E. A. Smith, and B. B. Woodward, the Rev. R. Boog Watson, and Dr. H. Woodward.

CORRESPONDENCE.

Occurrence of Sowerby's Whale (*Mesoplodon bidens*) on the Norfolk Coast.

On December 19, 1892, I received a telegram stating that a "strange fish" was on shore at Overstrand, near Cromer, and subsequently that it was some species of Whale; on the 20th, in company with Mr. S. F. Harmer, of the Museum of Zoology and Anatomy, Cambridge, who happened to be staying in this neighbourhood, I went to Overstrand, and we found it to be an adult female of the above rare species. Its history, we learned, was as follows. At about 8 a.m. on Sunday, December 18, one of the Overstrand fishermen saw from the cliff an object lying in the water near the beach, which he at first took to be a log of wood, but soon perceived to be a "large fish." After obtaining assistance he fastened a noose over its tail, and secured it by an anchor, till it was placed on a trolley and drawn up the gangway to a shed on the cliff, where we saw it. The animal was alive when first observed, but died before it was taken from the water. As placed, it was, unfortunately, in such a position as to render photographing impossible, and our attempts proved unsuccessful; I believe no photograph was taken after it had been removed from the shed. Before our arrival it had been eviscerated, and a very advanced fœtus was taken from it. We made a very careful examination of the exterior, and hope to publish a full description in due course. In the meantime, I may say that the female was of a uniform glossy black colour, with the exception of the anterior edges of the flukes of the tail, and the jaws, which were grey, of various shades, in places almost white, and the body was spotted and blotched with white or pale grey in a very curious manner; the fishermen told us that when quite fresh out of the water there was a bluish shade pervading the whole. The young animal was black above, and reddish on the sides and lower parts, probably owing to the effusion of blood into the skin, which would doubtless otherwise have been white. The total length of the old female, measured in a straight line to the centre of the tail, was 16 ft. 2 in., and that of the young one 5 ft. 2 in.; across the flukes of the tail the adult female measured 3 ft. 8 in.

The present is the nineteenth known example of this remarkable animal, all of which have been met with in the North Atlantic during the present century, but with the exception of one taken in 1889 at Atlantic City, which came into the possession of the United States National Museum at Washington, and of which no account has, I believe, at present been published, in no other instance has an example in perfect condition come under the notice of a Cetologist. Individuals, or their remains, have been found in Scotland and Ireland, but the only previous English example was met with at the mouth of the Humber, in September, 1885.

T. Southwell.

Norwich, *January* 9, 1893.

[This specimen has been purchased for the Rothschild Museum, Tring.—Ed.]

Movement of Diatoms.

Two notices have appeared in Natural Science containing criticisms of my paper on the occurrence of "Pseudopodia" among the diatoms *Cyclotella* and *Melosira*. One of these was by Mr. Jabez Hogg, the other—more recent—by Mr. Minchin. They both assert that the discovery of these so-called pseudopodia is a very old one; that they have often been described; and Mr. Hogg gives their compositions. I should be glad to make two remarks in reply.

Firstly, my slides have been examined by a majority of the best Diatomists and Biologists in England, and not a single one has suggested that he had either seen or heard of anything like these pseudopodia. They have also been sent to

several of the best foreign Diatomists, who likewise failed to refer me to any previous description. Mr. Hogg and Mr. Minchin dealt with generalities. I should be glad to have a reference to any paper where these pseudopodia have been described.

Secondly, I have found these diatoms with the pseudopodia in almost every bit of water—whether pond or stream—in the South of England, where I have looked for them. They occur in many places in London and the suburbs, and from Yarmouth to Sheerness, Eastbourne and Wiltshire.

I advise my critics to take the trouble to get some of these diatoms and study the phenomena for themselves. If they will do so, I do not think we shall hear much more about either opinions, processes, or old discoveries.

J. G. GRENFELL.

[I AM much surprised to find Mr. Grenfell including me among his "critics," with regard to the pseudopodia of diatoms. I do not profess in any way to be an authority on the subject, and the criticism which Mr. Grenfell kindly attributes to me was quoted word for word from Mr. Jabez Hogg. It was Mr. Hogg, not I, who asserted that the discovery of these so-called pseudopodia was a very old one, and that they have often been described. Mr. Grenfell seems to have overlooked the inverted commas in which these statements are placed. My only object was to place before English readers an account of Professor Bütschli's researches upon the movement of diatoms, and to point out that "any form of pseudopodia is quite inadequate and unnecessary to explain" the phenomena observed by him, without, however, expressing any opinion of my own as to the existence or nature of these so-called pseudopodia.

E. A. MINCHIN.]

PALÆONTOGRAPHICAL SOCIETY.

THE attention of the Council of the Palæontographical Society having been called to a paragraph in your January number, in which you wrote, "In the case of several of these monographs, announced as in preparation, we believe the statement is false, and that members of the Council are aware of the fact," I am directed to state that all these monographs have been promised, and that the promises have not been withdrawn. THOS. WILTSHIRE, Secretary.

[Promises are fickle. We have already proved the accuracy of our assertion in the case of two announcements, and the Secretary might be profitably "directed" to make further enquiries. The continued official repetition of promises, which are well known to be valueless, prevents other palæontologists from undertaking works that are much wanted.—ED.]

TO CORRESPONDENTS.

N.B.—CHANGE OF ADDRESS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, *now removed to* 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.*

All ADVERTISEMENTS *to be forwarded to the sole agents,* JOHN HADDON & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

H. ULLYETT (Folkestone).—The wingless bird referred to is not a species of *Apteryx*, but the well-known Weka Rail (*Ocydromus australis*). Examples of this common New Zealand Bird can generally be seen in the aviaries at the Zoological Gardens in London.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 13. Vol. II. MARCH, 1893.

NOTES AND COMMENTS.

Natural Science at Oxford.

REMUNERATIVE positions affording facilities for scientific research in Britain are so few, that all who are interested in progress will sympathise with every effort to retain, for purely scientific work, the services of those who have shown an aptitude for original investigation during their University career. There must be Fellowships and Lectureships in the colleges to enable the graduate choosing science as a profession to pursue his work during the interval between the taking of his degree and a possible permanent appointment; and the more of these endowments there are, the greater is the inducement for young University men to aim at something higher than mere honours in an examination. In the interests of Natural Science, we thus desire to call attention to "An Appeal to the Governing Bodies of the Colleges within the University of Oxford," now being circulated by the Linacre Professor of Comparative Anatomy, Dr. Ray Lankester. Though primarily a matter for the University itself, the issues involved have so direct a bearing on the future progress of the biological and geological sciences in this country, that the "Appeal" demands the support of all investigators. The University of Oxford is second to none in the repute of its Professors and Lecturers in Natural Science; its laboratories, museums, and scientific libraries are among the finest; but, if the governing bodies of the colleges continue practically to ignore the School of Natural Science, all these provisions and advantages are in vain. According to Professor Lankester's statement, the graduates in Science *are* ignored to an almost incredible degree, and hence the necessity for a vigorous protest.

It appears that the total number of open Fellowships in the Oxford colleges is at present about 300, and that of these only thirty

are held by graduates devoted to scientific research. Nearly all these, too, are in the hands of physicists and mathematicians; while only one is held by a zoologist. Considering that more than 200 classical and historical scholars are thus favoured, the division of honours is certainly as unfair an arrangement as can well be imagined. We may appropriately ask, with a critic in the *Daily Chronicle* of February 13, whether the amount of research published by Oxford men in classics and history is 200 times as great as that accomplished in zoology; and, with that writer, we may further truly exclaim:—

"The reverse might not be quite accurate, but it would be most unquestionably nearer the truth. A great mass of Oxford fellows are persons who batten for life on the college funds, because, when boys, they passed a successful examination. Like the Rev. Mr. Pontifex, they obtained honours in examination. They, furthermore, resemble that divine in having afterwards abandoned their studies. At twenty-three years of age it is extremely creditable to have got a 'first-class,' but the reputation so earned can hardly be considered a claim at sixty-three. I have heard it urged in palliation of the inactivity of the classical fellow, as contrasted with the science man, that there is less new work to be done in the former subjects. I beg leave to doubt this; but if it be true, then all the more reason to encourage a subject where there is still a wide field open to the investigator. If the colleges were to bestow their fellowships not by examination, but after the candidates have established some claim by original investigation, there would be a wholesome weeding out of those whose interest in their subject and capabilities were of the purely text-book order. With a test of this kind there would be some hope for the adequate recognition of science, and really able men would be attracted to Oxford, and stay there. Above all, that most delusive weed of university growth, the man of promise, would be rooted out, and his place taken by the man of performance."

As Professor Lankester remarks in his "Appeal," there is not much doubt that financial considerations on the part of those Fellows who are interested in the subjects taught in a college are at the root of the difficulty. Until some change can be effected in these fundamental matters, there is thus but slight hope of a due recognition of Natural Science, which can only be taught in the University laboratories and museums. That the reformation will come sooner or later is inevitable, and we trust its advent may be speedy.

MECHANICAL EVOLUTION.

WE have on previous occasions alluded to the speculations of the American school of zoology in reference to the mechanical origin of the various arrangements observed in the vertebrate skeleton. According to these authors, under the leadership of Professor Cope, there is every reason to believe (i) that living bony tissue is plastic, and therefore readily modified in its form by impacts, strains, friction, &c., and (ii) that acquired characters are inherited. Every process

in the evolution of part of a vertebrate skeleton is thus to be explained on purely mechanical principles.

To the discussion of this subject another interesting contribution has just been made by Professor Cope, who describes two cases of a repaired elbow-joint, one in a man the other in a horse (*Proc. Amer. Phil. Soc.*, vol. xxx., pp. 285–290, pl. ix.). In his well-known work on "Animal Mechanism," Marey has already described instances of the natural healing and repair of diseased or dislocated joints—how a new articulation is often completely formed, not only with accurately fitting faces, but also provided with the requisite ligaments and synovial fluid. Professor Cope's paper thus deals merely with the details of two remarkable examples of this repair, in which a new elbow-joint was completely formed after a dislocation. As the author remarks, if the mechanical necessities of a case of this kind lead to the production of a completely new and finished joint within the space of a few months, "how much more easy has it been for stimuli of allied character to develop the features of normal articulations during the ages of geologic time."

"We have here, also," says Professor Cope, "an instructive lesson as to the matter of inheritance. Everyone knows that mutilations, luxations, &c., are not usually inherited. This is because they are not 'acquired,' in the proper sense of the word. Since characters, truly acquired, are inherited, it is evident that a long continuance of the stimulating cause is necessary to produce a true acquisition. The difference between a character produced by causes apart from the normal life of an animal, and not repeated, and those produced by causes operating daily and hourly for geologic ages, is necessarily very great."

A Glimpse of the Tropics.

In the recent December and January numbers of the *Botanical Gazette*, Mr. D. H. Campbell gives an account of a six weeks' vacation spent last summer in the Hawaiian Islands. In position, these islands are unique, being "more isolated than any other land of equal area upon the globe." They are 2,350 miles from the nearest part of the American mainland, the bay of San Francisco, and about the same distance from the Marquesas and Samoa Islands to the south, and the Aleutian Islands a little west of north. As Wallace remarks in his *Island Life*, they are "wonderfully isolated in mid-ocean," the nearest of the widely-scattered coral reefs and atolls being six or seven hundred miles distant, and all nearly destitute of animal or plant life.

The group consists of seven large inhabited islands and a few rocky islets, extending in a S.E. direction just within the tropic of Cancer. Hawaii, by far the largest and most southerly, is 70 miles across and about the size and shape of Devonshire; the greater part

is, however, rendered sterile and uninhabitable by its active volcano and the lava-deposits. The entire archipelago is volcanic, and separated from the nearest continents by great depths, so the islands must have been always as isolated as at present.

The flora was well worked up by Hillebrand, and, as might be expected, is very peculiar; of 800 indigenous species of seed plants and ferns, 653 or 75 per cent. are endemic; of seed plants alone, 81 per cent.; and of dicotyledons, 85.

The capital, Honolulu, which is situated on Oahu, one of the more northern islands, "is like one great botanical garden." This is said to be largely due to Dr. Hillebrand, who introduced many foreign plants, and his place, kept much as it was when he left the islands, "was a very remarkable collection of useful and ornamental plants from the warm regions of almost the whole globe." Very striking to the traveller from temperate climes is the great variety and number of palms; the beautiful royal palm (*Oreodoxa regia*), with its smooth columnar trunk and its plume-like crown of leaves, betel-nut palms, (*Areca*), the wine palm (*Caryota*), the sugar palm (*Arenga*), and many fan-palms. The young coco palms are beautiful enough, though, unfortunately, very subject to the attacks of an insect which eats the leaves, but in old specimens the trunk is too tall for its girth, so that the trees look top-heavy. The great preponderance of Leguminosæ, especially the sub-orders Cæsalpinieæ and Mimoseæ, is also remarkable. All about the town is the rapidly-growing algaroba (*Prosopis juliflora*) a graceful tree, with fine bipinnate leaves and sweetish yellow pods. The pods are largely used for fodder, and the wood forms the principal fuel-supply for Honolulu. The monkey-pod (*Pithecolobium Samang*), tamarind, and various species of *Bauhinia* and *Cathartocarpus* were also noticed, besides a great number of shrubs and trees, with showy flowers or leaves, mostly familiar from pictures or the greenhouse; such were several species of *Musa*, the traveller's tree (*Ravenala madagascariensis*), and the beautiful *Hibiscus Rosa-Sinensis*. At Puna Hou College is a hedge of night-blooming cereus 500 feet long. Of fruit trees the mango, bread-fruit, and guava are common, also the alligator pear (*Persea gratissima*) and the papaya.

Away from the city the luxuriant vegetation is strange. Along the sea-shore the plain is almost destitute of trees, save for an occasional coco palm, while in the fertile lowlands near the sea are the principal cane- and rice-fields.

The valleys at the back of the city, though very rainy, richly repaid a visit by the luxuriance and variety of their vegetation. Grass-covered hills give way as one proceeds to thickets of *Canna* and a rosy-white *Clerodendron*, while the curious screw-pine is occasionally seen, though much more abundant in some of the other islands. Several showy ipomæas are very common.

With increase of moisture, masses of ferns increase in beauty and number, and at about 1,000 ft. elevation species of *Cibotium*

appear, the genus containing the largest tree-ferns of the islands. Here, too, is seen the tiny *Trichomanes pusillum*, forming dense mats on rocks and tree trunks, and looking like a delicate moss; the full-grown frond is not more than half-an-inch high. Epiphytic ferns also become frequent, species of *Acrostichum*, *Polypodium*, and the beautiful bird's-nest fern (*Asplenium nidus*). Everywhere are thickets of *Freycinetia*, very troublesome to get through.

Of the trees of this lower forest region, by far the most conspicuous is the euphorbiaceous *Aleurites moluccana*, with pale silvery-green foliage; and a little higher up the phyllode-bearing Koa (*Acacia Koa*), the principal timber tree, with wood not unlike mahogany in appearance. Conspicuous also is the mountain apple (*Eugenia malaccensis*), with beautiful crimson fruits, and, higher still, the nearly-related *Metrosideros* forms a striking object with its grey-green leaves and scarlet, feathery flowers.

Hawaii is reached by steamer in about thirty-six hours from Honolulu. On the way are passed Molokai, the barren and forbidding leper settlement, Lauai, and Maui, the next largest island.

Hawaii consists of three great volcanic cones, only one of which is now active. Mahukona, on the leeward side of the island, the first landing-place, showed a forlorn expanse of bare lava, with scarce a trace of vegetation, quite a contrast to the next stopping-place, Hilo, where the luxuriant vegetation comes down to the water's edge.

The forest here is most interesting, and owing to the great annual rainfall (180 inches) and its more southerly position, like the rest of the flora, more tropical in character. Ferns and mosses luxuriate, while flowers are almost entirely wanting. Many tree-ferns, species of *Cibotium*, had trunks from 15 to 20 or even 30 feet high, with fronds 18 to 20 feet long. Growing on their trunks were epiphytic ferns, the peculiar *Ophioglossum pendulinum*, with its long, strap-shaped leaves, and exquisite species of *Hymenophyllum* and *Trichomanes*. Of the terrestrial ferns, the tropical *Gleichenia dichotoma* and *Marattia Douglasi* were noticeable, and several species of *Lycopodium* and *Selaginella* were common.

Coffee is extensively planted in this region as well as on the lee side of the island, and the quality of the berry is exceptionally fine.

Near the volcano, Kilauea, about 4,000 feet above sea-level, are many interesting plants, one a *Vaccinium*, with berries resembling cranberries.

The leeward side of the island is dry and vegetation scanty. The soil, however, is very fertile, and, when water can be had, produces magnificent crops of tropical products, such as pine-apples, coffee, sugar, &c.

A flying trip was made to Kauai, the oldest, geologically, and the richest, botanically, of all the islands. Hillebrand states that not only is the number of species larger, but they are more specialised than in the other islands. Here were seen several woody Lobeliaceæ.

As in all the islands, there is a great difference between windward and leeward sides, the former affording some of the most beautiful scenery and luxuriant vegetation of any seen during the trip.

KIDNEY TUBES IN AMPHIOXUS.

DR. THEODOR BOVERI, of Munich, has made one of the most striking zoological discoveries of recent times. In Spengel's *Zool. Jahrbücher* (vol. v., p. 429), he describes and figures a system of segmental canals in *Amphioxus*. These occur all along the region of the pharynx—one pair for each two pairs of gill-slits. They lead from the body-cavity to the atrial sac. There are 90 pairs altogether: the first and the last open by single ciliated funnels into the coelome; the others by from five to seven funnels. A network of blood-vessels covers the funnels, and Boveri shows that carmine and indigo-carmine taken into the blood reach this network and are removed by the funnels. He gives a series of striking arguments in favour of regarding half the atrial cavity as the homologue of the primitive pronephric duct of Craniata. The whole paper is wonderfully convincing; and one has only to look for a moment at sections of *Amphioxus* to see the canals, and to be astonished that one has not seen them before.

To readers who are not specialists in Zoology, the discovery of kidney tubes in Amphioxus may seem a matter of little moment; but the presence of a definite order and sequence in the development of the excretory organs is one of the strongest links binding together the Chordata—the group of animals which includes the vertebrates and a few lowly forms which have only the gelatinous rod which, in vertebrates, is the embryonic predecessor of the vertebral column. In all the members of this group, the excretory organs first appear as simple tubules, like the tubules of worms. These open at one end by a funnel into the body-cavity; at the other, into a longitudinal duct at each side. In higher vertebrates, like birds and mammals, a complicated series of changes supervenes, and the original excretory organ comes into use as part of the reproductive apparatus while the permanent kidney is developed in connection with the posterior end of the original organ. Probably there is no plainer instance of the embryological law that the individual in its development travels along the path its ancestors traversed in their evolution; for practically every term in the series of changes in the higher vertebrates is preserved as the adult condition in some lower form. It was a puzzle why Amphioxus, which in so many of its organs preserves stages embryonic in higher animals, should be inexplicable in the set of organs most clearly identical in other chordates; and now Boveri has shown that its excretory organs are precisely what analogy and homology alike demand.

More Notes on Seedlings.

The January Bulletin of the Torrey Botanical Club opens with some "Studies upon Akenes and Seedlings of Plants of the Order Compositæ," by W. W. Rowlee, illustrated by five plates. The account, which occupies 17 pages, is much less comprehensive than that given in Lubbock's recent work (vol. ii., pp. 98–161), and the general observations are of less value, being based on fewer experiments. Thus the cotyledons, we are told, vary in shape from spathulate to orbicular, but Lubbock gives several instances of linear cotyledons,—*Bidens humilis*, species of *Ursinia*, and *Coreopsis gigantea*; the American author describes one species of *Coreopsis* (*C. discoidea*), which, however, like another species (*C. laciniata*) included by Lubbock, has broader cotyledons. Nor is any mention made of the interesting polymorphy of achenes, which obtains in the Old World genus *Calendula*. Mr. Rowlee's studies form, however, a valuable addition to our knowledge, as the greater number of his species are not included in the larger work, and the genera are in many cases unrepresented. Moreover, figures are given of every species described, both of the fruit and seedling, and often two or more stages of the latter.

The cotyledons of the Ox-eye Daisy are oblong, like those of the Corn Marigold and another species of *Chrysanthemum* (*C. carinatum*) figured by Lubbock, but Rowlee makes the two following leaves spathulate and entire, thus differing widely from their deeply-toothed character in the other two species. The seedling of the Yarrow corresponds with the description in the English work, except that there is no mention of any hairiness on the first leaves; it would be interesting to know whether they are glabrous in the New World, or the hairs have been merely overlooked. One *Artemisia* is described, viz., Wormwood (*A. Absinthium*), and its seedlings, we are told, may be distinguished from those of the Yarrow by the oblong shape and entire margin of the second and third leaves after the cotyledons. Comparison with the larger treatise shows that this is not a generic distinction, for there we find that in *A. annua* the corresponding leaves are three-toothed as in the Yarrow though in another species (*A. Mutellina*) entire.

The Helianthus (*H. divaricatus*) mentioned bears a general resemblance to *H. cucumerifolius*, included by Lubbock, the cotyledons being ovate and obovate respectively, while in each case the first internode and following leaves are somewhat densely hairy.

In speaking of the embryo, cotyledons and hypocotyl are invariably distinguished, the latter term including everything below the cotyledons, and consequently the radicle. This seems a pity; the radicle is an organ so well marked, not only from a morphological, but also from physiological and anatomical points of view, that it is a pity to ignore or slight its presence in the earliest stages.

Some remarks are also made on the germination of the seeds.

That of annuals and biennials is much prompter than of perennials, while a much larger percentage of the former germinate, giving larger and hardier seedlings.

The percentage of germination was found to be greatest in "persistent field weeds," only two of which, however, were studied; the Burdock and Dandelion; that of wayside and fence-row weeds like Elecampane (*Inula Helenium*) comes next, while in those which cannot be characterised as weeds it is considerably less, the general averages being 49, 33, and 8 respectively. This is taken as direct evidence that the vitality of seeds of a species is a factor in determining its abundance and ability to become a weed. Twenty-five seeds of each species studied were sown with the upper (pappus) end downwards, and a similar number erect; 149 of the former germinated, and 178 of the latter. The radicle emerges from the lower end, and these numbers show that it is a disadvantage for this end to be uppermost, a position, moreover, which the fruit would not naturally take, since the pappus, acting like the feathers on an arrow, tend to keep it erect.

The Hidden Coal-fields in the South of England.

Geologists who are interested in the study of the hidden coal-fields in the South of England will find some exceedingly suggestive remarks in two papers published by M. Marcel Bertrand. The earlier one, though entitled "Sur la Continuité du Phénomène de Plissement dans le Bassin de Paris" (*Bull. Soc. géol. de France*, ser. 3, vol. xx., p. 118), relates largely to the anticlinal and synclinal folds which cross the English Channel and North Sea. We are unable, however, altogether to follow the author when he accepts the present irregularities of the sea-bottom as corresponding to ancient undulations in the strata beneath. It is so rare in our seas to find an undisturbed rocky bottom, except near the Straits of Dover, and shifting sand-banks are so common, that it seems more reasonable to suppose that the irregularities of the sea-bed are mainly due to the scour of the tide. Yet some of these irregularities are undoubtedly ancient hills and valleys, or lines of escarpment.

M. Bertrand's second paper, "Sur le Raccordement des Bassins Houillers du Nord de la France et du Sud de l'Angleterre" (*Ann. des Mines*, Jan., 1893), deals mainly with the hidden coal-fields, their probable position, and their extent. The maps published with these papers are excellent, and ought to throw a great deal of light on the trend of the Coal-measures in our southern counties.

We would also direct attention to Mr. Brady's report on the Dover Coal Boring, reviewed elsewhere.

Prehistoric Archæology.

The "Congrès International d'Archéologie Préhistorique," which met last year at Moscow, has led to the publication of an in-

teresting series of papers on the Quaternary Geology of Russia. The most valuable of these, especially to the foreign reader, will be Professor S. Nikitin's outline of the present state of our knowledge of the subject. In this summary it is pointed out that Palæolithic man existed in Russia contemporaneously with the mammoth during the second half of the Glacial Epoch, but only towards the southern limit of the glaciated area. In the more northern districts all the implements yet found belong to the more advanced Neolithic races. The existence of a distinct Interglacial Period, like that recognised by Scandinavian and German geologists, has not yet been satisfactorily demonstrated in Russia.

It is impossible to criticise this paper without an intimate knowledge of Russian geology, but it may be worth while to remark that in Western Europe also, the relics of Palæolithic man are only found in regions beyond the southern limits of the last glaciation. Is it possible that in Russia, as was perhaps the case in Britain, Palæolithic man was Interglacial? His relics are not found in the areas covered by ice during the last glaciation, and this has been explained as the direct result of the advance of the ice, which ploughed up and destroyed all the pre-existing Palæolithic deposits as far as it could reach.

Our Monthly Selection.

Mr. James Payn has somewhere expressed regret at the supposed fact that men of science are unable to make their subject popularly interesting. It is certainly true that "popular science" does not, as a rule, result from the literary activity of persons qualified to write upon such subjects. We have had occasion more than once to point out that this, if a blessing, is one securely wrapped up; for it is to be presumed that an individual, if he cares to read scientific articles, prefers them to be fairly reliable, else why read an article which is clearly meant to be instructive?

The editor of a recently-started journal, entitled "The Sketch," advertises his willingness to consider paragraphs which are "smartly" written. As an indication of what he wants, attention may be directed to a paragraph in the first number—"Why not a Professor of the Zoo?" The writer talks a little about the parietal eye; for this overworked organ of vision appears to have just filtered down through the "dailies" to the "weeklies." In the course of time it may perhaps reach the monthlies. We present Mr. Payn gratis with the suggestion. The antiquity of the points with which it deals is, however, not the only claim which this paragraph has to be considered smart. Many of us will be astonished to hear that the "hole" in a baby's head at birth is the vestige of a parietal eye! The writer concludes with a lament that there is no professor at the Zoo to make men acquainted with these facts (!). Even though he presumably possesses no parietal eye himself, the two usual organs of vision would

be enough for him to observe that every year for the last—we don't know how many, there have been popular lectures on Zoology given at the Gardens. For the last few years they have been delivered by Mr. Beddard; and it is clear that a course of lectures on Zoology would not come by any means amiss to this paragraphist.

WE are always glad to note the gradual percolation of science through the lower strata of our population, and we have derived much instruction from a recently-issued pamphlet entitled "Leaves from the Book of Nature, or Stepping Stones in Creation," by L. Piers. The author seems to be a great admirer of the Geological Department of the Natural History Museum, and his acquaintance with the collection, if not extensive, is at least peculiar. He traces the history of our globe from the "Archaen" rocks "composed of schistose or granitic character," through the Cambrian period when the sea was at 90°, the Silurian and Devonian, with their Graptolites, Ammonites, Crinoids, and Nummulites, the Carboniferous, "Treassic" and Jurassic, then "the Cretaceous system composed of white chalk," down to the Tertiary periods and the great ice age. "Wonderful and marvellous truly are the mysteries of Nature"! thus we read of that "curious crustacean called *trilobite*; they were in three great families, *olenellus*, *paradoxides*, and *olenus*," then the Crinoids, whose "province was to check the too great increase of certain other creatures, while in their turn they were devoured by the larger fishes" [poor fishes!]. But, oh! the "strange and long reptile, named *Deinosaura*," "the *Plesiosaurus*, the earliest crocodile," the "extraordinary *Petrodactylus* and *Trinoceros*."

Two notes on page 16 inform us, first, that "the vertebrate kingdom are in five great divisions: fish, reptiles, birds, mammalia, man;" secondly, that "The visitors will find at the entrance to the Geological Department some excellent illustrated catalogues." Are these catalogues really so bad? or did Mr. Piers write his book before he read them?

IT is well-known that a long-haired variety of the tiger ranges at the present day as far north as the region of the Amur, on the frontier of China and Siberia. The animal is thus capable of living in a cold climate, but the astonishing discovery has just been made of evidence of its former range to a northern latitude even within the Arctic Circle. Among the fossil bones collected in the New Siberian Islands and on the adjoining mainland by a Russian expedition despatched from St. Petersburg in 1885, there are five characteristic limb-bones of the tiger. They are described by Dr. J. D. Tscherski in his report on the collection (*Mém. Acad. Imp. Sci. St. Pétersbourg*, vol. xl., no. 1, with 6 plates), and occurred in the same deposits as bones of the musk ox, mammoth, &c.

In the January number of the *Quarterly Journal of Microscopical Science* (vol. xxxiv., p. 317), Mr. Arther Willey has another interesting study on the ancestors of the Chordates. As the result of a series of observations, he groups together *Cephalodiscus* and *Balanoglossus* as Protochordata with mouth ventral, and with no endostyle; Ascidians and Amphioxus, as having an endostyle and a dorsally-situated mouth. In these two groups, *Cephalodiscus* is parallel with the Ascidians. Both are sessile, possess a U-shaped alimentary canal, have a small number of gill-slits (one and three respectively), and reproduce by buds. *Balanoglossus* and *Amphioxus* are also parallel, being free-swimming, with straight alimentary canal, many gill-slits, and without the power of budding. That difficult form, *Appendicularia*, which was long regarded as the most primitive of the Ascidians, Willey considers as a reduced and secondary derivative from the Ascidian stock.

The application of photography to the study of animal locomotion is still being extended. M. Marey, we are glad to record, is continuing his observations on the swimming of fishes, and has just published the result of photographing a skate (*Comptes Rendus*, vol. cxvi., pp. 77–81, Jan. 16, 1893). The body of the fish was firmly fixed, and the motion of the great pectoral fins was photographed from the side and from the front. The undulation of the fins proves to begin at the anterior border, and the whole progress of the motion bears a singular resemblance to the movements of a bird's wing during flight. M. Marey proposes now to devise means of studying the locomotion of the skate under natural circumstances, and will attempt to determine the extent and directions of the currents produced in the water.

There are many strange phenomena connected with the life of protozoa within animal tissues, but a recently-announced discovery by Professor W. A. Haswell (*Proc. Linn. Soc. N.S. Wales*, ser. 2, vol. vii., 1892, pp. 197–199) is, perhaps, the most remarkable of its kind. It is well-known that certain amœboid forms spend the whole of their existence within a single cell of the tissue of their host, and it is the ordinary rule among gregarines to begin life within a cell; but it now appears that even certain freely-swimming flagellate protozoa are capable of passing through life in a similarly restricted sphere. On examining a turbellarian worm from a pond in the neighbourhood of Sydney, Professor Haswell found one or more minute green flagellate protozoa in many of the cells. These were observed to be in an active state, and appeared to be closely related to the familiar *Euglena*.

Dr. Axel Goës has just described, in the *Bull. Mus. Comp. Zool. Harvard Coll.* (vol. xxiii., no. 5), the remarkable new form of arenaceous Foraminifer found during the dredging expedition of the U.S. Fish Commission steamer "Albatross." This form, of which the finest specimen measures 190 mm. in breadth, is by far the largest Foraminifer known. It consists of a "strong network of bundles of very fine chitinous threads, measuring in thickness 0·003–0·006 mm., incorporated with a thin layer of finest sand and *débris* of shells. . . . The test is leaf-formed, with outlines usually describing a triangular, fan-like or reniform figure, with more or less strongly arcuated edge, the whole reminding one of a Padina alga of 0·5 to 2 mm. in thickness." In its earlier stages, this Foraminifer has the fan-like shape of *Pavonina*, but, as the chambers increase in number, they no longer arch over the centre, but terminate with a blunt end at the top, the lower end being produced with long root-like appendages, which "serve probably as fastenings to the botton, where they often are entangled in masses of a Rhizammina." Dr. Goës compares this form with the *Jullienella* of M. Schlumberger (*Mém. Soc. Zool. France*, iii. (1890), p. 211). Dr. R. Hanitsch, on the other hand, writes to *Nature* (Feb. 16) to say that he thinks it is one of the deep-sea Keratosa, and suggests that it may possibly be referred to *Stannophyllum zonarium*, Haeckel.

We understand that advices have been received from the Villiers Expedition to Lake Rudolph, dated Vitu, December 20, 1892. Lieut. Villiers has had fever, and the command has fallen on Lieut. Stanford. Mr. J. W. Gregory is reported to have been in excellent health and spirits, and the expedition was to be on the march in a few days. Subsequent statements, however, have appeared, which show that Lieut. Villiers has recovered, and has joined the Portal expedition to Uganda.

We are glad to observe that, since our last issue, Professor Lapworth has closed the controversy on the geology of the Scottish Highlands, by contributing to the *Daily Chronicle* of February 8 a concise historical statement which we can endorse.

I.

The Nucleus in some Unicellular Organisms.

THE importance of continuous and exhaustive studies concerning the ultimate and irreducible nature, morphology, and function of animal and vegetable cells is universally seen. The cellular elements of tissues began to be observed even by Hooke two centuries and a quarter ago; and some further advances were made, but it was not until 1831 that Robert Brown, the great pioneer in botany, took the first great step leading to a practical advancement of the subject. He gave definite knowledge of vegetable cells, and he demonstrated that the nucleus was one of its normal elements.

It need hardly be remarked that the cell-theory proper had its foundation in the work of Schleiden, but by him it was not extended beyond the structure of plants; he clearly defined the vegetable cell as the elementary organ which constitutes the sole essential form-element of all plants, and without which a plant cannot exist; and as consisting, when fully developed, "of a cell-wall composed of cellulose, lined with a semi-fluid nitrogenous coating."

The cell was thus to Schleiden a vesicle with semi-fluid contents.

In the year following (1839) Schwann showed that the Animal Kingdom was ultimately as cellular in structure as the Vegetable; but to the vesicular wall and the semi-fluid lining of Schleiden he added a third element, the *nucleus*, and he deemed this essential to the history of the cell, at least in some period of its life.

From this time, the triple elements of the cell were accepted as its normal condition; but investigation made the continuance of this belief more and more uncertain. It was shown that cells multiplied by "budding," and that the nucleus underwent fission when the cell divided; and also that no cell could take origin save from a parent cell.

Soon it was demonstrated that there was no vital importance attachable to the cell-wall; and in 1857 Leydig declared it entirely unessential, and defined the cell as a "soft substance enclosing a nucleus."

Subsequently, Max Schultze contended that the life of a cell might be complete without a nucleus, but the cell was held to be the ultimate morphological unit in which life was manifested. Every

living thing presented itself as a sum of vital unities, any one of which manifested the properties of life.

The living matter of the cell was held to be *sui generis*. Not-living matter could not, by any combination we knew of, apart from the genetic intervention of living things, take on its unique properties. Only from the living could the living arise. This living matter was, and is, called protoplasm; and even now a cell can hardly be more clearly defined than as a small mass of protoplasm enclosing a peculiarly organised element known as the nucleus.

The further study of cells, aided by the constantly increasing power and precision of our microscopes, revealed complexity, reticulation, striation, foliation, radiation, and other appearances, at present undetermined by the best observers with the finest accessible optical powers; while the nucleus is seen to be highly complex, and subject to the most striking internal modifications during its cyclic changes.

To pursue or even indicate the history of the prolonged and untiring work upon the cell during recent years is no part of the object of this paper; enough to recall that the nucleus is found to be the initiating centre of all the great changes which the cell undergoes.

But the study of the cell has been conducted in the main (1) upon cells belonging to highly complex organisms, and, therefore, as vital unities of that organism must partake, in all their mutations, of the complexities involved in the developmental organism of which they are units; and (2) the different processes of cellular change and nuclear modification have been studied, of necessity, after death, and under the influence of desiccation and staining.

For years it has impressed itself upon the mind that the great problem of the nature and behaviour of the cell should at least be begun where the problems to be solved ought to present themselves in their simplest condition, and that would appear to be amongst the unicellular organisms—the organisms whose complete life, when traced through all its cyclic changes, begins in a cell and ends in a cell, presenting an elementary though, in their case, a permanent condition in the evolutional history of more complex organic forms.

These are easily and everywhere accessible in that remarkable but minute group of organisms known as *Saprophytes*. They succeed the saprophytic Bacteria in the destructive fermentation of large masses of dead organic tissue. There are only a few of these forms known, and their successive action upon the decomposing mass is both mechanical and physiological.

They are strictly unicellular; the majority are nucleated cells; but the problem of the cell is rendered the more interesting by the study of them, from the fact that amongst the group are distinctly non-nucleated forms.[1]

[1] *Vide* the author's researches, *Month. Micro. Journ.*, vols. x., xi., xii., xiii., xiv., xvi.; also *Proc. Roy. Soc.*, 1878, and *Journ. Roy. Micro. Soc.*, ser. ii., vols. v. and vi.

As typically represented, they are oval or suboval, are possessed of one or more flagella, and have an average long diameter of about the 1-4000th of an inch.

Two fair representatives of the group as re-examined during the last four years with the advantage of our immensely-improved object-glasses and eye-pieces are represented in Fig. 1. A represents a form about the 1-4000th of an inch in long diameter, and B a kindred form the 1-5000th of an inch measured in the same way. They are in the drawing magnified 1,800 diams.

A is distinctly nucleated, and trails a flagellum when swimming, but (as in the drawing) often "anchors" it to the "floor" of the stage; and from this position acts by springs upon the masses of decomposing matter around it. The form B simply swims either forward or backward by means of its flagellum at either end.

The life-cycle of A may be taken as representative, and will show what was actually known by us before the investigations upon the nucleus made within the last four years with our present apochromatic microscopic illumination and apochromatic objectives.

Taking A, Fig. 1, as the normal form, in certain conditions of the organism now definitely known to us, a sudden opening of the "beak" to which the flagellum is attached (*a*, Fig. 2, C) would arise partly dividing the flagellum, as shown in the figure. While this was increasing, so far as we could discover in our earlier studies—in the course of a couple of minutes—the nucleus C showed a definite dividing line, and the "trailing flagellum" also rapidly split, as at *b*.

The division first initiated widened (D, Fig. 2, *c*), and the front flagellum became wholly divided, the trailing one not splitting so fast; but the nucleus showed, as at *d*, a strong tendency to fission.

Very rapidly the bisection ensued, until generally, in not more than four minutes, the division had been wholly effected lengthwise, as in Fig. 3, E, *g*, *h*, and the nucleus was wholly divided save for a fine thread connecting the parts, as at *i*, K, and the trailing flagellum was nearly divided.

Soon the protoplasmic neck connecting the dividing forms was almost wholly gone, as in Fig. 4, F, and the two nuclei, *n*, *o*, were as perfect as in Fig. 1, A, that is to say, a highly refractive body with a distinct envelope, or border; and the two perfect forms, *l*, *m*, Fig. 4, become disconnected, dividing the trailing flagellum to its root, and each being free.

This process is exhaustive in the great majority of the forms. By following successive fissions—that is, one-half of each division—it was found that the end came at from four to five hours. In other words, this process of division ceased by the death of the last-divided form. But this only applied to about eighty-five in every hundred. The remaining fifteen go through a very curious change.

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Fig. 5 represents a final form derived by fission. Its chief distinction is a strong nucleus, and always a free-swimming condition.

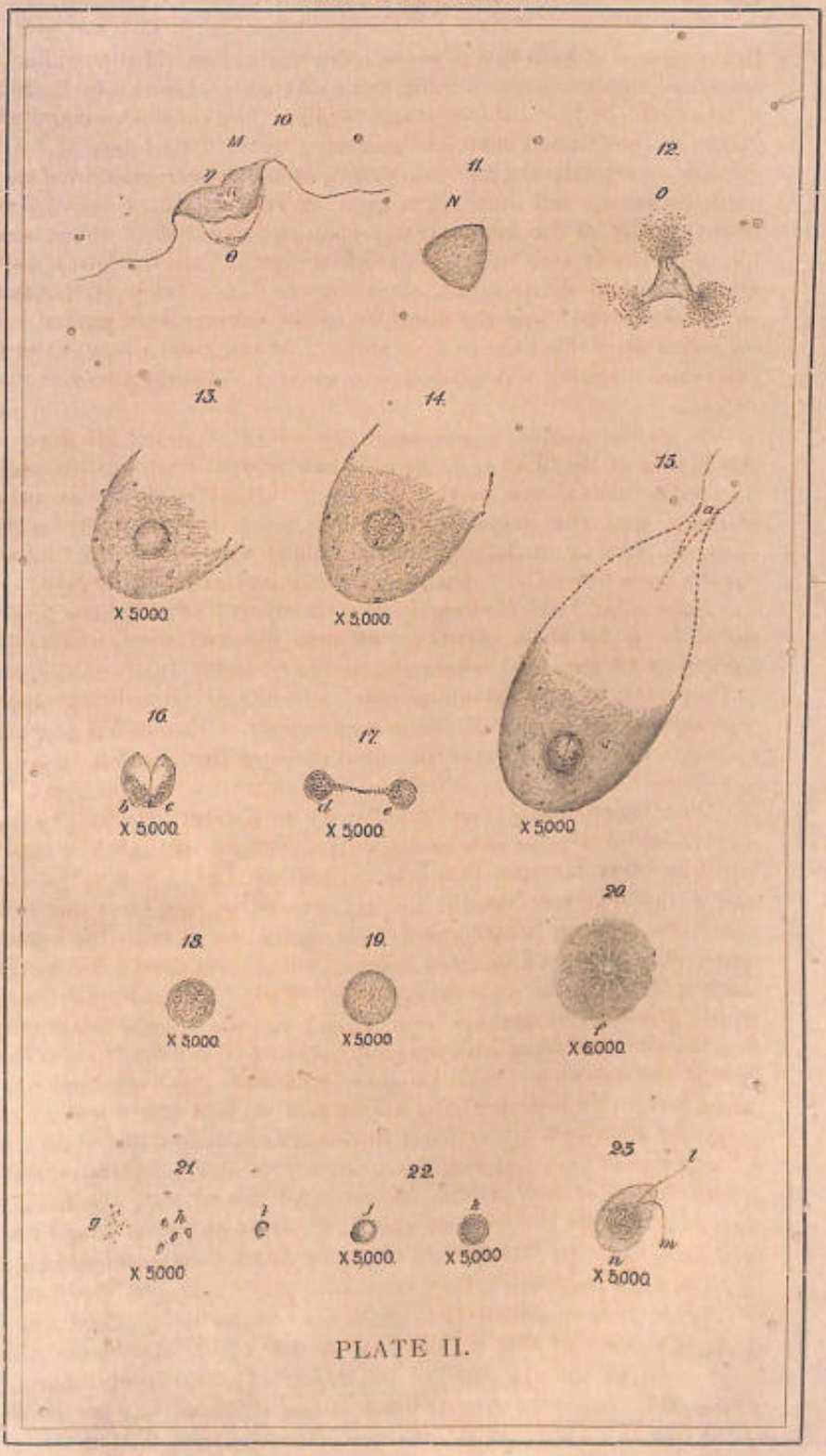
In the course of from five to seven minutes there would supervene an amoeboid condition, not striking, but still visible, as shown in H, Fig. 6, where the body-substance would oscillate from the hollow condition shown to the lateral expansions indicated by the dotted lines at *s*, *t*, a vacuole—curiously like an "eye-spot"—would appear, exhibiting very marked opening and shutting, as at *v*; and the nucleus, *u*, was driven to the middle of the body, greatly enlarged, and, losing its nucleus-like aspect, was torn apart in irregular halves; meanwhile, a very strong sigmoid division-line, shown in J, Fig. 7, at *w*, *x*, became extremely marked, and the divisions of the nucleus were pushed one on each side of this line, as at *w* and *x*. At this time a point of protoplasmic material was pushed out, as at *y*, dividing into two fine threads.

A gliding motion now ensued, by which *w* glided off from *x*; this is seen at Fig. 8, κ, α, β, showing two normal forms nearly freed from each other in this way; but in our earliest work it was quite manifest that the nuclei of these two forms had quite strikingly changed, having undergone some radical alteration, and trailing flagella were formed at *z*, partly by growth and partly by division.

Soon after total freedom from each other, these modified forms swam freely for some minutes, and then directed their motion to some part of the field where the ordinary forms (with the usual nuclei) were in greatest abundance, and almost immediately what was seen to be an act of fusion commenced. The initial stage is shown in Fig. 9, L, where the unlikeness of the nuclei is seen at γ, δ.

The fusion takes place rapidly up to a certain point, for in eight minutes a large part of the body-substance of each has fused with the other, as seen in 10, M, where the nuclei are fully united, and a radial or star-like diffusion takes place, as seen in η; and this continues, growing fainter and less perceptible for an hour, or sometimes an hour-and-a-half, until it wholly disappears, and all trace of nucleus is gone, the mass of the plasm of the combined cells being faintly granular, or perhaps reticulated; but the trailing flagella (θ) fuse together and unite with the body, which now rapidly changes, so that in the course of fifteen minutes it assumes the shape and condition shown in Fig. 11, N. At the end of four hours a kind of pulsating movement shows itself in this triangular sac, and at length it burst one or two, and sometimes (as in Fig. 12, *o*) the three corners of the triangular sac, and an enormous number of semi-opaque and extremely minute particles, escaped (Fig. 12), and by watching these they were seen to pass by growth into forms corresponding to the common form from which they arose.

These observations were made in the original and renewed study of the behaviour of this organism as a whole; but during the past three years the greatly superior microscopic apparatus, both illuminating and image-forming, placed at our disposal, has led to an


10
M
n
θ
11
N
12
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14.
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22.
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X 5000

PLATE II.

endeavour to study alone the behaviour of the nucleus, both before and during, as well as after, the various steps in the cyclic changes undergone by this cell.

A magnification of from 5,000 to 6,000 diameters was found efficient for this, and with the life-history of the cell known, it was less difficult to trace by themselves the nuclear changes.

In studying these, it is only the changes occurring and observed in the living organism, and during its life, that have proved of any real value. The effect of desiccating and staining these saprophytes was in all senses unsatisfactory; the details were too minute, or too delicate to admit of this treatment, for whatever method was adopted, the results, as compared with the living form in the same stage, were simply unintelligible and useless. But this could only be really seen by *comparison* of the results of the same phenomenon in the living organism, and in the dried and stained state.

Nevertheless, by using a one-and-a-half per cent. of acetic acid, to which varying quantities of methyl-green are added, a decided strengthening of the special nuclear features was effected. Thus the nuclear envelope seen in most stages of its activity is much strengthened, and so were the beaded threads embedded in the nuclear hyaloplasm; and little, if any, inconvenience could be seen to result in the organisms.

In the inactive stages of the organism, or when no cyclic changes were manifest, the nucleus was hyaline, or at least exhibited nothing but the faintest traces of a colourless reticulation when very critically examined. This simple state of nuclear hyaloplasm is seen in Fig. 13, representing a portion of the body of the organism and the nucleus, the latter magnified 5,000 diameters.

By steadily watching a form in this condition, it would in all probability be seen to become more marked in its granulation, and at length what appeared like a delicate thread of beaded structure became more and more visible, occupying at first the circumference and at length the whole plasm of the nucleus, as shown in Fig. 14; but when this was complete a distinct line, made more visible by methyl-green, appeared, as shown in the nucleus of that figure. No indication of any division anywhere in the body of the organism had yet appeared. Now, however, it soon becomes manifest that the complex foldings and twistings of the beaded thread in the nucleus condense on either side of it, leaving the dividing line in a hyaline space, as shown in Fig. 15; and now for the first time a line appears at *a* and opens, so that the fission of the *body* has begun.

After this the process is rapid, occupying only three or four minutes. The nucleus opens as in Fig. 16, where it is seen apart from the body, corresponding to the stage seen in Fig. 2, D, rapidly passing into the condition seen in Fig. 17, corresponding to the state shown in Fig. 3, E, and where the fine-beaded thread appears to be drawn out, connecting the still dividing nuclei; but in this

condition the convolutions of the beaded thread have again distributed themselves over each divided nucleus, and soon enlargement and total separation take place, as in Fig. 18.

In the divided organism containing this organism the division-line soon again appears in the nucleus, and successive fissions go on. But in the nuclei belonging to the condition of special fissipartition shown in Fig. 7, the nuclei present quite a distinct appearance. No wall is at all discernible in connection with it, and it is slightly larger than the ordinary form; it is highly refractive, and, on critical examination, does not show the exquisitely fine convoluted beaded thread of the usual nucleus, but dissociated white, and slightly darker, somewhat oblong granulations, as shown in Fig. 19. When in the usual order, in the act of fusion shown in Fig. 9, L, this nucleus comes into contact with the ordinary one (γ, δ), there is almost immediate "melting of either into other," the whole mass resulting being milky and almost opaque, taking a white star-like appearance, resulting apparently from the diffusion of the whole mass through the entire plasm of the fused organisms, for in the course of a short time it disappears, and, as in Fig. 11, N, a non-nuclear triangular body results.

The bodies ultimately emitted from this are of extreme minuteness, and partially opaque; and in their growth one of the most remarkable features appear.

In Fig. 21, if *g* represent the original spores as sent out into the fluid, *p* will represent their growth in form and relative size in an hour, *i* shows one of the same in an hour-and-a-half, while *j* (Fig. 22) gives the result of growth at the end of nearly three hours; but now very little advance in size is made; there is a pause, as if the form had been arrested in growth by death. But, on critical examination, there is found to be a very slight enlargement taking place, and with it the formation of the *internal* beaded thread-like convolution which may take thirty to fifty minutes to complete, as in Fig. 22, K.

From this time growth is rapid; but it appears to proceed from within the nucleus outward, as shown in Fig. 23, where a small part of the body-substance has arisen outside the nucleus, *n*, and, at this stage, the two flagella are seen as though emerging from the nucleus, as at *l*, *m*. The body-substance now rapidly grows until the adult form and size are reached, as in Fig 1, A, when the processes of the life-history once more repeat themselves.

It would thus appear that every point in the cyclic changes in this organism originate in, or are initiated by, the nucleus. Fission is not only indicated, but begins first in the nucleus, while the nuclear changes during fission are worthy of careful note. The change in the character of the nucleus in certain forms after fission is full of suggestion, and that their subsequent union with nuclei of the common type is equivalent to fertilisation, we can scarcely doubt. Not less interesting is the origin of the nucleus in the growing form, and its relation to the growth of the body.

The non-nucleated saprophytes (as Fig. 1, B) may, perhaps, in some sense, be looked upon as wholly nuclear, although I have been unable to discover anything but the most faint traces of structure within them. They present the constant appearance of the nucleus when in a totally inactive state, as shown in Fig. 13.

This appears to present the activities of the nucleus in its simplest condition as accessible to our present means. That there are phenomena far more delicate than those here recorded, we can scarcely doubt, but it would appear that it is in the unicellular organisms that the first lessons in cell-history may be learnt.

W. H. DALLINGER.

II.

Are Great Ocean Depths Permanent?

IT has been suggested by the editor that English readers would like to hear my views on that much-debated question, the Permanence of Ocean Basins. I am rather at a loss how to deal with the subject, because this question involves so many difficult chapters in the history of our planet, and because I regret to see that discrepancy of views exists on fundamental principles.

Mr. Wallace begins by arguing from the principle that "on any large scale, elevation and subsidence must nearly balance each other, and thus, in order that any area of continental magnitude should rise from the ocean floor, ... some corresponding area must sink to a like amount." I venture with deference to reply that I cannot agree to this. It seems, on the contrary, as if two different types of movement had been going on since the first formation of the terrestrial crust. In the first place, there is folding, recently explained in a masterly way by Professor Lapworth, in his Address to the British Association. Secondly, there is the sagging-down or "effondrement" of smaller or greater parts of the crust, caused by the progressive diminution of the planet's radius. *This descent of parts of the earth's crust seems to be the true origin of the great oceanic basins.*

Sometimes the contour of the sunken area follows the trend of a folded mountain chain; at another time it may cut right across it. In smaller examples the outline very often takes a more or less irregularly circular or elliptical form. The descent of a considerable area, forming a large new depression, demands a certain part of the existing volume of oceanic waters for the filling of the new depth. The consequence is the sinking of the oceanic surface all over the planet, and the *apparent step-like rising of coast lines.* Thus is explained the apparently episodic elevation of whole continents, without any disturbance of horizontality, or the least alteration of the net of watercourses spread over the land. It is in this sense alone that a certain balance of "elevation" and "subsidence" might be conceded.

In the entire Pacific region the limits of the oceanic basin are traced out by the trend of long mountain folds. So it is from New Zealand and New Caledonia to the borders of Eastern Asia, to the Aleutians, and all along the western coast of both Americas. This is not the case in the Atlantic, nor in the Indian Ocean; here the

coasts are not outlined by folded structure, except in the arch of the Lesser Antilles, and in the corresponding short arch passing through Gibraltar, which serves to connect the mountains of north-western Africa with those of the south of Spain. These are what we may call the Pacific and the Atlantic types of oceanic regions.

Indian geologists have shown how the immense Asiatic mountain waves, moving southwards against the Peninsula, have been dammed back by the resistent Peninsular mass, the Korána Hills, and the wedge-shaped mass of Assam; so that they actually form distinct arches, separated by deep angles receding to the neighbourhood of Tank, north of Dera Ismail Khan, to the Upper Jhelum and Brahmaputra Valley. In this case we call the Peninsula the "Vorland."

Now cast a look on the map of the North Pacific, and compare the receding angles which mark the western and the eastern ends of the Aleutian arch, where it abuts against Kamschatka and North-West America. You will remark that this part of the Pacific is a "vorland," and homologous to the Indian Peninsula, whilst the Yellow Sea, Behring Sea, and others lie within the folded region. You may also examine the Mediterranean, and observe that the western half lies wholly within a curved and folded mountain chain (Apennines, Sicily, North Atlas, Gibraltar, Andalusian Cordillera), and that in the eastern half all the part south of Crete and Cyprus and east of Sicily lies on the African "vorland," and the rest on the sunken parts of the Tauro-Dinaric arch.

In the Atlantic region the mountain folds, as a rule, break off against the ocean (*e.g.*, Brittany coast, Devon and Cornwall, south-west Ireland), or else have their folds facing away from the ocean, as in the case of the Alleghanies, and all other folded zones on the eastern side of North America as far as Newfoundland. The folds disappearing in south-west Ireland and in Brittany so very much resemble those rising from beneath the Atlantic on the coasts of Nova Scotia and New Brunswick, that M. Marcel Bertrand has ventured to publish a sketch-map, showing these chains trending right across the Atlantic.

There exists a curious tendency for a depression or a sort of valley to form in front of the great folds facing the "vorland." For instance, the depressions of the African desert in front of eastern Atlas, the river valleys in front of the high Indian chains, and the Persian Gulf in front of the Zagros chains. Quite recently the Austrian exploring ship "Pola" found a depth of 4,400 metres near the south-west coast of Greece, near the front of the Dinaric arch; and some of the greatest oceanic depths show exactly the same position in front of the arches of Japan, the Kuriles, and the Aleutians with Alaska. This is the homology, for example, between the Ganges valley and the Tuscarora depth, both marking the limit of the folded ranges and the "vorland."

The structure of the earth's crust does not, therefore, tell us that

the great depths must be very old or permanent; so let us compare the character of the sediments near the existing coasts.

The Old Red Sandstone is an extra-marine formation; yet the Old Red Sandstone runs out into the sea in the Orkneys, reappears in the Shetlands, and the same palæontological and extra-marine character is known in Spitzbergen. Old Red exists on the north coast of Lapland, and on the White Sea. The plant-bearing beds of the Karoo run out to sea in British Kaffraria; they are repeated in India and Australia. The fresh-water beds of the Wealden pass over from England to the Continent; they not only reappear in Hanover, they run out into the Atlantic in the lower Charente, and on the coasts of Spain and Portugal. Why must the continent which formerly bounded all these vast fresh-water basins have been limited within the existing 1,000- or 1,500- or 2,000- fathom line? The breaking down of the bed of the Ægean Sea, described by Spratt and Neumayr, is of Post-pliocene date, for Pliocene fresh-water deposits form parts of the coast; and yet the depths go far beyond 1,000 fathoms. In 1892 the "Pola" measured 3,591 metres in lat. 35° 52 36", long. 29° 1' 24" E., quite near the south-west coast of Asia Minor, and close to the mighty Ak Dagh (10,000 feet); and this although the separation of the neighbouring island of Rhodes is so recent, that not only do the Pliocene fresh-water beds pass over from the Continent, but according to Bukowski also considerable masses of Middle Pliocene fluviatile conglomerates, originating in Asia Minor, have been deposited by a great river on this island.

Now suppose the existing quantity of oceanic water to decrease, say by evaporation into the ether, as Zöllner once thought, or in any other way; we might by this gradual diminution of the entire quantity attain a beach-line 500 fathoms below the present shores. The continents would appear so much higher, and dry land would extend. Plains would successively appear, more or less similar to Holland, and our present rivers Shannon, Seine, Loire, &c., would flow through these plains. In most cases the rivers would be caused to cut back their valleys, new transverse and parallel lines of erosion would appear, and the plain would be diversified into hills and valleys. The hills south of the Shannon would probably show the rest of those anticlines and synclines which dip below the ocean in south-west Ireland, and we should be able to see a greater part of the northern Armorican arch. The Scilly Islands would appear as another granitic laccolite within the continued Armorican region of Cornwall. The gneiss of Eddystone would come up within this northern Armorican arch, exactly as the gneisses of the Alps stand up within and behind the folded arches. In a similar way, in the south, the anticlines and synclines of French Armorica, which disappear north and south of Brest, would begin to be visible; but in the north-west of Ireland we should see a plateau, ending in a steep cliff, the abrupt boundary of a deep channel, separating the great island of Rockall

from the European continent. And all this varied new-born land would be green and full of life, and people would not be at all willing to believe that it ever was not so.

Then we would descend to the new shore, and one of our great masters would tell us that ocean depths are permanent, that is to say, from 1,500 fathoms downward, or from 2,000 fathoms. The general impression resulting from the study would be the same as now, but the assumed permanent level would be reduced by 500 fathoms.

We might invite our master to undertake an excursion with us; we would go to Scotland. An isthmus, some 1,200 feet high at the narrowest part—Sir Wyville Thomson's ridge—would lead us first to some isolated peaks, nearly 3,000 feet high, and over a rising country to the high peaks of the Faröes. We would observe land-born coal-beds between the great coulées of basalt. Proceeding further, we would travel to the north-west, over a broad tract of rocky land some 1,200 or 1,300 feet high; then first meet an isolated mountain of about 1,800 feet, and from there ascend to the volcanic mass of Iceland, where we should see those vast fields of lava, dotted with active volcanoes, and observe the long faults and open rents cutting through the masses of lava and trending across Iceland in a broad arch, first from south-west to north-east, then northward, beyond the volcano Askja to Husavik; and beyond this broken and breaking zone we might gain the great "effondrement" or "Kessel-bruch" of Faxafjord, beset all round by volcanoes and hot springs, from Snaefells Jokull to Reykjavik. In following the rents so well described by our indefatigable Thoroddsen, we might detect faulted plant-bearing beds, and recognise the equivalents of the Faröe coal-seams. If some younger and more impressible student be in our company he might well exclaim, in face of these plant-bearing beds, stretching on to Greenland and showing the existence of a vast dry land in late times, and in face of these rents and volcanoes: "Verily, Professor, the sagging-down of the North Atlantic is the most recent event; it is going on before our eyes; and as the highest mountain chains are the youngest, so also are the deepest parts of the planet the most recent." I fear I should not know how else to answer the student than, "Really, I do not know."

Now let us quit the coasts and examine the interior of a great continent.

Modern geology permits us to follow the first outlines of the history of a great ocean which once stretched across part of Eurasia. The folded and crumpled deposits of this ocean stand forth to heaven in Thibet, Himalaya, and the Alps. This ocean we designate by the name "Tethys," after the sister and consort of Oceanus. The latest successor of the Tethyan Sea is the present Mediterranean.

I asked Dr. Diener, recently returned from India, to give me his estimate of the thickness of the deposits in the Silakank region. Dr. Diener answers: From Dhauli-Ganga Valley, between Gweldung

and Pethathali encamping ground above Silakank (19,265 English feet), to Sirkia River in Hundes; a complete section from (Cambrian?) Haimantas to the Gieumal Sandstone (Cretaceous), without a great discordance, gives, according to Dr. Griesbach:—

Haimantas	3,000—4,000 feet.
Lower Silurian	200 ,,
Upper Silurian	1,100 ,,
Devonian..	700 ,,
Carboniferous	1,200—1,400 ,,
Permian and Trias	3,600—3,900 ,,
Lias and Spiti Shales	1,400? ,,
Gieumal Sandstone	1,200—1,500 ,,
	12,400—14,200 feet.

The determination of the thickness of the Spiti Shales and Gieumal Sandstone is difficult, because these less-resistent beds are crumpled into local folds.

A parallel section across the Kurkutidhár range (Chor Hoti, about 18,000 feet), Shalshal encamping ground and Shalshal Pass (16,390 feet) to Hundes gives, without the Haimantas:—

Silurian	1,200—1,400 feet.
Devonian and Carboniferous	1,800—2,300 ,,
Permian and Lower Trias	200 ,,
Middle Trias	100 ,,
Upper Trias to Dachstein Limestone ..	1,400 ,,
Dachstein Limestone, Rhætic and Lias ..	2,200 ,,
Spiti Shales	1,000? ,,
Gieumal Sandstone	1,500? ,,
	9,400—10,100 feet.

These figures show that a great and deep ocean has been incorporated into the continent, and that the deposits of this ocean form part of the highest mountain ranges.

It may be remarked that within the eastern Alps Mesozoic limestones of different ages contain deep-water radiolarian chert. But the great and well-bedded masses of white Rhætic limestones of Austria betray distinct proofs of a continuous rising of the shore-line. It is also true that certain bright red enclosures within these white limestones seem clearly not to be red deep-sea clay, but true *terra rossa*, formed by atmospheric decomposition of the limestone; so that these beds must have formed reefs in the ocean. Therefore it is at present difficult to say whether in the Alps the Tethyan Ocean did at any time attain the total depth of, say, 2,000 fathoms; or whether deposits followed so rapidly and depression was so continuous that this was not the case.

The later Tethyan history, the recapitulation of the vicissitudes which led to the formation of the existing Mediterranean, forms certainly one of the most attractive chapters of historical geography. Marine deposits of Mediterranean type (Erste Mediterranstufe, Miocène inférieur) enter the Rhone Valley, surround the present site

of the Alps, and continue far away to the East, to Persia, and have been met with by Griesbach near Balk, on the Oxus. Then these deposits were folded, in the Taurus, in Asia Minor, in Switzerland. Afterwards came the sagging-down of certain parts of these folds, near Vienna, in Hungary, &c., and all that varied series of consequent events. After the first Mediterranean came the formation of an immense horizon of salt deposits, stretching from Wieliczka to Persia; then a second Mediterranean reaching far into the newly-formed depressions; then the appearance of vast fresh-water lakes, lasting through a long period of time till the breaking down of the Ægean land and the re-conquest of the Black Sea.

Look at smaller examples of such partial subsidences; see Margerie's instructive paper on the Corbières, showing the sinking down of the Pyrenees, Miocene beds passing beyond Narbonne, while south of Cape Leucate two more recent Pliocene "effondrements" form the Rousillon, described by Depéret, and the Golfe de Rosas.

But this is only part of the Tethyan history. Michelin's and Duncan's palæontological studies in the West Indies have revealed the European character of certain deposits. It is the "Gosau type" of the Cretaceous which appears in Jamaica, and the Castel-Gomberto horizon of Upper Oligocene (warm type of Sables de Fontainebleau) is known in several other isles. In regions still further off, one of our first masters, the venerable Dr. Philippi, has shown that the present molluscan fauna of the Chilian coasts is of quite recent origin, and that until the beginning of Quaternary times the European Mediterranean molluscan types stretched far down the western coast of South America. At the same time the Mesozoic deposits of Chili, and those recently discovered at Taylorville in California,[1] show purely European characters, and the Neocomian of Begotá is the exact equivalent of that of Barrême.

These facts teach us that an ocean-bed existed, but that some coast-line, maybe only an interrupted line, once stretched across the present Atlantic, and permitted the Gosavian and Oligocene corals, and the Miocene shells also, to cross. I do not overlook the fact that Dr. Philippi himself, struck by the analogies existing between the flora of Chili and that of Europe, recently refused to accept the hypothesis of a "bridge" to Europe, and preferred to suppose that identical climatic and other external causes produced analogous and even identical species of terrestrial plants. I refer to what has been excellently said by Mr. Blanford on this theory, a few years ago, in his address to the Geological Society of London. I believe that the parallel correspondence of the marine faunas up to the Quaternary period gives a more correct clue to the correspondence of the existing terrestrial floras in Chili and in Europe.

So I think that we must not only concede the extinction of a great

[1] See Hyatt and Dillen on the Jura and Trias at Taylorville, California. *Bull. Geol. Soc. America*, 1892, pp. 369—412.

Palæozoic, Mesozoic, and Tertiary ocean in south-western Eurasia, but admit also great recent changes in the middle or southern Atlantic. Geological evidence, therefore, does not prove, nor even point to, a permanence of the great depths, at least in the oceans of the Atlantic type.

Let me remark in a few words that, although I believe in the possibility of the formation of large new depressions, I do not hold with the old opinion, lately taken up again by M. Faye, that the continued sinking of the ocean beds may force chains of mountains to appear all round. This view could only be propounded for the Pacific basin; but the Pacific chains are folded in the direction *towards* the ocean, and not *from* the ocean. They are easily divided into arches, each of which presents the *convex* side to the ocean, so that the Pacific everywhere presents the character of a "vorland."

Let me, at the end of this long note, allude to a broad biological fact. In the higher beings we see *lungs* always preceded by gills; so it is even with the human child. The adaptation for breathing our atmosphere is of a later date; and we conclude that the whole terrestrial air-breathing fauna is a *derived* fauna, derived from amphibious forms quitting shallow water. This fact evidently points to a long existence of dry land, long enough to permit this accommodation to be effected; the accommodation clearly has been going on since Palæozoic times. Still there exists no proof that individual continents always remained the same, and we even know for certain that such was not the case with by far the greater part of these continents.

A similar lesson is also taught by the *eyes* in all the higher organised beings of the deep sea. The optical apparatus of abyssal species is profoundly modified by the exceptional environment, while the normal types of eyes are met with in the same genera within moderate depths. Therefore, we must also regard these deep-sea forms as *derived* forms. The blind and blinded Trilobites of Cambrian beds, the blind Trinuclei and the widely-expanded eyes in certain species of Aeglina in Lower Silurian strata teach the same lesson. At the same time, they show that deep-water must have existed over Bohemia, and over a good many other Palæozoic tracts, and that the depths were considerable enough to call forth these same abyssal metamorphoses of the eye.

We might, therefore, rather be induced to infer that in Prepalæozoic times there may have existed a universal hydrosphere or panthalassa covering the whole of the planet. Only with the first appearance of dry land began the deposition of clastic sediments. The higher forms of life may have been developed in waters of moderate depth and may successively have spread to the sun-lit continents, and to the dark depths, while the slow elaboration of the existing inequalities of the terrestrial surface was going on.

But this elaboration is still in progress. I believe with Reyer,

Fisher, Jukes, and many others that the great depths are mostly covered with volcanic products, with lava and ashes forming immense plains and overlain eventually by the deposits of the abyss; but I see no reason why parts of the ocean or even of the dry land may not to-morrow sink to form new depths, or why we should believe that all the great ocean basins have been continuously covered by water since panthalassic times. So far as the Atlantic is concerned, there even exists some evidence to the contrary.

But all this is unripe fruit. Our scholars will some day know more than their masters do now; so let us patiently continue our work and remain friends.

EDW. SUESS.

III.

The Origin and Classification of Islands.

EVERY island has its history, and in the case of all but newly-formed volcanic islands or coral islets, every island has a double history, that of the island itself and that of its colonisation by the plants and animals which live upon it. The rocks of which an island consists will give us an insight into, though not always a complete knowledge of, its geological history; and a study of its living inhabitants will generally enable us to decide whether it has been colonised as an island or by direct former connection with a continent. Some biologists maintain that the fauna of an island will show whether it has ever been united to a continent or not, and this is the question which I propose to discuss in the present paper, because the answer to it involves some important inferences and conclusions.

There are many ways in which an island can be formed; it may be but a portion of a continent severed from the mainland by the erosive action of the sea; it may be the mountainous part of a country which has sunk beneath the ocean; it may have been thrown up from the floor of the ocean by volcanic action, or it may have been built up by the growth of reef-making corals. There are, however, only two ways in which an island can have been populated without the intervention of man; either it must once have been united to a continent and its inhabitants must be the descendants of those that then lived on that continent, or else its tenants must have been transported across the sea by the help of drift-wood, or by birds, or by winds and storms.

It is evident, therefore, that *in most cases* there is likely to be a certain relation between the geological structure of an island and the nature of its fauna and flora. Islands formed in the ocean, whether by direct upheaval, or by volcanic eruptions, or by coral growths, are not likely to possess a large assemblage of plants or of animals; they may be covered with vegetation, but the animals found on them must be the descendants of occasional waifs and strays. On the other hand, an island which has once been part of a continent will, if it remain large enough, continue to support a large number of animals, and these will generally include a certain number of Mammalia and Amphibia.

Islands have consequently been divided into two great classes—

oceanic and continental—which are defined by Dr. A. R. Wallace in the following terms :—

Oceanic islands are "of volcanic or coralline origin, usually far from continents, and always separated from them by very deep sea, entirely without indigenous land mammalia or amphibia, but with a fair number of birds and insects, and usually with some reptiles."

"Continental islands are always more varied in their geological formation, containing both ancient and recent stratified rocks. They are rarely very remote from a continent, and they always contain some land mammals and amphibia, as well as representatives of the other classes and orders in considerable variety."[1]

As general definitions framed for the purpose of describing the conditions which have governed and limited the geographical distribution of animals, these sentences are doubtless sufficiently accurate, especially as Dr. Wallace admits there are some islands which do not come very clearly under either of these categories; but he proceeds to lay down a *canon* the truth of which is by no means so apparent. He says :—"The total absence of warm-blooded terrestrial animals in an island otherwise well suited to maintain them, is held to prove that such island is no mere fragment of any existing or submerged continent, but one that has been actually produced in mid-ocean. It is true that if a continental island were to be completely submerged for a single day, and then again elevated, its higher terrestrial animals would be all destroyed, and if it were situated at a considerable distance from land, it would be reduced to the same zoological condition as an oceanic island; but such a complete submergence and re-elevation appears never to have taken place, for there is no single island on the globe which has the physical and geological features of a continental, combined with the zoological features of an oceanic island."

Seeing how little we yet know of the geology of distant islands, this is a statement which further knowledge may at any time disprove, and there is, even now, good reason to believe that it is contrary to facts. If this assertion can be proved to be incorrect, I shall claim to reverse Dr. Wallace's argument, and to maintain that inasmuch as an island does exist which combines the geological features of a continental island with the zoological features of an oceanic one, then we may assume that the submergence and re-elevation of a continental island can take place, and, consequently, the absence of mammals in an island *cannot* be held to prove that it has never been united to a continent.

Considering the many subsidences and upheavals which are known to have occurred along the borders of continental areas since the beginning of Tertiary time, it would indeed be strange if some tracts, isolated by subsidence, had not been completely submerged for a time, and afterwards raised afresh from the sea. The West Indian

[1] "Island Life," by A. R. Wallace, second edition, 1892, p. 243.

region is one where such an occurrence is very likely to have happened, for deep-water deposits of late Tertiary age occur in many of the islands; while the raised coral-reefs which are found in the same islands, and reach up to a height of 1,800 ft. above the sea, prove that there has been recent upheaval to at least that extent.

Many of the smaller islands are volcanic, and may have been thrown up at any time; but Barbados, the most westerly of all the islands, has just the features of which we are in search; in its faunal aspect it is decidedly oceanic, while its geological structure is a curious combination, being partly continental and partly oceanic. The facts of the case are so remarkable that a brief review of them may here be given.

Barbados stands on a submarine bank or ridge which slopes away in every direction till a depth of more than 1,000 fathoms is reached. The core and base of the island consists of stratified rocks, ordinary sandstones, clays, and limestones, such as are formed in shallow water near a coast-line where rivers of some size carry detritus into the sea, and these strata must have been deposited very near such a shore, for many of the sandstones are composed of large quartz grains, which would not be carried far from land. Above these shallow water strata lie deposits of a totally different character, consolidated oceanic oozes, like those which are now found only in the deeper parts of the ocean, and are known as Globigerina Ooze, Radiolarian Ooze, and Red Clay. All these kinds of ooze occur in Barbados, and there is not only a superficial resemblance between them and the modern oceanic oozes, but a complete identity of structure, and a close analogy in chemical composition; upheaval and exposure to rain and weather have, of course, effected some little alteration, but have not obscured their structure.

It is certain, therefore, that the shallow sea and the extensive shore-line which it bordered sank to a very great depth, certainly to more than 1,000 fathoms, and probably to as much as 2,000 fathoms (12,000 feet). The site of Barbados was then part of the ocean-floor, but after a time upheaval took place, and it was gradually raised till it came within the sphere of reef-building corals; a small coral islet was the result, but as the upheaval continued, the earliest reefs were raised above the sea, and the area of the island was gradually enlarged. This process went on till the island attained its present dimensions (about the size of the Isle of Wight), the soft oceanic deposits and the still older sandstones and clays, being protected from the erosive action of the waves by a thick coating of coral rock, except over a small area in the north-west part of the island, where the rain has cut deep valleys through the stratified rocks, and by carrying sand and mud into the sea has prevented the growth of continuous coral reefs on that side.

Now, an island with such a history must necessarily have received its present fauna and flora in the same casual way as an

oceanic island that had never formed part of a continental area. Accordingly, though Barbados is exceedingly fertile, and though the island when first discovered was clothed with forest and underwood, its native terrestrial fauna is a very small one.

There are only two mammals in Barbados which have been supposed to be indigenous, a monkey and a racoon-like animal, but I am informed by Col. Fielden that the monkey proves to be the Green Monkey of Western Africa, *Cercopithecus callitrichus*, and the "racoon" is a South American animal (*Procyon cancrivorus*). The monkey was doubtless brought over in slave-ships, and as it is known that the Caribbean Indians frequented the island before it was colonised by Europeans, and as the early settlers had intercourse with the colonists of Guiana, it is quite possible that the Procyon was introduced by man.

There are no indigenous Amphibia, but there are two Snakes and four species of Lizards. One of the snakes is a species peculiar to Barbados, the other may have been introduced by human agency from some of the other islands. Of the lizards, three are South American species, and the fourth is found in all the Lesser Antilles, though it is not yet known from South America. The manner in which reptiles may be landed on an island like Barbados is illustrated by the case recorded by Col. H. W. Fielden in the "Zoologist" of 1888, p. 236; this was the landing of an alligator on the shore from a floating tree-trunk, actually witnessed in 1886; it had doubtless been transported from one of the great South American rivers, but it was promptly dispatched by those who witnessed its arrival.

In a paper on the birds of Barbados, Col. Fielden remarks that, so far as he can judge, "the mammals, reptiles, and land molluscs owe their introduction either to ocean-currents, accidental occurrences, or to the direct agency of man, and a review of its avi-fauna does not point to a different conclusion." He also speaks of Barbados as a "truly oceanic island in the sense of its never having formed part of a continent since the introduction of its present meagre fauna," nor "since it emerged as a coral-reef from the ocean." This is perfectly true, but yet it does not come under Wallace's definition of an oceanic island for the reason already stated.

The difficulty of drawing hard and fast lines between oceanic and continental islands is also illustrated by the structure and fauna of the Seychelles Archipelago, in the Indian Ocean. These islands are surrounded by water of more than 1,000 fathoms, and are 850 miles distant from the coast of Africa. They might, therefore, be expected to exhibit all the features of oceanic islands; the facts, however, are as follow: The larger islands consist entirely of granite, and granite is a deep-seated rock which can only be exposed by the prolonged and repeated processes of erosion which take place on large areas of land. Dr. Wallace admits them to be remnants of

"a very extensive island," though he doubts whether this island ever formed part of a continent.

Turning to their zoology, we find that they are entirely destitute of mammals, but that they possess Amphibia, having two species of frogs and three species of Cæcilia, snake-like creatures, which burrow underground in the manner of worms. Now, it seems impossible to explain the presence of these Amphibia unless, at some remote period, the islands formed part of a continent, for salt-water is fatal to them, and destroys even the eggs of frogs. On the other hand, if the former connection with a continent be admitted, what can be said of the absence of mammals, for one would have expected that some of the smaller genera, such as rats, mice, civets, lemurs, and insectivores would have survived. Dr. Wallace suggests that the islands have at some time been so nearly submerged that the portions remaining above water were too small to support the existence of the smallest mammals. If this be the explanation, and if we accept the evidence of the Amphibia as to ancient continental connection, then the absence of mammals in such islands cannot be taken as proof that they have never been part of continental land.

New Caledonia, again, in the Pacific, is regarded by Dr. Wallace as an oceanic island. There are no indigenous Mammalia or Amphibia, the solitary frog being known to have been introduced. There are several peculiar lizards and a snake (one of the Boas); and the island is separated from neighbouring groups by water of more than 1,000 fathoms deep. Notwithstanding this limited vertebrate fauna there is evidence that New Caledonia has once been part of an extensive land area. Stratified rocks, believed to be of Secondary and Tertiary age, enter into its geological structure, and there is a genus of land snails (*Placostylus*) which occurs in the neighbouring archipelagoes, as well as on Lord Howe's Island and in New Zealand. Hence it has been recently argued that the unity and limitation of the *Placostylus* area can only be explained by the supposition that these islands are portions of a broken-up and submerged continent which was distinct from Australia. To reconcile such a supposition with the absence of mammals, it is only necessary to assume that this continent dates back to a time when mammals were not in existence in the Pacific region, just as Australia dates from a time when only marsupial mammals were in existence.

The cases above mentioned show that there is no constant relation between the geological structure of oceanic islands and the manner in which they have received their present inhabitants, for there are islands which are not far from a continent, and have clearly formed part of the continental area within the limits of Tertiary time, and yet are without any indigenous Mammalia or Amphibia. There are other islands which are destitute of Mammalia and are geographically oceanic, but nevertheless, have a geological structure of continental type. In other words, there are islands which testify to the former

existence of continents in what are now oceanic areas; and when all the facts are considered it is seen that they are opposed to the extreme views regarding the permanence of oceans and continents which have been accepted and maintained by Dr. Wallace in his "Island Life."

It would appear, therefore, that if the division into continental and oceanic islands be retained, a fourth class must be admitted into the classification; not only must continental islands be divided into ancient and modern, but oceanic islands must be defined geographically and then divided into those which are the remnants of sunken continents and those which are of recent origin. It will, however, be very difficult to distinguish islands of recent formation from mountain peaks which have been submerged and again elevated above the ocean level, for lofty mountain peaks so often consist of volcanic rocks.

I am convinced that the attempt to exclude islands in which stratified rocks occur from the category of oceanic islands will only lead to confusion and misconception, and that it is a mistake to infer from "the absence of warm-blooded terrestrial animals in an island otherwise suited to maintain them," that the island is of recent formation. The characters of an island fauna may, perhaps, be relied on to show whether the island has been *colonised* by former connection with a continent or not, but beyond this it will be no guide to the geological history of the region. The biological evidence must simply be taken for what it is worth, and the geological history of the island must be read from its geological structure, without the bias given by any preconceived theoretical ideas about the permanence of oceans and continents. There is, in fact, no hard and fast line to be drawn between oceanic and continental islands.

A. J. Jukes-Browne.

II.

NOTE ON MR. JUKES-BROWNE'S PAPER.

The Editor having kindly sent me a proof of Mr. Jukes-Browne's paper, I beg to make a few remarks thereon.

I cannot but think that Mr. Jukes-Browne's criticism of the Darwinian classification of islands, which I have adopted and more fully developed, is rather one of words and definitions than of realities. The very terms of the classification—"Oceanic" and "Continental"—show that it is a broad and wide-reaching one; and its main implication, the permanence of oceanic and continental areas, is equally broad and fundamental. That there should be islands situated upon the ever-fluctuating margin of these two areas which are difficult to class, or which may, at different geological periods, have possessed the characteristics of "oceanic" or of "continental" islands, is what might certainly be expected; the wonder is that there are so very few of them. Barbados is, technically, an oceanic island; but it is

situated upon the old sea-margin of the American continent, and a portion of that old continental margin forms the base of its oceanic deposits. I recognised the possibility of such a base for some apparently oceanic islands in the passage quoted by Mr. Jukes-Browne, but was not then aware that any such existed. Of course, if old stratified rocks could be shown to form the base of any of the mid-oceanic islands, the whole classification, and the theory which is founded on it, might be imperilled; but this has not yet been done.

It is evident that, with island groups whose components vary in size from many thousands of square miles to small sea-washed rocks, all definitions must be taken broadly and as applying to the group. Even among the pre-eminently continental British Isles there are many hundreds—out of the thousand of which they are said to consist—which have neither mammalian, amphibian, nor reptilian inhabitants; but it will hardly be objected that such cases as these upset the biological definition of continental islands. In the same manner the Seychelles are classed as belonging to the Madagascar group, and are, therefore, ancient continental, while Mauritius, Bourbon, and Rodriguez are true oceanic islands.

I do not know why Mr. Jukes-Browne should say that I regard New Caledonia as an oceanic island. At p. 473 of *Island Life* I refer to it as probably once connected with New Zealand; and again, at p. 485, I suppose it to have once formed an extension of New Zealand, which, though in some respects anomalous, has all the main characteristics of a continental island.

Looking at the question broadly, as a generalisation applying to all the well-marked islands and island-groups of the globe, I entirely deny the validity of the conclusions expressed in the last three paragraphs of Mr. Jukes-Browne's paper, conclusions which are founded exclusively on islands situated upon the margin of the continental area.

ALFRED R. WALLACE.

IV.

Biological Theories.

III.—THE RECAPITULATION THEORY.

DIRECT observation has shown that, when an animal species *varies* (*i.e.*, *becomes* unlike what it was before) in adult structure, those stages of the development which are nearest the adult undergo a similar but usually smaller change. This is shown in domestic species by the observations of Darwin, and the result is in exact harmony with the well-known law of Von Baer, which refers to natural species, both nearly related and very widely dissimilar.

Von Baer's observations as well as Darwin's, and as well as those of every student who has ever compared the embryos of two vertebrate-species, may be summarised as follows:—

Animals which, though related, are very unlike in the adult state, resemble each other more closely in early stages of development, often, indeed, so closely as to be indistinguishable in those early stages. As development proceeds, *in such species*, the differences between the two embryos compared become more and more pronounced.

If similar comparisons could be instituted between an ancestral species and its much modified descendants, there is no reason for doubting that a similar result would be reached. This, indeed, has been done in the case of some breeds of pigeons, which we have excellent reasons for believing to be descended from *Columba livia*. True, *C. livia* is not a very remote ancestor, but I do not think that will vitiate the argument. Let me quote Darwin verbatim: "As we have conclusive evidence that the breeds of the Pigeon are descended from a single wild species, I have compared the young within twelve hours after being hatched; I carefully measured the proportions (but will not here give the details) of the beak, width of mouth, length of nostril and of eyelid, size of feet, and length of leg in the wild parent-species, in pouters, fantails, runts, barbs, dragons, carriers, and tumblers. Now some of these birds, when mature, differ in so extraordinary a manner in the length and form of beak, and in other characters, that they would certainly have been ranked as distinct genera if found in a state of nature. But when the nestling birds of

these several breeds were placed in a row, though most of them could just be distinguished, the proportional differences in the above specified points were incomparably less than in the full-grown birds. Some characteristic points of difference—for instance, that of the width of the mouth—could hardly be detected in the young. But there was one remarkable exception to this rule, for the young of the short-faced tumbler differed from the young of the wild rock pigeon and of the other breeds in almost exactly the same proportions as in the adult state." ("Origin of Species," 6th edition, p. 392.)

As, it seems to me, would be expected, the differences between two more-distantly related animals at a stage corresponding to the time of hatching in pigeons is very much greater. A bird is obviously a bird at the time of hatching, and often the species and even the breed (as in the case just quoted from Darwin) are readily recognised at that time. A lizard or other reptile at the corresponding stage is easily recognised as a reptile. As shown by Von Baer the distinction between the bird and the reptile becomes more and more difficult as we examine earlier and earlier stages, and in the earliest stages, *i.e.*, in the ovum, it is often quite impossible to say even to which great phylum of the animal kingdom the ovum belongs.

The variation which produced birds from reptilian ancestors may be taken as a type of the kind of variation which is most familiar to us, *i.e.*, the variation affecting chiefly the adult animal.

Another kind of variation is illustrated by the three genera of gnats, *Culex*, *Corethra*, and *Chironomus*. Whatever may have been the form and structure of the most recent common ancestor of these three, the variation which has produced these from that ancestor appears to have affected the larval stages more markedly than the adult stages. These three are quite unlike in appearance in the larval stage. The mode of formation of the imago within the larva in *Chironomus* is quite unlike that in *Culex*. The three larvæ differ not only in appearance, but in internal structure. In the adult stage, though easily distinguishable, they are very much more alike in form, and even in internal structure, than in the larval stages.

We may not know the exact course of evolution of these three, but we may at least say that the variation in the average structure which has occurred in their evolution has led to a greater difference in larval structure than in adult structure.

Two kinds of variation must, therefore, be recognised—that affecting chiefly the adult structure, and that affecting chiefly the structure of the individual in early stages of development.

My object now is to show that in neither case can a record of the variation at any one stage of evolution be preserved in the ontogeny, much less can the ontogeny come to be a series of stages representing, in proper chronological order, some of the stages of adult structure which have been passed through in the course of evolution.

This would seem to be obvious enough, and there would appear to be no need for any argument to establish the view here put forward, viz.:—that the ontogeny is not an epitome of the phylogeny, is not even a modified or "falsified" epitome, is not a record, either perfect or imperfect, of past history, is not a recapitulation of the course of evolution. My excuse for urging what appears to me to be an obvious fact, is the existence of a statement the direct opposite of this view in almost every zoological text-book which has been published within the last fifteen or twenty years, as well as the frequent urging of the recapitulation theory in our colleges and universities, in the University Extension Lectures, and in a recent lecture given to the British Association, and even in the presidential address of one of our most eminent embryologists to the Biological Section of that Association.

Incidentally, it may be well to point out the restricted sense in which I use the word "variation." To vary, is, I believe, to *change* or to *become unlike*, whatever the thing varying was previously like. To *be unlike* is *to differ*; *not to vary*. The unlikeness observable among the members of a species is variety, or difference, or unlikeness, but it is not variation. Variation is change in the average constitution of successive generations of a species leading to the production of a new species, or race, from an old one. I have myself used the term in another sense in a previous paper in this series, but that is no reason why I should continue to use it in a sense which is liable to lead, and indeed has led, to confusion.

The terms "vary" and "differ" as above defined are not even partially synonymous. Difference and variation are respectively statical and dynamical (if I may use that term to express the opposite of statical).

In order that any structure of the adult which varies, and hence ceases to exist as an adult structure at all, may become an ontogenetic record of that adult structure, it is necessary that variation should occur in a way utterly unlike the way in which it does actually occur. The more the adult structure comes to be unlike the adult structure of the ancestors, the more do the late stages of development undergo a modification of the same kind. This is not mere dogma, but is a simple paraphrase of Von Baer's law. It is proved true, not only by the observations of Von Baer and of Darwin, already referred to, but by the direct observation of everyone who takes the trouble to compare the embryos of any two vertebrates, provided only he will be content to see what actually lies before him, and not the phantasms which the recapitulation theory may have printed on his imagination.

In order to produce a "record" it is necessary that new chapters be added at the end of the pre-existing record. It is necessary, in fact, that as the adult structure varies in one direction, the late stages of development shall vary in another, so as to become, not more like the new adult structure than they were before, but more like the old

one. If the variation is a mere increase in the definitive size of an organ which has hitherto not only increased in size in the evolution of the species, but also has *grown* during development of the individual, then, of course, a variation affecting the rudiment of this organ at all stages of development in which it is present, and causing it to grow more rapidly, will render the late stages of development (as far as that organ is concerned) more like the adult stage of the ancestor. This, however, is a mere result of the mode of growth of the particular organ; its smaller size in late embryonic stages than in the adult is not a result, still less is it a "record" of the course of evolution.

The antlers of stags will serve as an example. Each stag develops a new pair of antlers in each successive year, and each pair of antlers is larger than the pair produced in the previous year. This yearly increase in the size of the antlers has been put forward as an example of an ontogenetic record of past evolution. I, however, deny that it is such a record. The series of ancestors may have possessed larger antlers in each generation than in the generation before it. It is not an occasional accidental parallelism between the ontogeny and the phylogeny which I deny, but the causal relation between the two. Had the ancestors had larger antlers than the existing ones, there is no justification for the assumption that existing stags would acquire antlers of which each pair, in later years, would be smaller than those of the previous year. The yearly increase in the size of the antlers is itself a character determined by Natural Selection. Phylogeny appears to have run parallel with the *pre-existing* Ontogeny. There are many breeds of hornless sheep, but they do not bear large horns in early years and then shed them. If a rudiment ever appears in the embryo of such sheep, its growth is very early arrested.

So it is in all the alleged cases of recapitulation. The gill-arches and clefts, and the blood-vessels of an embryo bird or mammal, present that striking resemblance to the corresponding parts of the *embryo* of a fish which is expressed in Von Baer's law. They differ, perhaps, only very slightly indeed. They are the modified representatives of the embryonic structures of a common ancestor. Whether they were, in that ancestor, the rudiments of gill-arches and clefts, &c., like those of an adult fish, or not, cannot be decided by embryological study. All we learn is that what now serve in their modified forms as the rudiments of the gill-arches of a fish, and of certain parts about the throat of a bird or a mammal, are so similar in early stages of development as to show that these parts are homologous. The greater resemblance of them to the adult structures of a fish than to those of a mammal may justify the belief that they served in a common ancestor as the rudiments of adult structures more like the adult structures of a fish than of a mammal, and that is all: they do not prove even that.

The statement that this stage in the development of a bird or a mammal is the modified remnant of the *adult* structure of the ancestor; and that the ontogeny is even in part made up of a series of remnants of ancestral *adult* structures arranged in chronological order, is not only unjustified, but is demonstrably false. If any generalisation in the whole science of zoology is borne out by fact, it is the law of Von Baer with reference to animals living free only in later stages of development. That law claims a parallelism between the development of a fish and of a bird which is quite inconsistent with the recapitulation theory, and completely consistent with the observed facts.

The early stages of the fish embryo are very like those of the bird embryo. These two *do* correspond to each other. The statement that the embryonic structure of a bird follows a course which is, from beginning to end, roughly parallel with, but somewhat divergent from, the course followed by a fish, is borne out by the actual facts. A bird does not develop into a fish and then into a reptile and then into a bird. There is no fish stage, no reptile stage, in its ontogeny. The adult resembles an adult fish only very remotely. Every earlier stage resembles the *corresponding earlier stage* of the fish more closely. There is a parallelism between the two ontogenies. There is no parallelism between the ontogeny and the phylogeny of either a bird or any other animal whatever. A seeming parallelism will fall through when closely examined.

Each transient stage in the development of any individual is a modification of the corresponding stage of development of its ancestors. It is in no case a modification of the adult stage of the ancestor. The adult stage of a bird, and no other, corresponds to the adult stage of the fish-like ancestors (if it ever had such ancestors).

The stalked "pentacrinoid" larva of *Antedon* (=*Comatula*) is a modified equivalent of the stalked *larva* of the "pentacrinoid" ancestor (if ever there was such an ancestor), and not the modified equivalent of the adult ancestor. The possession of a stalk in early stages of development appears to be an advantage, and hence the specific constitution which determines the development through a stalked stage has been preserved by Natural Selection. There is no evidence whatever to justify such mystical conceptions as those involved in even the most reasonable forms in which the recapitulation theory has been applied to this case.

The promise which the theory gave of serving as the guide to knowledge of past history, without the labour involved in palæontological research, was, indeed, tempting: and when the "royal road to learning" had been shown by it, it is not surprising that some zoologists should have entered for the race along this road. To what goal that road has led may be learned by a comparison of the numerous theories as to the ancestry of "chordata" which have been put forward by those who adopted the theory without enquiring as to its validity.

In this connection it may be not out of place to conclude with a quotation from Fritz Müller :—

"A false conception, when the consequences proceeding from it are followed further and further, will sooner or later lead to absurdities and palpable contradictions." (Opening sentence of chapter ii., "Facts for Darwin.")

That is precisely what the "recapitulation theory" has actually led to.

C. HERBERT HURST.

V.

Recent Observations on Fertilisation and Hybridity in Plants.

THE object of the following paper is to bring before the readers of NATURAL SCIENCE the most recent results which have been attained by workers in that department of Plant Physiology which relates to the actual process of the fertilisation of the female by the male element, and of the secondary processes by which the access of the active to the passive element is assisted. The enquiry naturally divides itself into two branches: (1) The nature of the process itself; (2) The subsidiary phenomena. The first of these enquiries evidently goes to the foundation of the laws on which depends the succession of life on the earth. Observations on this head, to have any value, must be carried out by experts of great knowledge, and with trained skill in the use of the most delicate microscopical appliances; the lowest forms of life, as well as the highest, must be put under requisition to yield up their secrets. As we ascend in the scale of organised beings, the various vital processes become more complicated, and secondary phenomena play a larger part in them; and it is in Flowering Plants that these become most interesting and most within the scope of the non-scientific observer. As, therefore, this will be the most familiar branch of the subject to those of our readers who have not made the physiology of plants their special study, we propose to commence with it.

The process by which the ovule of Flowering Plants is fertilised by the pollen-grain—*i.e.*, is acted on so as to be enabled to produce an embryo, without which no seed can germinate—is sufficiently well-known. The pollen-grain (the male element) must first be deposited on the stigma, where it puts out a pollen-tube which penetrates into the embryo-sac of the ovule, and here, coming into contact with one of the embryonic vesicles (the female element) fertilises it, the result being the growth of the embryo within the embryo-sac. The subsidiary phenomena are here the conveyance of the pollen-grain to the stigma, and the entrance of the pollen-tube into the embryo-sac. In the last-named process a great uniformity prevails throughout the Angiosperms or larger section of Flowering Plants; the only important exception being the

very interesting one recently discovered by Treub in the case of the Casuarineæ; but as that has already been fully described in these columns,[1] we will not further allude to it. Much greater variation exists in the phenomena connected with pollination, or the carriage of the pollen-grains to the stigma.

There is no more fertile source of error than the too rigid or too universal application of general laws. It is perfectly true that Nature "works not by partial, but by general laws." But a natural law is not, as some seem to suppose, an external force which directs the phenomena of life, it is simply a generalising of the phenomena themselves. A certain course of proceeding is of advantage to a particular organism or set of organisms; in other words, they are governed by a general law; but in some special case the same end is best attained by other means, and we find what is generally termed an exception to the general law. In no department of physiological botany is this variation more strikingly exhibited than in the phenomena of pollination. It was in 1793 that Christian Konrad Sprengel's celebrated work *Das entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen* was published. Centenaries have been celebrated on more trivial grounds than the appearance of this "epoch-making" work; since in it was first clearly laid down the law that in a large number of flowers the structure and arrangement of the male and female organs render self-pollination almost impossible, understanding by this term the pollination of the stigma by pollen-grains from anthers in the same flower. It is a matter of general knowledge how greatly this law was exemplified and extended by Darwin and his followers; but here again the tendency to a too universal application of the law has manifested itself, and during the last few years a remarkable number of observations have been made which indicate that self-pollination is far more general than had at one time been supposed. The perfume and the bright colour of flowers are undoubtedly important agents in attracting insects to assist in cross-pollination; and it has been assumed that this must always be their function. But that this is not invariably the case is certain. It has long ago been pointed out that the bee-orchis, *Ophrys apifera*, the flower of which so remarkably simulates a bee, is not visited by insects, and yet produces seeds abundantly. Very curious also are the facts in connection with the genus *Aristolochia*, in which the conspicuous pitcher-shaped perianth has been regarded as a contrivance for attracting insects and ensuring self-pollination. The walls of the pitcher are clothed with woolly or glutinous hairs, which have been assumed to have the function of detaining the pollen brought by the insects which enter the pitchers in great numbers from other flowers, and thus enable them to reach the stigma. W. Burck states, however, as the result of observations

[1] NATURAL SCIENCE, vol. i., p. 132.

on several species of *Aristolochia*, that when flies enter the chamber formed by the lower part of the perianth, and become dusted with pollen, in their efforts to escape again from the chamber they come into contact with these hairs, and thus lose, before they pass to another flower, almost every grain of pollen. In *A. barbata* at least 600, and in *A. ornithocephala* about 6,000 pollen-grains would be required for the fertilisation of all the ovules in an ovary.* Out of a very large number of flies captured in these chambers, only a very few showed the presence of even a small number of pollen-grains adhering to them. On the other hand, both these two species and *A. elegans* are abundantly fertile when pollinated with their own pollen. He concludes, therefore, that these species at least are self-pollinated. Herr Burck also points out the interesting fact that in *Aristolochia* the true stigma and style are abortive, and that the so-called stigmatic surface consists of the connectives of the anthers which have coalesced by their sides into a cup, are provided on their margins with papillæ, and have assumed the functions of a stigma. In the instances just named, and in others of the same kind, it is an allowable hypothesis that the bright colour or sweet scent of the flower is a survival from cross-pollinated ancestors which has now lost its meaning. But this will not apply in other instances. The hazel and the larch have, it is true, unisexual and therefore necessarily cross-pollinated flowers. But they are not visited by insects, and are universally regarded as typical examples of wind-fertilised plants. What, then, is the object of the bright red colour of the styles of the one and of the scales of the opening cones of the other? It cannot be a survival, since the Corylaceæ and the Coniferæ are both unquestionably archaic forms of life. Bright colours, indeed, appeared very early in the evolution of plant-life. How are we to explain the brilliant red of the sporange of *Sphagnum*; or the bright pigment of the antherid or "globule" of the Characeæ; or the brilliant colour of species of *Peziza* or *Boletus*? Does Nature love beauty for its own sake?

Prominent among those who advocate the anti-Darwinian view of the prevalence of self-pollination, is Mr. Thos. Meehan, of Philadelphia, a botanist of great experience in the cultivation of plants. He adduces a large number of American plants in which he asserts this mode of pollination to be the rule, among others, in *Amsonia Tabernæmontana*, belonging to the Apocynaceæ, in which the flowers are showy and abundantly fertile, but their structure is such that no insect, not even a thrips, can gain entrance to the nectary. The mouth of the corolla-tube is so densely matted with hairs that if the pollen-clothed tongue of an insect were thrust through the mass, it would be completely cleansed; nor is there any room for the tongue to pass the capitate stigma. To effect pollination the anthers curve over and rest upon the stigma. Other examples of habitual self-pollination are given in *Symplocarpus fœtidus*, belonging to the

Aroideæ, in which the flowers are frequently proterogynous and as frequently proterandrous; in *Portulaca pilosa*, the flowers of which open only in the sunshine, and yet seed abundantly when grown in the shade; in *Cuphea Zimpani* (Lythraceæ), *Lopezia coronata* (Onagraceæ), in several species of *Lonicera*, *Phytolacca decandra*, *Lycopersicon esculentum*, *Hamamelis virginica*, &c. A large number of the American Compositæ he asserts to be self-pollinated. He has observed pollen-tubes entering the clefts of the bilobed stigma before it opens; and the pollen-grains, even when large and brightly coloured, frequently fall on the stigma before any insect can possibly enter the flower. Mr. Meehan, indeed, goes so far as to say that whenever a species is unusually productive, he finds, as a rule, arrangements for self-pollination. An Italian botanist, Dr. Terraciano, states that in many species of *Nigella*, notwithstanding the conspicuousness of the flower and the presence of nectaries, the structure is adapted for self-pollination. According to Warming, all the Greenland and Iceland species of *Euphrasia* are self-pollinated. Even so decided a Darwinian as Professor Caruel, of Florence, is inclined to doubt whether too much has not been taken for granted in the prevalent theory of the part played by the bright colour and sweet scent of flowers in attracting insects, seeing that, as far as we know, the visual and olfactory organs and perceptions of many animals are very different from our own. In particular, he calls attention to Sir John Lubbock's observations on the different effect of colours on many animals from that which they produce on us; to the eyes formed of facets and to the ocelli of insects, and to their sensitiveness to the ultra-violet rays of the solar spectrum. Rosen asserts that even with many wind-pollinated plants, such as *Carex* and *Festuca*, self-pollination is the rule.

On the other hand, the orthodox view receives support from a very large nnmber of fresh observations which reveal adaptations clearly intended to promote cross-pollination by the agency of insects. In a series of papers in the *Botanical Gazette* and elsewhere, Mr. C. Robertson describes a large number of observations to this effect on American plants. Professor Halsted states that the flowers of the barberry are very rarely, if ever, self-pollinated, and calls attention to the remarkable irritability of the stamens of species of *Portulaca*, which promotes the scattering of the pollen over the bodies of insects visiting the flowers. The structure of the flowers of *Arum*, *Dracunculus*, *Helicodiceros*, *Arisæma*, *Amorphophallus*, and other species of Aroideæ, has been made a special study by several Italian botanists. The extraordinary simulation by the open flower of the appearance and odour of decomposing flesh, appears specially designed to deceive and attract necrophagous coleoptera and diptera, which aid in the carriage of pollen. Schulz asserts that in the family Sileneæ of Caryophyllaceæ the proterandry is in many cases so marked as to render self-pollination impossible. Mr. G. F. Scott Elliot has, in the *Annals of Botany*, a very interesting paper on the

pollination of South African plants by birds. The Cinnyridæ, or sun-birds, are exceedingly good pollinators, especially *Nectarinia chalybea* and *bicollaris*, and *Promerops caper*. Like bees, they, as a rule, visit only one species of flower at a time. Mr. Scott Elliot believes that the identity of colour—an unusual shade of red—in the majority of ornithophilous flowers and on the breast of species of *Cinnyris*, is an important element in this pollination. Mr. H. N. Ridley calls attention to a remarkable structure in the flowers of *Bulbophyllum macranthum* and other orchids of Singapore for cross-pollination by the agency of insects. C. Correns describes the special arrangements for this purpose in *Salvia* and *Calceolaria*. Lists of cross-pollinated plants are given by McLeod from Belgium and the Pyrenees, by E. Loew and Knuth from Germany, by Delpino, from Italy, and by Scott Elliot from Madagascar. Kellgren enumerates 33 species of Leguminosæ growing on the Omberg, an isolated mountainous region in Germany, chiefly covered with pine woods, all pollinated by lepidoptera. The mode of pollination in the different species of *Yucca* is of great interest, and has long attracted the attention of botanists. Professor Riley, who has made it a subject of special study, states that in all the species examined by him, self-pollination is almost impossible; and that in each species the pollination is effected by a single species of insect. The pollinator of *Yucca filamentosa* is a moth, the female of *Pronuba yuccasella*. The object of the visit of the moth is the deposition of its eggs in the ovules of the *Yucca*, the coats of which it pierces. This is always effected as soon as the flower opens; and the moment an egg has been deposited the insect rushes to the anthers and carries a quantity of pollen to the stigma, stuffing it into the stigmatic cavity with its proboscis. Ten or twelve ovules may thus be destroyed, but the number in each ovary is so large that this does not practically affect its fertility. In Philadelphia, where the moth makes its appearance about the time that the *Yucca* is in flower, the latter produces abundance of seed, while it does not set its fruit in Washington and St. Louis, where it flowers a fortnight before the arrival of the moth. Finally, the report, in the *Botanical Gazette*, of a series of experiments carried on by Miss M. Reed, mostly on petunias, fully confirms Darwin's statement that cross-pollinated produce more seed-vessels than self-pollinated plants, and that the capsules are heavier.

Attention has long been called to the fact that not a very small number of plants produce, in addition to their ordinary showy flowers, others—termed cleistogamic—which never open, in which the corolla is partially or entirely suppressed, but which ripen abundance of seeds, being, of course, self-pollinated. A very good example of these cleistogamic flowers is furnished by the various species of "dog" violet, *Viola sylvatica* and its allies. The conspicuous flowers which appear in the early spring are mostly sterile;

but throughout the later spring and summer very inconspicuous flowers are abundantly produced, completely concealed in their calyx, and these almost invariably give rise to fertile capsules. It would appear as if nature provided in them a surety for the production of seed in case of the continued sterility of the early spring flowers. Several species of Leguminosæ produce cleistogamic flowers, borne, in some cases, on underground shoots, the seed-vessel never appearing above the surface of the soil, and other instances are furnished by the genera *Oxalis*, *Impatiens*, and many others. During the last few years considerable additions have been made, by Meehan and others, to the list of cleistogamic species, and these include several plants belonging to the British flora, such as *Polygonum acre*, *Hydropiper*, *Persicaria*, and *maritimum*, *Scleranthus annuus*, &c. In *Polygonum acre* and *Hydropiper* the cleistogamic flowers are very numerous, and appear always to produce ripe seeds; they are small and completely hidden in the leaf-sheaths. W. Burck describes several species of tropical plants which bear flowers that never open, and must, therefore, be self-pollinated, although coloured and scented, and producing abundance of nectar. This is strikingly the case with *Myrmecodia*, one of the genera which furnish abodes in their stems for colonies of ants; and he suggests the explanation that the flowers were at first adapted for cross-pollination, but that the visits of insects have been gradually suspended in consequence of the attacks of the ants which inhabit the tubers. A. Schulz connects the appearance of cleistogamic flowers with unfavourable climatal conditions. In *Tephrosia heterantha*, a native of the Argentine Republic, Hieronymus states that the pollen-grains are few in number in the cleistogamic flowers, and that their tubes pierce the wall of the anthers in order to reach the stigma.

The structure of the flowers in the genus *Vitis* is very interesting. In the native state all species of vine have two kinds of flower,—male, in which the pistil is subject to all degrees of abortion, and hermaphrodite, in which both stamens and pistil are fully developed. The stamens differ remarkably in the two kinds of flower. In the hermaphrodite flowers the filaments are short and curved backwards, so as to remove the anther as far as possible from the stigma; in the male they are longer and erect; but in the cultivated vines of Europe, all of which are varieties of *Vitis vinifera*, and have only hermaphrodite flowers, the filaments are long and erect, as in the male flowers of the wild plant. The pollen-grains of the two kinds of stamen also differ in their power of fertilisation. Millardet, who has given great attention to the phenomena connected with the fertilisation of the vine, states that in the wild state the vine is wind-fertilised, although the flowers have a powerful odour, the purpose of which is obscure. In the cultivated state he has observed abundance of two small coleoptera in the flowers, which may also probably take some part in the pollination. Kronfeld

states that the cultivated grape-vine is also occasionally pollinated by honey-bees.

As has already been pointed out, those flowers which are unisexual—*i.e.*, which have either pistil and no stamens, or stamens and no pistil—must necessarily be cross-pollinated. Schulz, however, states that in many of our trees which are normally unisexual, hermaphrodite flowers do occasionally occur. Thus, in the elder, hermaphrodite flowers or transitional forms between these and unisexual are frequently to be found at the base of the male catkins. Hermaphrodite flowers also occur in the birch, though less frequently, very rarely in the hazel; in the oak there are often ovaries at the base of the male catkins, and rudiments of stamens in the female flowers. The mode of fertilisation of the hazel is involved in great obscurity, since scarcely a trace of the ovules can be found in the female flowers at the time when the catkins are discharging their pollen. In the ash, all kinds of condition may be met with, male, female, and hermaphrodite flowers; and either the same or different kinds on the same tree. The ash is probably on the road to becoming completely diœcious.

Few phenomena in physiology, whether animal or vegetable, are more puzzling than those of parthenogenesis, that is, the production of a normal fertile embryo without any preceding act of fertilisation. The example in the vegetable kingdom of *Cœlebogyne* is familiar to all students of text-books. Another very remarkable instance is now on record. There have been few more interesting contributions to botanical science during the last few years than Dr. D. D. Cunningham's "On the Phenomena of Fertilisation in *Ficus Roxburghii*," published in Calcutta in 1889, with beautiful illustrations. According to Dr. Cunningham the figs are in this species either male or female, all on the same plant being of the same sex. The male figs contain perfect male flowers, which produce pollen, and atrophied female or "gall-flowers," which never produce seed, but within the ovaries of which the eggs of an insect—usually a species of *Eupristis*—are deposited and develop into pupæ. The female figs contain perfect female flowers, in which the eggs of the insect are never found, and which produce fertile seeds. The terminal opening of both the male and female figs is so obstructed by a covering of bracts that they are almost completely closed chambers; and the perfect development of both the male and female flowers is dependent on the access of the "fig-insect" to the interior of the cavity, without which they do not arrive at a functional condition. Although the development of the embryo in the female fig is essentially connected with the access of the insects to the cavity, Dr. Cunningham believes that it does not depend on the introduction of pollen by their agency. The nearly entire closure of the opening by bracts presents an almost insuperable obstacle to the introduction into the female fig of a sufficient quantity of pollen for the fertilisation of every one of the very numerous ovules

by a separate pollen-grain; and but very few pollen-grains could be found in the female figs. Although it is possible that in some cases ordinary pollination may occur, the author asserts that the embryo is usually formed without any process of fertilisation, arising as an outgrowth of the embryo-sac. The full development of both the male and female flowers appears to depend on a simple hypertrophy of the tissues of the fig, resulting from the irritation caused by the female insect in the act of laying its eggs within the ovary of the "gall-flowers" of the male figs, and of their persistent attempts to do the same within the flowers of the female figs, in which attempts they are frustrated by the great thickness of the wall of the ovary. In connection with the fertilisation of the fig, it may be mentioned that Professor Riley has enumerated fourteen species of insect as taking part in the so-called "caprification" of the wild figs of North America. He recommends to the fig-growers of California the introduction for this purpose of *Blastophaga psenes*.

With regard to the connection between the duration of life of the individual and the mode of fertilisation, Meehan makes the general statement that flowers which are wholly dependent on insects for their fertilisation are always perennials, and that an innumerable number of their flowers fall unfertilised; while all annuals, on the other hand, can self-pollinate themselves when cross-pollination fails; and in almost all cases all the flowers of annuals are fertile.

The phenomena of hybridity in the vegetable closely resembles those in the animal kingdom. It is possible to fertilise an ovule by pollen belonging to a different species, but only if the two species are very nearly allied. It is very rarely that species which are fertile with one another can be placed in different genera. Hybridisation is one of the every-day resources of the gardener; but that cross-breeding occurs in nature has been doubted by some. There seems, however, scarcely to be room for doubt that in some of our abundant wild genera, such as *Rubus*, *Salix*, and *Hieracium*, hybridity is not uncommon in nature. It has long been known that in some genera, such as *Passiflora*, and in some Orchideæ, the ovules appear to be even more readily fertilised by pollen of a different species. W. Focke now states that this is also the case with the species of *Lilium* belonging to the group *bulbiferum*, and with some species of *Hemerocallis*; and J. H. Wilson affirms the same respecting the Cape genus *Albuca*, also belonging to the Liliaceæ. According to Millardet none of the so-called hybrid vines cultivated in Europe are true hybrids, *i.e.*, products of the crossing of distinct species; they all spring from the crossing of different races of the same species, *Vitis vinifera*. He further states that, in the vine, it is the male parent that exercises the preponderating influence on the descendants. Rimpau has carried out a series of experiments on the crossing of some of our most common agricultural plants. He has obtained ten artificial and nine natural hybrids in wheat, and also obtained a fertile hybrid between

wheat and rye belonging to different genera. Between different kinds of barley he obtained two artificial and six natural hybrids; in no case did he succeed in fertilising a two-rowed barley by pollen of a variety with a larger number of rows. Among oats, five natural but no artificial hybrids were obtained. Very few natural hybrids occur among peas; with different varieties of beet crossing is much more common. Rimpau states that if a new form exhibits great variability among its descendants, it is probably hybrid; while, on the other hand, it is most likely a spontaneous variety if the descendants maintain great constancy.

The Darwinian hypothesis that a sexual mode of reproduction is absolutely necessary to the maintenance of the higher forms of life, and that continual propagation by non-sexual methods must result in deterioration and ultimate extinction, has not been allowed to pass unchallenged during recent years. Möbius in Germany and Meehan in America have pointed out the great length of time, extending in some cases to thousands of years, during which some plants have been continuously propagated by non-sexual methods without apparent deterioration or increased liability to disease. This is the case with many fruit-trees, such as the fig, date-palm, banana, yam, batatas, and olive, and with the Canadian water-weed, *Elodea canadensis*, imported into this country from America, the male plant of which is still unknown here; though it is asserted by others that this pest is now gradually dying out from our rivers and canals. On the other hand, the weeping-willow and the Lombardy poplar—varieties which never produce seed, and which are propagated solely by cuttings—do appear to be threatened with extinction, owing to their abnormal liability to disease.

We now come to the second part of our subject, and, as we have already said, the more difficult one—the nature of the process of fertilisation itself. In general terms, fertilisation, or fecundation, may be defined as the union of the elements of an active male with those of a passive female cell, the result being the production of an embryo which develops into a new individual. In the animal kingdom, the only mode of reproduction in all the higher forms is a sexual one, non-sexual propagation having survived only in the lower types. In the vegetable kingdom, the two modes work side by side in the higher forms, while in the lowest forms sexuality is unknown. It becomes, therefore, a problem of great interest to determine in what way the sexual mode of reproduction arose among plants. Much light is thrown on this question by the phenomena which have been observed among Algæ. A very common mode of propagation among our ordinary fresh-water Algæ is by means of non-sexual zoospores or swarmspores, minute flagellate bodies which escape from the cells of the parent plant, move about in the water with great rapidity, and then finally come to rest, withdraw their cilia, and develop into new plants. Among the brown sea-weeds, there is a great variety in the

nature of the reproductive organs: we have non-sexual flagellate zoospores; we have a rudimentary sexual process in the conjugation of zoogametes, flagellated motile bodies which closely resemble zoospores, but which have no power of germination without first uniting in pairs; and, finally, we have an advanced mode of sexual reproduction in which the male and female elements differ almost as widely as they do in the highest flowerless plants, the former being a minute motile multiflagellate antherozoid, the latter a much larger perfectly passive oosphere. But what is most interesting is that we find in the brown sea-weeds all sorts of intermediate conditions between these, leading us to the irresistible conclusion that all the higher developments of sexuality have had their first origin in the union of two motile flagellate masses of protoplasm between which there is no apparent differentiation; but that very early, as we ascend in the evolutionary scale, a differentiation sprang up between these two masses of protoplasm, which became gradually more and more marked as they developed into what we call male and female cells. The latter soon lost their flagellate character, became quiescent, and increased in size; the former gradually passed from a biflagellate zoogamete indistinguishable from a zoospore, to the multiflagellate antherozoid of the higher Cryptogams. The passage from these to the germinal vesicles in the ovule and the pollen-tube of flowering plants is somewhat more difficult to follow.

The production or non-production of sexual organs in plants is very much a question of external conditions. In a recent paper in the *Biologisches Centralblatt*, Professor Möbius has published a very interesting summary of our knowledge on this subject. An abundant supply of nutriment promotes the production of vegetative rather than of reproductive organs; hence the value of root-pruning in increasing the fertility of fruit trees. The conditions which, on the whole, favour the formation and fertility of the sexual organs are—a high temperature, with not too much moisture and not too much supply of nutriment, abundance of light, and, in the case of the fruit trees of temperate climates, a period of complete rest in the winter. Many of the fruit-trees of our climate will not flower in the tropics. The potato, which blossoms only sparsely with us, produces flowers every year in its drier native country of Chili. *Epilobium angustifolium* will flower with us only in sunny situations, and the brighter the light the deeper the colour of the flowers. The production of flowers and fruit has an exhausting effect on the plant. A well-known illustration of this is the "century plant," *Agave americana*, which flowers only once, when from 30 to 100 years old, and then dies. A very abundant fruit year is commonly followed by one of comparative scarcity.

A similar law prevails also in the lower forms of vegetable life; but with this difference, that we find there the much greater plasticity which is the great feature of a low type of life. Many of our

commonest Algæ will fructify only in conditions which are unfavourable to the production of their vegetative organs. From an exceedingly interesting series of observations made by Klebs on that beautiful organism the "water net," *Hydrodictyon reticulatum*, we learn that all the cells in a net are apparently equivalent, *i.e.*, are adapted to produce either non-sexual zoospores or sexual zoogametes, and that the tendency to produce one or other of these structures is largely a question of nutrition. In a single net, consisting of equivalent sister-cells, some of the cells can be excited, by external conditions, to develop zoöspores, others to produce zoogametes, the formation of the former being, in this case, absolutely dependent on light. A similar law prevails in *Volvox*.

Professor Weismann, in his recently-published volume of "Essays," advocates the view that the old idea of an essential difference, at all events of an opposition, between male and female elements, between so-called "sperm-cells" and "germ-cells," must be abandoned; that they are essentially alike, and differ only so far as one individual differs from another individual of the same species; and that fertilisation is no process of rejuvenescence, but merely a union of the hereditary tendencies of two individuals. This view he supports from the results of experiments which he regards as demonstrating the remarkable fact that, even in animals as high in the scale as the Batrachia, the "sperm-nucleus" can be made to play the part of an "ovum-nucleus," and *vice versâ*; it being possible to produce a free-swimming larva in the formation of which no "germ-nucleus" has taken any part; but, as far as the vegetable kingdom is concerned, the weight of evidence does not appear to be in favour of Weismann's hypothesis.

As has already been pointed out, very early in the evolution of the lower forms of vegetable life the motile zoospores began to assume sexual functions, and to become converted into zoogametes, and these again into active male and passive female elements; and, although the plasticity which is characteristic of the lower forms of life renders this differentiation for a time unstable, it becomes gradually more and more firmly established. Even in that class of beautiful Algæ, the Conjugatæ, of which *Spirogyra* and *Zygnema* are examples, where the "conjugating" cells show scarcely any difference even under high magnifying powers, it has now been shown that there is an essential difference between the male and female cells in their physiological constitution, and in their behaviour towards one another. Still more decisive is the fact, established by Auerbach in the animal, and by Rosen in the vegetable kingdom, that the male and the female nuclei behave differently to staining reagents, the former belonging to the class of "cyanophilous," the latter to the class of "erythrophilous" substances; that is, they take up especially blue and red colouring substances respectively. Other histological differences have also been pointed out between the male and female nuclei,

such as the possession of nucleoles by the former and not by the latter.

The act of fertilisation or fecundation appears to consist in the actual fusion of the nuclei of the male and female cells; at least this is the view taken by one of the highest authorities on this subject—M. Guignard—from a very careful series of observations on *Lilium martagon* and *Fritillaria imperialis*, in opposition to the theory of Van Beneden and some other observers. This fusion is assisted by the action of remarkable bodies which have only recently come under observation, and which have been termed "tinoleucites" or "directing spheres." These bodies, which have been observed by Guignard and Van Tieghem in plants, and by Fol and others in animals, are minute spherical masses of protoplasm occurring in both the male and female cells, which appear to exercise the function of directing the course of the male nucleus so as to bring it into contact with the female element. There appears to be also some directing principle in the higher Cryptogams which governs the antherozoids in finding their way to the oosphere within the archegone.

Without dogmatising on so intricate a question, the theory may at least be hazarded that every act of fertilisation is simply a modification of a process of nutrition, the conveying to a potential germ of some property which it does not derive from its own ancestors, but which gives it greater completeness, and endows it with greater resources in the struggle for existence. Sufficient, at all events, has been said to show how rich a field there is still open to the skilled microscopist in the investigation of the interesting problems connected with the perpetuation of animal and vegetable life on the surface of the globe. While it is probable that no human researches will ever lead to a solution of the questions: "What is life?" and "How were the phenomena of life first infused into inorganic matter?", we seem, year by year, to be able more closely to follow the steps by which, from the first germs of life, has been gradually evolved that marvellous complexity in the animal and vegetable worlds, the laws of which excite our increasing wonder and admiration the more minutely we investigate them.

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ALFRED W. BENNETT.

VI.

Animal Temperature.

THE higher animals have within their bodies some source of heat and some mechanism to regulate the production and loss of that heat, for, equally in the height of summer as in the depth of winter, their mean temperature is constant. This fact was known to the Ancients, but imperfectly; they had no thermometers, no exact methods of determining temperature. They judged from their sensations, but here sensations are imperfect guides.

A patient attacked by an ague feels chilly, miserably cold; he huddles up by a fire, he shivers, and his teeth chatter; he says that he is cold, but at this precise moment he is in a fever-heat. How does this contradiction arise? The feeling of heat or cold arises in the nervous structures of the skin. When these nerve-endings are flushed by a rapid stream of blood, there is a sensation of warmth; should the blood supply be small, not enough to compensate for the loss of heat by radiation and conduction, there arises a sensation of cold. During the *rigor*, for such is the name given to the early stage of ague, the vessels of the skin are contracted, the skin is pale and cold, its temperature twelve or sixteen degrees below the normal blood-heat; but, at the same time, the temperature of the internal parts has risen six or seven degrees above the normal.

Let the converse condition be studied in the same disease during its third or *sweating* stage. The skin is now red and flushed, its blood-vessels are dilated; the patient is bathed in perspiration; he says that he is intensely hot, although his internal temperature is rapidly falling to the normal.

Animal heat the Ancients considered to be beyond the reach of physical or chemical laws. They could assign no cause for it, and therefore looked upon it as some innate quality, something essentially "vital." This "vital" heat was supposed to be concentrated in the heart, and to be distributed to the body by the blood in the veins; it was prevented from accumulating by respiration, the chief function of which was to cool or temper the blood.

After the year 1595 it was first possible to determine the temperature of the body more exactly, and to make observations upon the independence of the temperature on external conditions. It was about this time that Galileo invented the thermometer. As observa-

tions now increased, numerous speculations as to the source of animal heat arose. It must be connected with the movement of the blood, for how otherwise could be explained the cooling of a corpse or of a limb deprived of its circulation? The heat might arise in the blood alone or have its origin in the heart, and only be distributed by the blood stream.

It was well known that heat arose during fermentation and by the contact of acid and base; animal heat was, therefore, considered to arise by some similar process or processes taking place in the blood. The former opinion was held by Willis, the anatomist, who, in his treatise "de Ascensione Sanguinis," written about the year 1670, gives the theory that there is in the blood a combustion which depends upon the fermentation excited by the combination of different chemical substances.

Friction was another well-known source of heat, and was the explanation given for animal heat by the celebrated Dutch physiologist, Boerhaave, who lived in the early part of the eighteenth century. He explained the "vital" heat as due to the friction of the particles of blood in the vessels.

A much more correct opinion had already been formed by Magin in 1674. This physiologist, after his experiments in the constitution of air and its relation to the heat of combustion, extended the analogy of combustion to animal heat. He held that the function of the lungs was not to cool the blood, but to enable that fluid to absorb the nitro-aërial spirit (oxygen) of the air, and so generate heat.

Black, in 1757, discovered that carbonic acid gas, which is formed by the burning of fuel, is also given off by the lungs; he thus showed a strict connection between the processes of combustion and respiration; he supported the theory that the heat arising from the union of oxygen and carbon was the real "vital" heat.

A few years later Lavoisier introduced a theory exactly similar to Black's, but how far it was dependent on or independent of Black's work is not known.

A serious objection was quickly raised to Black and Lavoisier's theory; it was shown that if the heat were produced in the lungs, the temperature of that organ would be incompatible with its life. To remove this objection, Crawford in 1781 proposed his ingenious theory, which he had based on experiments upon the specific heats of venous and arterial blood. His experiments appeared to show that the specific heat of arterial was considerably greater than that of venous blood; the combination, therefore, of the blood with the oxygen of the air in the lungs gave rise to the liberation of only a small amount of heat in the lungs; the blood here was arterial. When, however, the blood reached the systemic capillaries, it became venous, and the latent heat was rendered sensible. This was a very "materialistic" explanation of "vital" heat, and was, in certain respects, a distinct advance towards truth.

It is interesting that Crawford and Lavoisier were the first to make experiments on the amount of heat given off by animals, and even to compare the oxidation, as represented chiefly by the carbonic acid, with the amount of heat produced. They showed that the oxidation could produce the necessary heat.

John Davy in 1815 showed by more exact experiments that there was no perceptible difference in the specific heat of arterial and of venous blood, and therefore Crawford's theory must fall to the ground. This, however, by no means affected the doctrine of Lavoisier, that the oxidation in the animal body was the cause of the heat. In fact, Liebig not many years later, by properly interpreting the calorimetric experiments of Daling and Despretz, placed Lavoisier's doctrine on such a firm basis that it became strong evidence for the Law of the Conservation of Energy.

In the last fifty years the labours of Helmholtz, Ludwig and Pflüger have proved that the heat is formed in the tissues, that heat is produced by the activity of muscles and glands. Bernard also, by showing how variations of the circulation of the bood could be effected by the nervous system, threw a great light upon the process of heat regulation. The formation of heat varied in different parts, but the temperature was equalised and regulated by the continual blood-stream; the circulation of the blood, being under the control of the nervous system, could, when necessary, send more blood to the surface, and so increase the loss of heat, at another time could husband the warmth by diminishing the blood-supply to the skin.

Such have been the chief lines along which the knowledge of the causes of animal temperature has advanced. The doctrine of "vital heat," propounded by the vitalistic school, has fallen before the advance of materialism; "vital heat" is shown to be similar to heat arising from physical and chemical processes.

Numerous observations upon animal temperature have now accumulated. Of these the most interesting, without doubt, lies in the difference which has been found to exist between the lower and the higher animals. Those animals which are high in the scale of evolution, such as birds and mammals, have a high temperature, and this has a mean value (about 37° C. or 98° F. in man) which is independent of the temperature of the surrounding air. The lower animals on the contrary, such as molluscs, fishes, amphibians and reptiles, have a temperature generally only slightly above that of their surroundings. This difference between the two classes is expressed by the terms "warm-blooded" and "cold-blooded" animals.

The warm-blooded animal has in correspondence with its high temperature a very energetic oxidation within its tissues, and this must, *cæteris paribus*, be more vigorous in the winter than in the summer. The cold-blooded animals, on the contrary, have a much smaller oxidation, and one which is so lessened by cold that in winter

these animals find their energy so reduced that they pass the time in sleep—they hibernate.

The higher animals have been classed as the warm-blooded, the lower as the cold-blooded. This classification is, however, not absolutely exact. Indeed mammals are found, such as the hedgehog, bear, and dormouse, which are in an intermediate position, since, in spite of their high temperature in warm weather, their temperature falls in winter, they become inactive and hibernate, the oxidation of their tissues is reduced to a minimum; on the other side, there are bees, animals of a low order, which have and maintain a higher temperature than most cold-blooded animals, and are not reduced to spend the winter in slumber.

In order that an animal may have under very varying conditions a constant temperature, it must possess some regulating mechanism. There are two means by which regulation can be obtained, variation in the amount of heat produced and variation in the loss of heat. Both methods are found in use. When a white mouse, for example, is removed from cold to warm surroundings, it regulates its temperature by increasing the loss of heat from its body, it sweats and exposes to the air as large a surface of its flushed skin as possible; at the same time, its heat-production is lessened, it is less active, it gives off less carbonic acid, there is less oxidation in its tissues. Let the same animal be now returned to a cold chamber. At once its ears and face become pale, showing that the vessels of the skin contract, and prevent loss of heat by the blood passing to the skin; when not in active movement the animal huddles up together, ruffles its fur, and makes it as bad a conductor of heat as possible; it also regulates the heat production, as shown by involuntary shivers or active voluntary movements, it gives off more carbonic acid, there is increased oxidation in its tissues.

These regulating mechanisms appear to be under the control of certain parts of the nervous system. The exact anatomical seat is unknown, nor are known the anatomical differences which separate the cold-blooded from the warm-blooded animal, and which would explain the condition of the intermediate forms, such as the hedgehog, the dormouse, bees. It has been shown that by the action of certain nervous poisons, and by intense cold, warm-blooded animals may be made to pass into a condition somewhat resembling that of the cold-blooded animals. Ignorance now reigns here, but not so absolutely for ever.

M. S. Pembrey.

SOME NEW BOOKS.

IDLE DAYS IN PATAGONIA. By W. H. Hudson. 8vo. Pp. viii. and 256. Illustrated. London: Chapman & Hall, 1893. Price 14s.

IF every sojourner in remote regions of the world occupied his "idle days" as profitably as has Mr. Hudson, our information regarding the animal life and its inanimate surroundings of the whole globe would be far less incomplete than it unfortunately is at present. We are, indeed, glad to welcome another volume by this author, written in the same charming and lucid style as his "Naturalist in La Plata," and embellished by illustrations, executed in the same beautiful style. The present work is, however, far less exclusively devoted to the Natural History of the country it describes

CRESTED TINAMOU *(Calodromas.)*

than was its predecessor, but for a vivid description of the mode of life in Patagonia, and the nature of the country, it can scarcely be surpassed. As with "La Plata," most of the chapters in the work before us have already seen the light in the form of articles in magazines and other serials, and this reproduction has given the author the opportunity of revising and amplifying his descriptions and maturing his opinions.

Confining our attention to those parts of the book devoted to the Natural History of Patagonia, we note that the author gives us excellent illustrations, accompanied with observations on their

mode of life, of the Black Vulture, the Upland Goose, and the Crested Tinamou, as well as of several of the smaller birds. Perhaps the most generally interesting chapter in the whole book is that on "Bird-music," in the course of which the author points out how impossible it is to convey any adequate idea of the songs of birds by mere description. He illustrates this by mentioning that before he had ever visited England he was anxious to obtain some idea of the nature of the melody of the British songsters, but that all his efforts were in vain. And the description of his delight at first hearing the music of our English fields and groves ought to remind us all how little we really appreciate, as a rule, the natural charms of our own land. Possibly some further development of the phonograph may be the ultimate means by which "Bird-music" may be rendered comprehensible to those who cannot hear it at first hand, as we greatly doubt if this can ever be effected by any merely graphic method.

Of scarcely less interest is the author's description of the ways of the leaf-bearing ants, on pp. 140–141; while those whose special study is Mammals will not fail to notice the observations on the burrowing Tuco-tuco (*Ctenomys*) and the Mara (*Dolichotis*). As the author well remarks, it is very curious to find in a burrowing animal the eyes of so large a size as they are in the former of these two rodents.

In commending this book to the attention of our readers, we may express a hope that it will ere long be followed by others from the same ready pen. R. L.

THE GREAT SEA-SERPENT: An Historical and Critical Treatise. With the reports of 187 appearances (including those of the appendix), the suppositions and suggestions of scientific and non-scientific persons, and the Author's conclusions. With 82 illustrations. By A. C. Oudemans, Jzn. Published by the Author, October, 1892. London: Luyac & Co.

Of this work we give two reviews, the one by a naturalist, the other by a literary contributor.

I.

THE author in his preface compares his work with that of Chladni on meteoric stones. Chladni, he says, opened the eyes of unbelievers by collecting and comparing all accounts of meteoric stones up to the nineteenth century. Meteoric stones were again found and were proved to be quite different from terrestrial stones. Unfortunately for the argument, remarkably few sea-serpents have been caught, and those few have proved to be not at all different from well-known objects.

We confess to have found Oudemans' book exceedingly dul reading. 379 pages are devoted to genuine or invented accounts of various appearances; 110 to explanations hitherto given. No doubt it is useful to have the literature of the subject compiled, but the author might have contented himself with a much greater compression of the interminable newspaper discussions, evidence on oath of sailors and fishermen, and so forth. We do not see that he advances at all beyond Mr. Hoyle's bright and short account of the sea-serpent in the "Encyclopædia Britannica." A number of the records are pure myths; some others are due to mistaken observations of floating sea-weed; porpoises swimming in a row; basking sharks, and so forth. The persistent records from the Norwegian coasts are most probably explained by the existence of gigantic cuttle-fish; and there remains

a residuum which quite possibly may be explained by the discovery of some new animal. Oudemans' own theory is given with a considerable amount of confidence, in some 50 pages towards the end of the book, and on page 516 an ideal sketch of the animal is produced. It is a Pinnipede to be called *Megophias megophias* (Raf.), Oud.; and a phylogenetic table of its relations with other Pinnipedes, living and extinct, is given. It is very possible that a large Pinnipede may exist, but, on carefully going through the characters suggested for it by this author, it is difficult to see that he has been guided in his selection from reports by any sounder principle than relying on what appeared to suit his hypothesis, and rejecting or explaining away inconvenient ones. Further notice of this book I leave to a literary contributor.

P. C. M.

II.

THE attitude of the nineteenth century, social as scientific, towards the unknown may be summed up by the remark made by a lady member of the upper ten concerning those less happily situated in the social scale—"I don't know them: they don't exist." In the face of this attitude Dr. Oudemans has presented the public with a treatise of no less than 592 pages on our old friend the sea-serpent. We are warned on the title-page that the little volume contains reports of 187 appearances, "including those of the appendix," but we are not told what the appendix of the serpent is, and there are no special reports on appearances of his tail. The author much deplores the fact that his illustrations are not due to the unlying kodac. Nevertheless, the reader does not lose by this little omission in the baggage of observant travellers. It has been the means of providing portraits of the sea-serpent such as we feel assured no camera yet invented could have produced. The most impressive of these is one compiled by Messrs. Renard, *père et fils*, who combinedly took observations one moonlight night during a voyage on the high seas in 1881. The result is the portrayal of a dragon worthy the sublimest efforts of Sir Augustus Harris. We fancy even our own patron saint would have quailed before it, unless, indeed, following the advice given by Dr. Oudemans, he had armed himself with "explosive balls and harpoons loaden with nitro-glycerine." This particular specimen possessed not only teeth, "sharp, enormous, and white," but a phosphorescent tongue and an eye that looked backwards. Probably that eye also winked, for Dr. Oudemans includes both description and drawing under the heading of "Cheats and Hoaxes"—a section sternly dealt with at the beginning of the book. However, in the next chapter, entitled "Would-be Sea-Serpents," the illustrations are not to be outdone, in spirit and conception, by mere cheats and hoaxes, and in fig. 12, *Lineus longissimus*, Sow., we find something really worth looking at. Chapter iv. consists of reports and papers on the appearances of sea-serpents in various parts of the world. These are mostly compiled by naval officers, and a few—a very few—scientists. The Church, however, is not unrepresented, and we would not like to call in question the evidence of the Archbishop of Upsala, writing in 1555; but surely the Archbishop's illustration is the gem of the collection. The sea-serpent is depicted in the act of swallowing a sailor, and we can only charitably suggest that the original sketch was intended as a representation of Jonah's unfortunate experience. Among so much documentary evidence, it is difficult to discriminate critically. Even Milton is pressed into the service, quotations being given in "Reports

and Papers" from *Paradise Lost*, where the great leviathan is mentioned as being the special dread of the Norwegians (the coast of Norway being the favourite haunt of the monster according to the most reliable eye-witnesses). But why is Coleridge left out?

"Beyond the shadow of the ship,
I watched the water snakes.
They moved in tracks of shining white,
And when they reared the elfish light
Fell off in hoary flakes."

Later on, we come upon another picture of a sailor being cut off in his prime by the wily monster, and we pass on with considerable relief to Dr. Oudemans' "Conclusions," where, under the headings of "Harmlessness, Timidity, and Playfulness," we learn that the great sea-serpent is, after all, a mild creature, a conviction probably deduced from the fact that, if the great sea snake has ever shown his animosity to the unwary invaders of his haunts, no victim has, so far, survived to tell the tale.

It is sad, however, to find that Dr. Oudemans' great sea-serpent is, after all, only a monster sea-lion, and we don't know of his having done anything to justify Dr. Oudemans' calling him *Megophias megophias* (Raf.), Oud.

L. B. P.

Handbuch der Palæontologie.—Palæozoologie, vol. iv., pt. i. Edited by K. A. Zittel. 8vo. Pp. 304. Illustrated. Munich and Leipsic: R. Oldenbourg, 1892.

We are glad to congratulate Professor Von Zittel on the untiring energy which has enabled him to get well into the Mammals in the present fasciculus of this magnificent and invaluable work. It need hardly be mentioned that the whole of the Vertebrate portion of the work has been written by the Editor himself; and the present fasciculus is fully equal to its predecessors in the care and attention

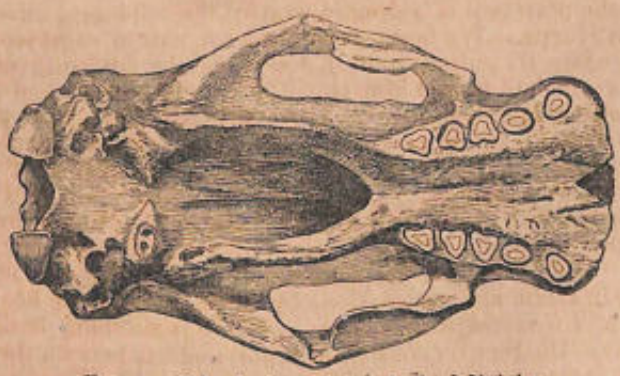

Fig. 1.—Palatal view of the skull of *Mylodon*.

which has been bestowed upon it, and in the excellence of the illustrations. Three of the latter we are enabled to reproduce as samples. We are especially glad to notice that the author has devoted particular attention to the recent important discoveries which have been made among the fossil mammals of South America, and that a host of new forms have been assigned to their proper serial positions. This is the more creditable to the author, seeing that owing to unfortunate circumstances the vertebrate palæontology of that country

has been involved in an intricate labyrinth of confusion, from which it can only with much labour and care be freed. We are persuaded, however, that in accepting in the main the conclusions of Señor Ameghino, the author has been well advised; and we are glad to see the newly-discovered Argentine Tertiary Marsupials placed next the living Australian Thylacine.

Almost the only portion of the work that we do not like is the classification which the author has seen fit to adopt. In place of dividing Mammals into the three primary groups of Ornithodelphians (Monotremes), Didelphians (Marsupials), and Placentals, Professor Zittel takes only two primary groups, namely, Eplacentals and Placentals; the former including the Monotremes and Marsupials, together with the extinct Allotheria, or Multituberculata. Now, we are fully prepared to admit that there is much to be said in favour of a binary instead of a ternary subdivision of the Mammalian class. If, however, such binary scheme of classification be thought advisable, we have no hesitation in saying that the Monotremes (together with the Multituberculata) should form one subclass, while the Marsupials should be brigaded with the Placentals in the second, and that there is no sort of justification for the scheme followed by our author.

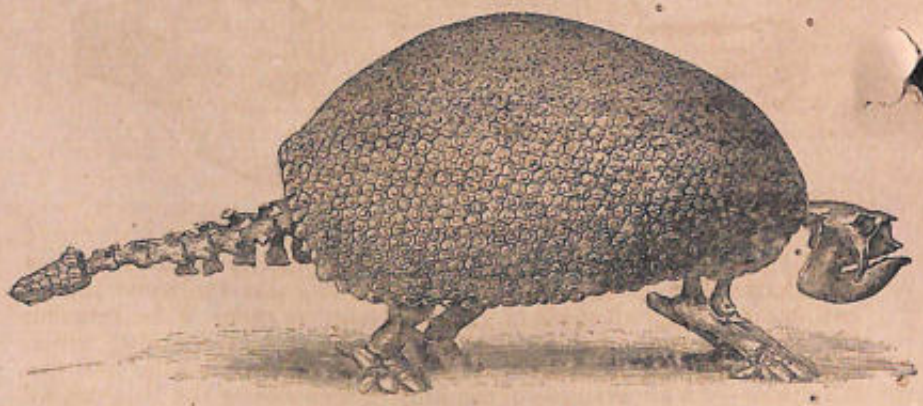

FIG. 2.—Restored skeleton of *Glyptodon*, with the tail incomplete.

The present fasciculus includes the Monotremes, Multituberculata, Marsupials, Edentates, Cetaceans, Sirenians, and the Condylarthrous and Perissodactylate sections of the Ungulates.

A large space is devoted to the Mesozoic Mammals, among which the Allotheria (Multituberculata) are assigned a rank equivalent to that of the Monotremes, while all the other forms are included (and we believe rightly) among the Marsupials, some of the Cretaceous types being even placed in an existing family. There may be some justification for making the Banded Anteater (*Myrmecobius*) the representative of a distinct family, but there is surely none for placing the Peramelidæ between that family and the Dasyuridæ. Then, again, we must take exception to making the Rat-kangaroos a distinct family (to which, by the way, a wrong name is given) separated by *Thylacoleo* and the Phalangers from the Kangaroos (Macropodidæ).

In the Edentates, special interest attaches to the excellent restorations of the skeletons of the Glyptodonts, one of which is herewith given. We are, however, persuaded that the division of the Ground-Sloths into several distinct families is not justifiable. We notice that the author has seen no reason to follow certain new views

which are supposed to justify the removal of the extinct Zeuglodonts from the Whales.

Beyond the circumstance that a large number of the newly-discovered South American forms are described and figured, the only point in the part devoted to the Ungulates to which we need direct attention is in regard to the classification of the Perissodactyles. Following recent American views, the author adopts a phylogenetic classification, and includes in the same family such widely-different forms as the modern *Equus* and the Eocene *Hyracotherium*. On the other hand, *Systemodon*, which is very close to the latter, is placed in the Tapiridæ. That these two Eocene forms may be the ancestral types of the two families to which they are respectively referred, we are not going to dispute for a moment. We do say, however, that to include them in such families, and to separate them from one another, is, to our mind, only confusing classification. Moreover, if we are to have a phylogenetic classification, the author does not go far enough.

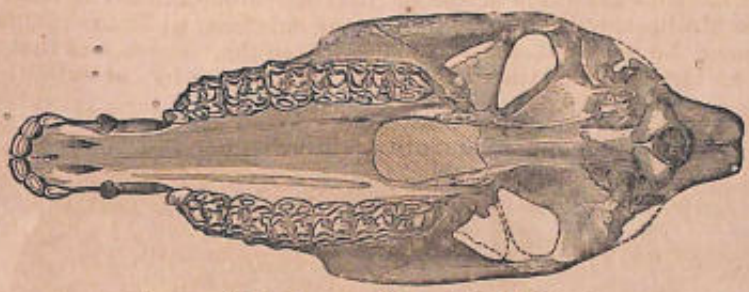

FIG. 3.—Palatal aspect of the skull of *Hipparion*.

Surely, seeing that the Condylarthra are admittedly the ancestors of the Perissodactyla and Artiodactyla, there is just the same justification for putting all three in a single group, as there is for refusing to admit that there may be an Eocene family (Lophiodontidæ) which shall include the ancestors of both the modern Equidæ and the Tapiridæ. As an example of the illustrations among the Equidæ, we reproduce one of the skulls of *Hipparion*.

That the work (however much we may be disposed to differ from it as regards points of detail) when complete will be of inestimable value to the palæontologist and zoologist, we have already had occasion to say elsewhere, and our only regret is that the limited cult of the former science in this country forbids the publication of one on the same lines in England. R. L.

ÉLÉMENTS D'ANATOMIE COMPARÉE. By Remy Perrier. Pp. 1,000. With Illustrations. Paris: J. B. Baillière et Fils, 1893. Price 20 francs.

ZOOLOGICAL text-books seem at the present time to be multiplying at a rate altogether out of proportion to the numbers of the students for whom they are intended. This is not only the case in this country, but also on the Continent. The commencing student of zoology has so many works to choose from (we cannot, however, justly say that there is an *embarras de richesses*) that he probably ends by taking one at random. It would be much better if the elementary student would refrain altogether from purchasing text-books, and trust himself to the

professor whose lectures he attends. However, if he must buy a text-book, then let him consider two standards by which, in our opinion, a book of this kind should be judged. A book intended for elementary students ought to be of moderate bulk, and it should be thoroughly up to date. In these days of examination, the very newest discoveries are apt to be inquired about by examiners. The book should be rendered fairly portable through judicious condensation, not so pronounced, however, as to produce obscurity. The writer of a text-book should remember that human flesh is weak, and also that time is short. M. Perrier's book is not, judged by these standards, altogether ideal; still, we gladly admit that it has its good points. Possibly, too, the French student is differently placed with regard to examinations. The author might, however, if he had taken thought, have refrained from adding an inch or two to the stature of the book; this would have been a distinct gain, for we have seldom handled so awkwardly shaped a volume. If there is a second edition it is to be hoped that a fission into two equally-sized halves will take place.

A feature of M. Perrier's book is the introduction of coloured plates, illustrating, of course, the circulatory system of various animals. The illustrations are not generally very first-rate, and the same remarks apply to the woodcuts and "process" blocks by which the text is really very lavishly illustrated. What is wanting in quality is certainly made up in quantity. Furthermore, the author pays more attention to papers published outside France than is sometimes the case with his countrymen. Altogether, we are favourably impressed with M. Perrier's book.

TEXT-BOOK OF THE EMBRYOLOGY OF MAN AND MAMMALS. By Dr. Oscar Hertwig. Translated from the third German edition by Edward L. Mark, Ph.D., Hersey Professor of Anatomy in Harvard University. Pp. 670, with 339 figures in the text and 2 lithographic plates. London: Swan Sonnenschein & Co., 1892.

ALMOST as much as in the case of Balfour's great treatise on comparative embryology, this book is the result of the author's original investigation. The two books have very different purposes. Balfour was practically inventing a new science, and he endeavoured, so far as could be done, to go over the whole animal kingdom, and set forth, in orderly and systematic exposition, the facts and the principles of Comparative Embryology. The very result of the enormous stimulus to research given by his treatise is, that the treatise is now so largely behind our knowledge that it is rather a landmark in history than a text-book for beginners. Dr. Oscar Hertwig has written his book specially from the point of view of the needs of those studying human anatomy. The facts and principles of comparative embryology are used chiefly as a guide to the ontogeny of higher forms. But the student of medicine or morphology who masters this volume will have a large knowledge of comparative embryology, and will have the unity of the animal kingdom strongly impressed on him. It would be difficult to think of anyone from whom a book of this kind would be more valuable, for a large part of the extension of our knowledge has been due to the studies of Oscar Hertwig and his brother on the Cell and the Coelom-Theory.

Those who have been unable to follow recent developments of embryology closely, will be most interested by the chapters on the middle layer. Not long ago it was assumed that the mesoblast was a

germinal layer of equal value with epiblast and hypoblast. True, it appeared later in development, and its mode of origin was in dispute. Here there will be found a different conception. Hertwig divides Metazoa into animals with two germ layers and animals with four germ layers. In the latter, the two middle layers are both derived as definite organs from the inner layer; the one is the median notochord; the other, the paired sacs of the body cavity. In Amphioxus alone, this condition is perfectly definite; but the recent discovery[1] of segmental ducts, and nephridia in Amphioxus certainly will increase the content of morphologists in regarding Amphioxus as a representative of the ancestral Chordata. In higher forms, the presence, actual or ancestral, of masses of food-yolk has so compressed the pouch-like evaginations from the primitive gut that they arise as solid outgrowths.

Hertwig keeps by themselves these four primitive layers as "Epithelia," which serve for the limitation of the surfaces of the body. At the close of segmentation only one layer is present—the epithelium of the blastula. From this the other layers arise by infoldings and outfoldings. But there is another element of development. In the Cœlenterates and Echinoderms, when the two primary layers are established, there is found between them a supporting gelatinous layer. There wander into this from the primary layers cells most often starting from the point where these layers pass into each other. These immigrant cells lose their epithelial character and spread out as branched cells like connective tissue corpuscles. This factor in development is called by Hertwig the "mesenchyme," and he finds it in the development of all higher forms as the origin of the connective tissues and the blood.

Agreeably to the intention of the book as a text-book, this theory is laid down crisply and definitely, and it certainly is an extremely important advance on current ideas, although, for reasons that will readily occur to specialists, it may well be that the two middle layers and the mesenchyme do not exhaust that complex telescoping of forgotten stages in the development of organs we call mesoblast.

For the printing, illustrations, index, and references to literature, the highest praise is due. The translator has done his work well, but he is not to be congratulated on his translation of the word "anlage"—a translation on which he plumes himself so much in the preface. "Fundament" is a clumsy word, and both the noun and adjective have meanings already in possession. As an English equivalent, "forecast" is the best word, and for an adjective "incipient" is very convenient.

P. C. M.

Die Zelle und die Gewebe. [The Cell and Tissue.] By Dr. Oscar Hertwig. Pp. 300, with illustrations. Jena: Gustav Fischer, 1892. Price 8 marks.

A book treating of the structure of cells, written by Dr. Hertwig, and published by Herr Fischer, of Jena, is sure to be excellent in matter and in style. The volume before us is, on account of its very excellence, difficult to use as a text for a short discourse in this review. Merely to praise is apt to produce rather laborious reading. The only proper alternative would be to give a general account of the book; but this would occupy a little too much room; we shall therefore

[1] Since the publication of this edition.

content ourselves with noting a few points. The subject of which Professor Hertwig treats is one which the Germans have made particularly their own; it is true that eminent persons, not Germans, such as Professor Edouard van Beneden, have investigated these matters, but the bulk of the work has been done in Germany.

Professor Hertwig is, of course, a zoologist, but he has by no means neglected the botanical side of his subject; indeed, the work would be comparatively valueless if he had. It is one of the most remarkable generalisations of Biological science that animal and vegetable cells are identical in all important characters. This was first realised when the identity of their protoplasm was proved by Von Mohl; and the discovery of the nucleus, first due to our countryman, Robert Brown, led the way towards proving another fundamental resemblance between the structural units of the plant and the animal. Finally, the recent discoveries that the phenomena of the dividing nucleus termed karyokinesis were not peculiar to the animal cell, but also characterised the vegetable cell, left nothing wanting to prove the close similarity of the two. There are still remaining certain cells in which a nucleus has not been discovered; on the other hand, the balance is restored by the existence of other cells which have more than a fair share of nuclei. As to the former class, it is doubtful whether a nucleus is ever really wanting. The mammalian red blood corpuscle will at once occur as an instance of such a cell; but, as Professor Hertwig points out, these bodies are not the equivalents of cells at all. Some of the simpler amœboid organisms appear to be without a nucleus, and it was suggested that in such cases the organism was really a free nucleus, with little or no protoplasm. This suggestion was made, we believe, by Dr. Will; Dr. Hertwig alludes to the matter, but does not quote Dr. Will. The Bacteria are organisms whose real nature has been disputed; some have been unwilling to allow to these "plants" the rank of a cell. Bütschli, however, found deeply staining bodies in Bacteria to which he assigned the value of a nucleus. Dr. Hertwig does not refer in this connection —as he might have done—to Mr. Harold Wager's investigations into the nuclei of Bacteria, communicated to the British Association at Cardiff.

An important part of this work deals with karyokinesis. To the general account is appended a brief historical sketch of the matter, and a discussion of the more debatable points, and among these the origin of the centrosoma is the principal one. After giving the arguments in favour of its nuclear derivation, the author ends with the opinion that the time is not ripe for a definite settlement of the question. In the current number of the *Quarterly Journal of Microscopical Science* is a valuable paper upon these matters.

An interesting section of the book deals with the position of the nucleus in the cell—that is, its relation to growth, deposition of "formed substances," &c. The nucleus is all-important; a cell, in fact, is a territory governed by a despot, the nucleus presiding over all the activities of the protoplasm. It was pointed out a few years ago that the yolk in the ovum was first formed in the neighbourhood of the nucleus, a discovery that entirely does away with a view at one time current that the yolk, or, at any rate, a good deal of it, was not of home manufacture, but fabricated outside the ovum, and conveyed to it through the pores in the egg-membrane. Dr. Hertwig quotes and illustrates from the observations of Haberlandt upon the relation of the nucleus to thickenings in the cell-wall of vegetable tissues, and to the growth in length of cells; when these circumstances are

occurring, the nucleus is always apparently to be found at the seat of action. Finally, there are the important studies of Gruber and others upon the artificial breaking-up of cells; in such experiments it has been found that a fragment of the original cell in which the nucleus is left can be regenerated anew, while the remaining pieces live for a time, but eventually decay. The book is, in fact, a very complete and impartial summary of existing knowledge upon one of the most important and fascinating branches of Biology; but it is, of course, written by a specialist for people who are acquainted with, at least, the outline of the subject. Nevertheless, in the proper sense of the word, it ought unquestionably to be a most popular work.

THE GAME BIRDS AND WILD FOWL OF THE BRITISH ISLANDS: Being a Handbook for the Naturalist and Sportsman. By C. Dixon. 8vo. Pp. xvi. and 468. Illustrated. London: Chapman & Hall, 1893. Price 18s.

THE stream of books on British Birds seems to be an endless one, but, nevertheless, there is doubtless an opening for the present volume, which deals with all those groups of special interest to the sportsman, namely, the game birds properly so-called, the pigeons, rails, plovers and their kin, as well as the water-fowl. In his preface, Mr. Dixon—whose name is already well-known through several popular works on ornithology—is careful to observe that he is largely indebted to the writings of others for much of the material on which his work is based, although his own ornithological studies have enabled him to add a considerable amount of original matter connected with the habits of the birds described. Since, we believe, there is no other work of this kind covering precisely the ground occupied by the volume before us, Mr. Dixon's handy and well-illustrated manual ought to be acceptable to all sportsmen who take interest in the natural history and habits of their quarry.

We cordially agree with the author's prefatory remarks concerning the subject of bird classification, and the haste with which almost every aspirant to ornithological knowledge propounds some startling new scheme, destined to revolutionise the science—until superseded by the next attempt; and we think he has done well in keeping more or less closely to one of the older arrangements. In attaching the termination "*formes*" to each one of the old ordinal names, such as "Anseres," we are of opinion, however, that he is ill-advised; the only possible advantage of such a cumbersome affix being in cases where the terms are employed in the sense of subclasses, as, for instance, when Mr. Sharpe uses the name "Anseriformes" to include the order "Anseres" and several others of the old ordinal groups. In placing the Sand-Grouse under the "Galliformes," rather than with the "Columbiformes," the author has no sort of justification, as the osteology of these birds is alone amply sufficient to show that their affinity is much closer with the former than it is with the latter group. Neither are we yet convinced of the propriety of removing the Bustards from the vicinity of the Cranes, and placing them with the Plovers.

The mention of Cranes reminds us that under the heading of the Demoiselle Crane, the author states that the time during which this bird may be taken is "August 1st to March 1st, otherwise by authority of owner or occupier of land." Now, seeing that there is but one solitary record of the occurrence of this species in the British

Islands, it strikes us that the introduction of the statement in question is a trifle superfluous. Indeed, in a book intended primarily for the sportsman, we should have thought even any mention of this particular species of Crane might have been perfectly well omitted.

These are, however, but unimportant blemishes in a work which we can thoroughly recommend to the attention of the class of readers for whom it is specially designed, as being both readable and, so far as we can see, at the same time accurate and up to date.

R. L.

THE HEMIPTERA HETEROPTERA OF THE BRITISH ISLANDS. By Edward Saunders, F.L.S. 8vo. Pp. viii. and 350, with a structural plate. London: L. Reeve and Co., 1892. Price 14s. (Large paper edition with 31 coloured plates, price 48s.)

IT is with pleasure that we chronicle the appearance of this work, which is uniform with Canon Fowler's "Coleoptera of the British Islands," lately issued by the same publishers. Since the appearance of the Ray Society Monograph of the British Hemiptera by Messrs. Douglas and Scott in 1865, and Mr. Saunders' own excellent synopsis of the order in 1876, numerous species of bugs have been added to our fauna, and inevitable changes and re-changes in nomenclature have been made. The classification and synonymy of the British Hemiptera are now brought thoroughly up to date, and it is to be hoped that the book will incite many naturalists to the study of these interesting insects, which, though much neglected in comparison with moths and beetles, well repay the student for his work, and in some respects offer him more opportunity for discovery than the more popular groups.

Only 12 pages are devoted to an account of the affinities, anatomy, and development of the Hemiptera, and the methods of their capture and preservation. It is to be regretted that this portion of the work has not been carried to a greater length; collectors, even now, too often think that to give a specimen a name is the only end of their study; and attention might well be directed to some of the morphological problems towards whose solution the commonest bug may contribute material. Short though it is, however, this introductory portion is clear and reliable, and as far as external structure is concerned, well illustrated by the plate.

The book, however, is issued as a systematic work and, as such, it is excellent. In his arrangement the author follows Puton; entomologists familiar with the sequence of the families in Douglas and Scott will notice that the Hydrometridæ and Hebridæ are brought forward to the middle of the series between the Aradidæ and Reduviidæ, while the Capsidæ are relegated to the end. There are synoptical tables of the genera and species which must have cost the author great trouble, and which, in conjunction with his full and accurate descriptions, should safely guide the student to correct determinations. Those who can afford the large edition will find, in some cases, the coloured figures a "royal road" to identification; but among the more critical groups, such plates are of doubtful utility. The Hemiptera are classified in the main not by colour, but by structure; and outline figures of the distinctive parts in nearly allied genera and species would be of more value than the coloured plates, good as these are. In the table of the genera of the Capsidæ,

for example, which extends over six pages, and in which the differences are often relative, a few good outlines would be invaluable to the beginner. We could also wish to see figures of the immature stages of some of the Hemiptera.

The descriptions are occasionally relieved by accounts of habits, and a full list of the known British localities for each species is given. In a group so comparatively little worked, these localities are at present, probably, as much a guide to the distribution of hemipterists as to that of Hemiptera; the southern counties of England, and Norfolk, however, seem to have been so well searched that species absent from them may be presumed to have a northern or western range in the British Islands. It is a pity that Mr. Saunders has not given at least the outlines of the distribution of each species abroad. A knowledge of this must go hand in hand with that of the British range of an animal, to enable the student to form conclusions as to the relative age of allied forms, and the time and method of the introduction of the various elements of our fauna. Mr. Saunders, in his hints on collection and preservation, lays stress on the necessity for recording localities. It is to be hoped that collectors will all take his advice. Entomologists now, happily, recognise that the value of a specimen is vastly increased when its locality is known; the patient accumulation of such records may furnish invaluable material for future workers at animal distribution and its allied problems.

Altogether Mr. Saunders' work must be considered a most valuable addition to the literature of the British fauna.

THE FAUNA OF BRITISH INDIA, INCLUDING CEYLON AND BURMA: MOTHS. Vol. I. By G. F. Hampson. 8vo. Pp. xxiv. and 527, with 333 woodcuts. London: Taylor & Francis, 1892.

THIS is the first volume on invertebrate animals which has appeared in the excellent series of Indian faunistic works edited by Dr. W. T. Blanford. It contains synopses of the families of moths, and of the Indian genera and species of most of the families included under the terms Sphinges and Bombyces. Mr. Hampson, however, rejects all tribal divisions of the Lepidoptera except the primary separation into butterflies (Rhopalocera) and moths (Heterocera), and he appears to consider the groups of Noctuids, Geometers, Pyralids, &c., as of only family, and not tribal, value.

After a short introduction on the external anatomy of the Lepidoptera, there is a "tree" showing the supposed relationships between the families of moths, followed by a table in which these families are differentiated, mostly by the neuration of the wings. Mr. Hampson has followed, in the main, the classification propounded by Snellen in his "Vlinders van Nederland." British lepidopterists will notice that here, as in Mr. Kirby's recent "Catalogue of Lepidoptera Heterocera," the Sphingidæ are deposed from their place at the head of the moths, and placed next the Notodontidæ. In Mr. Hampson's genetic tree the Saturniidæ appear at the head of one high branch, while the Epicopiidæ, Uraniidæ, and Geometridæ figure at the summit of the other. The Micropterygidæ and Hepialidæ are considered the lowest families, though removed from the main stem, on which the Tineidæ take the lowest place.

The descriptions of the genera and species are illustrated by cuts, the pattern of the wings being generally shown on one side, and their neuration on the other; details of the antennæ, palps, &c.,

are also often given. Mr. Hampson has "lumped" not a few "species"; for example, *Protoparce orientalis*, Butl., is given as a form of the European *P. convolvuli*, L. It is becoming more and more evident that in many groups of Lepidoptera the terms "species" and "variety" must be used according to the taste of the author.

We are promised two more volumes of the work during the next three years, bringing the series down to the end of the Pyralidæ. The remaining families are to be worked out by Lord Walsingham. The volume before us will be gratefully received by students of Indian moths, as it meets a real want, and will prove an excellent guide to future workers.

DOVER COAL BORING: Observations on the Correlation of the Franco-Belgian, Dover, and Somerset Coal-fields. By Francis Brady, C.E. June, 1892. With Reports by M. Victor Watteyne, Mons; James McMurtrie, F.G.S., Radstock; and M. R. Zeiller, Paris. [No place of publication, printer, or date; but Watteyne's Report is dated 16 July, 1892; McMurtrie's, 8 October, 1892; and the section, 19 December, 1892.] Pp. 41. Map (South Wales to Liége), and detailed section of boring.

THIS is the official report on the boring to the westward of the Shakespeare Tunnel, Dover, sunk at the instance of Sir Edward Watkin, on behalf of the Channel Tunnel Company, in search for workable coal seams thought to occur at this point. Although the various Reports differ somewhat, the mysteries surrounding the venture are now dispersed by the detailed section, from which we gather that the following beds were penetrated, commencing at the bottom of a 44 ft. shaft of 9 ft. diameter:—

Depth. Ft.		Thickness. Ft.	in.
91	Lower Grey Chalk	91	0
130	Indurated Chalk Marl	39	0
138	Chloritic Marl	8	0
186	Light-coloured Gault	48	0
256	Dark blue Gault	70	0
259	Sandy Gault	3	0
1113	Various beds representing the Lower Cretaceous and Jurassic	854	0
1136·6	Shale, Sandstone, and Bind	23	6
1140	Coal	2	6
1229	Shales, &c., (with a 6 in. Coal)	88	6
1231	Coal	2	0
1277	Shales, &c.	41	0
1279	Coal	2	0
1311·9	Shales, &c.	25	0
1313	Coal	1	3
1433	Shales, &c.	120	0
1434	Coal	1	0
1456	Shales, &c.	22	0
1458·6	Coal	2	6
1570	Shales, &c.	109	6
1572·3	Coal	2	3
1763·9	Shales, &c.	191	6
1766·6	Coal	2	9
1831	Shales, &c.	64	6
1832·8	Coal	1	8
2038	Shales, &c.	207	0
2039	Coal	1	0
2177·6	Shales, &c.	138	6
2181·6	Coal	4	0

In the report by Mons. Watteyne, which follows the section, we read: "The Coal-measures were reached in beds practically horizontal,

and it was bored in them as deep as 1,940 ft., the horizontal bedding being continuous. In that thickness of 783 ft. seven seams, being more than 1 ft. 6 ins. thick, were found. . . . Two facts are to be quoted about the Dover boring—the *horizontality* and the *regularity* of the beds." But this, it is pointed out, may be due to the boring being just made in the axis of a fold. From analyses made by M. Watteyne, he believes the coal to be of the same quality as the "gras flambant" of the Belgian basin. Under similar conditions of thickness and depth, the average cost of working the Hainaut seams in 1890 was 10 fr. 33 c. (8s. 3d.) per ton.

In Zeiller's report on the fossil plants (which has already appeared in the Comptes Rendus of the Academie des Sciences) the following lists are given:—1,894 ft.: *Mariopteris sphenopteroides* (?), Lesq.; *Neuropteris scheuchzeri*, Hoffm.; *N. rarinervis*, Bunb.; *N. tenuifolia*, Schl.; *Lepidodendron aculeatum*, Sternb.; a *Cordaicarpus* (?) *congruens*, or *Carpolithes corculum*, Sternb. 1,900 ft.: *N. scheuchzeri*; *N. rarinervis*; *N. tenuifolia*; *Cyclopteris*; *Calamophyllites goepperti*, Ett.; *Lepidostrobus variabilis*, L. and H., and *Cordaicarpus*. 2,038 ft.: *N. scheuchzeri*; *Lepid. lycopodioides*, Sternb.; *Stigmaria ficoides*, Sternb. M. Zeiller concludes his report with these words:—It may therefore be concluded from the presence of these two species [*N. rarinervis* and *N. scheuchzeri*] in the Dover boring, that, as presumed by Mr. Brady, the beds traversed by this boring rightly belong to the upper region of the Middle Coal-measures, and, if one may state it more precisely, that they cannot be either more recent than the beds of Radstock, in Somerset, or more ancient than the deepest beds of the upper zone (*charbons gras et fléxus*) of the Pas de Calais.

Analyses of the Dover Coal (lower seams) gives 83·80 carbon, 4·65 hydrogen, ·97 nitrogen, 3·23 oxygen, with heating power in units of 14,867. Eight workable seams have been met with (a total thickness of 16 ft. 11 in.) between 1,181 and 1,875 ft. from the surface, while the lowest yet met with, 4 ft. thick, and 2,222 ft. from the surface, is, as pointed out by M. Watteyne, well within payable depth, as worked in Belgium. The rest of this interesting pamphlet is taken up with commercial speculations, hopes, and anticipations.

LE CAOUTCHOUC ET LA GUTTA-PERCHA. By E. Chapel. Pp. 600. Paris: 1892. Price 20 francs.

THIS work, by the Secretary of the "Chambre Syndicale des Caoutchouc, Gutta-Percha, etc.," gives a complete history of the origin, composition, and manufacture of the industrial products mentioned. The first 80 pages of the book deal with the history of the substances, *i.e.*, the early discoveries and uses, vulcanisation, etc. The second part, pp. 81–295, treats of the botany and geographical distribution of the trees producing the exudation, and appears to collect together much that is valuable both from a botanical and an industrial point of view. Pages 297–340, deal with the physical properties, composition, action of heat and reagents, vulcanisation and its alterations, and the elimination of the sulphur, while the fourth part (pp. 341–519) is devoted to the manufacture and uses. The fifth division of the book is reserved for Gutta-Percha, which is treated in the same systematic way as Caoutchouc; and the sixth and last part deals exclusively with the commercial questions, export and import, custom dues, publications especially concerned with the industry,

and similar matters. There are many illustrations, and the book is interesting not only to the manufacturer but to the general reader.

A TEXT-BOOK OF TROPICAL AGRICULTURE. H. A. A. Nicholls, M.D., F.L.S. 8vo. Pp. xxi. and 312, with illustrations. London: Macmillan & Co., 1892. Price 6s.

THIS book is the outcome ot a competition. The Jamaica Government offered a premium for the best Text-book of Tropical Agriculture specially adapted for the use of colleges and higher schools in that colony, and the author having sent in the manuscript of part of the book before us, it was awarded the prize on condition that some chapters were added treating of most of the cultivated tropical plants not noticed in the original. This was done, and in 1891 the Government of Jamaica published the book, of which Messrs. Macmillan have just issued the London edition, of the convenient size, general excellence of production, and with the bright scarlet cover of their other manuals for students.

The work has already passed the test of practical experience in Jamaica, and is now being adopted by the Governments of other colonies.

The author hopes to serve not only scholastic institutions, but peasant proprietors, owners of small estates, and intending settlers in tropical countries, and the fact, as stated in the preface, and evident from the merest glance at the text, that we have here "not a mere compilation, but the record of experience gained by study, observation, and experimental cultivations," should ensure it success.

The book is divided into two parts. Part I.—"Elements of Agriculture"—is a capital introduction to the science, occupying 85 pages, well arranged in thirteen chapters. The first is brief and introductory, the second and third give a concise account of Soils, how they are formed by atmospheric and other action, their constituents, classification, and properties. Chapters 4 and 5, on Plant Life, tell in about 20 pages of the parts of a plant, its nutrition, composition, and reproduction by seed or vegetative processes. A short chapter follows on Climate, "the greater or less degree of heat, light, and moisture," and the influence thereon of elevation, forest, aspect, or soil. Then under manures is considered the restoration or fertility to an exhausted soil, or the improvement of a poor one, and the most important "general" and "special" manures, their preparation, use, and effect, are described.

Chapter 8, "Rotation of Crops," follows, and is, unfortunately, necessarily short, very little attention, as the author remarks, having been given to working out a proper system of rotation in the West Indies, as has been done in Europe and North America. The importance of experiments is urged, and a course of rotation suggested, viz., yams or tanias (the rhizome of *Colocasia esculenta*) the first year, maize the second, sweet potatoes the third, and castor-oil or a similar crop for the fourth. Stress is also laid on the beneficial mechanical effect on the soil, from the necessary frequent turning over and consequent exposure to the chemical action of the atmosphere, while the rotting of roots of former crops make channels in all directions for water and air. Moreover, a proper system of rotation prevents blight and keeps away destructive insects

that confine their depredations to particular plants, as these pests will be starved out in the seasons intervening between the recurrence of their especial crop.

Drainage, Irrigation, and Tillage Operations form the subjects of the next three chapters, the third containing a description of the various instruments and their uses. Then a few words on Pruning, which, it must be remembered, is a scientific operation quite different from hacking or mere reduction of the bulk of a tree, and finally a chapter (13) on Budding and Grafting; this, by the way, with some good illustrations.

Part II.—Agriculture Products—occupies the remainder or rather more than two-thirds of the book. It is a most interesting account of the principal plant-products of our West Indian Colonies, and the mere general reader, as well as the man of science, will find it so, comprising as it does a well-written history of the growth and preparation of such everyday commodities as coffee, cocoa, tea, sugar, spices, drugs, dyes, or tobacco, "most universally used by mankind."

Tropical cereals, fruits, and food-plants like cassava, arrowroot, yams, and others, are also considered. Take, for instance, the chapter on Coffee. The idiosyncrasies of the two cultivated species, both of which hail from Africa, *Coffea arabica*, and *C. liberica* are mentioned, then its propagation in the seed-beds or nurseries is described, and a word is thrown in in favour of bamboo-pots, made by sawing the stem through an inch or two below each node, as strong and inexpensive. Then follow the preparation of the land, holing, planting out, the necessity for shade, weeding, topping, pruning, manuring; a "catch crop" may be reaped with advantage while the shrubs are growing; the vacant ground between the rows being planted with maize, plaintain, sweet potatoes, or other such food-products. A brief account is given of the enemies of the coffee trees and how to meet them; and, finally, the gathering of the crops and the preparation of the berry for market.

A similar plan is followed in the case of other products, and, in a few cases, sketches of the plant or shoot with the flower, fruit, or seed are also given. These sketches might have been more numerous and exhaustive; they are generally small and convey but little information.

Dr. Nicholls has, of course, dealt only with the West Indian products, some of which are, however, universal in the tropics, but the principles inculcated are so general, that the work will be valuable in the Old as well as the New World, and deserves to be widely known.

Le Thé, botanique et culture, falsifications et richesse en Caféine des différentes espèces. By Antoine Biétrie. Small 8vo. Pp. 156. With 27 figures in the text. Paris: Baillière et Fils, 1892. Price 2 francs.

Struck by the ignorance of most of its consumers as to the cultivation and preparation of tea, by the consequences of its immoderate use, and the numerous methods of estimating the active element therein, the author of this little volume was induced to study some of its properties both from a botanical and chemical point of view. His account should especially interest English-speaking people for, from the statistics given on page 11, they are by far the greatest consumers; the Australian heads the list with 2·9 kilogrammes per person, then follow the English and Canadian with 2 and 1·98 respec-

tively, the French are next with only ·9; then the American with ·6; the Dane with ·2; and other European nations with a rapidly decreasing amount down to the Italian, who is presumably satisfied with ·001.

The book is divided into three parts; the first, dealing with "Botany and Culture," begins with a botanical description including a good figure of a shoot, and also sketches of the leaf and parts of the flower. Chapter II. tells of a few unproductive attempts at European cultivation, and gives a very brief account of the culture and the three yearly pickings in China. Then the preparation of green and black tea is described, the difference lying in the treatment of the leaves; the author expresses his belief in the view that the black owes its colour to the fermentation the leaves have undergone, to which is also due the much greater percentage of ammonia salts than in the green, the proportion being as 40 to 13. Anyhow, as we read in chapter IV., tea is a delicious beverage, though liable to be spoilt in the making; accordingly we are told how they make it in China *fide* General Tcheng-Ki-Tong. In Japan they do it a little differently, and in France differently again; the English method, which strikes us as infinitely superior to them all, is not given.

The physiological action includes not only the well-known effect on the nervous system, but also that on the circulation and respiration.

The second part gives a useful account of its adulteration with leaves from other plants. Rough figures of transverse sections illustrate how by presence or absence of certain tissue elements and arrangements we can distinguish the true from the false.

Part III. is chemical; it tells us what tea contains, and how the green and black varieties differ. The latter are usually much richer in the characteristic alkaloid caffein or thein, which M. Biétrie decides to consider as identical; several of the methods of estimating the alkaloid are described. The last part contains short diagnoses, so to speak, of a large number of "teas," with hints for distinguishing them by physical characters.

We quite enjoyed reading the book, though it has the French failing of falling to pieces as soon as opened.

MESSRS. BAILLIÈRE & SONS, of Paris, have just issued a new catalogue of zoological works, referring especially to fishes (and fisheries), reptiles, and batrachians; also a useful catalogue of phanerogamic botany.

WE gladly welcome the second volume of Mr. Blake's *Annals of British Geology*, containing abstracts of the books and papers published during 1891. The editor's comments, which were not always gratefully received, are now relegated to footnotes. The abstracts are generally well done, but it seems a pity that so much space is occupied by lists of names taken from British Museum catalogues.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

PROFESSOR DR. ANTONIO BORZI, of Messina, has been appointed ordinary Professor of Botany and Director of the Botanic Gardens at Palermo.

DR. FAUSTO MORI, hitherto extra-ordinary Professor of Botany in the University of Sassari, succeeds Professor F. Tornabene as extra-ordinary Professor of Botany in the University of Catania.

PROFESSOR FRIEDR. OLTMANNS, Assistant at the Rostock Botanical Institute, has been appointed extra-ordinary Professor of Botany in the Philosophical Faculty at Freiburg-im-Breisgau.

THE Chair of Vulcanology at Catania, rendered vacant by the death of Professor Silvestri, has been abolished. We are glad to learn, however, that a new Chair has been founded in the University of Naples, to which Dr. H. J. Johnston Lavis has been appointed.

MR. W. R. OGILVIE GRANT has been promoted to the rank of First-Class Assistant in the Zoological Department of the British Museum. He has just completed the manuscript of the volume of the Ornithological Catalogue relating to the Game Birds.

IN accordance with an Imperial decree, issued on February 9, the city of Dorpat (or Derpt) is henceforth to be known as Jurjeff. The name of one of the oldest and most renowned universities in Russia is thus changed. It is merely one more step towards the Russianising of the Baltic province.

THE *Botanical Gazette* of January last informs us that the State University of Iowa has sent Professor B. Shimek to Nicaragua "to follow the route of the canal as near as practicable and make a general investigation of the country, its general character (fertility, climate, &c.), its people, its geology, its flora (special attention being paid to the cryptogamic flora), and its fauna." As the Professor is expected back in Iowa City with his collections "not later than April 1, 1893," he must indeed be a rapid worker to render a satisfatory account in all these departments.

THE new University of Chicago is making a grand start, and we heartily wish it success. The following is a list of the teaching staff, so far as geology is concerned: T. C. Chamberlin, Head Professor of Geology; R. D. Salisbury, Professor of Geographic Geology; J. P. Iddings, Associate Professor of Petrology; R. A. F.

Penrose, Jr., Associate Professor of Economic Geology; C. R. Van Hise, Non-resident Professor of Pre-cambrian Geology; C. D. Walcott, Non-resident Professor of Palæontologic Geology; W. H. Holmes, Non-resident Professor of Archæologic Geology; George Baur, Assistant Professor of Palæontology (Biological Department); Edmund Jussen, Docent in European Stratigraphy. If other departments of knowledge are represented on anything like the same scale, this will be the most completely equipped University in the world. The geological professors propose to start a monthly magazine, which will be issued under the auspices of the University.

WE much wish that space allowed us to print in full the strong circular-letter of protest addressed by Dr. John Young to Lord Kinnear, as chairman of the Scottish Universities Commission, on the "omission of Geology from the subjects for which it is proposed to endow chairs in Glasgow University." At present the teaching is not the duty of any Professor in the University, and it has been carried on by means of lectures, by Dr. Young himself, under the Honyman-Gillespie Endowment. For twenty-seven years Dr. Young has divided the Natural History chair into two subjects—Zoology and Geology—and he says sorrowfully, "I have given the best years of my life to minimise the evils of a duplicate commission, or, rather, professorship, and it is hard indeed to find that my earnest appeal on behalf of my chair, my emphatic evidence that I cannot keep pace with two sciences—either of which is enough for one man's energy—have evoked no hint of help from a Commission appointed to improve the teaching in the Scottish Universities." The University has, strangely enough, divided the History chair into Civil and Ecclesiastical, but ignores the claims of Geology. Dr. Young concludes:—"I fervently hope that before I demit office, Zoology and Geology may be adequately provided for, and my successor spared the weary task which has been mine for so many years."

IN a new storey recently added to Firth College, Sheffield, Professor Denny is provided with accommodation for the work of the Biological department to the extent of a laboratory and museum combined, and a lecture theatre communicating with it. The dissecting benches at present available for work seat about thirty students. It is satisfactory to find the number of students steadily increasing every year. In the development of a teaching museum Professor Denny has so far devoted the very limited fund at his disposal to the acquisition of osteological preparations and embryological models, which already fill the museum cases provided. The rooms are fitted with electric light throughout. Hitherto the biological work of the College has been carried on in the museum of the School of Medicine.

WE notice that the President and both Secretaries of the Geological Society this year are graduates of St. John's College, Cambridge. For a long time this college has been prolific of geological students—indeed, we believe we might say that it has produced more geologists than all the other Cambridge Colleges together. This is probably owing in the main to the influence of Professor Bonney, when tutor of the college. Of the University honours in geology, all the Sedgwick Prizes except one, and all the Harkness Scholarships except two, have been won by Johnians.

ON February 14 the President of the College of Surgeons, Mr. Thomas Bryant, delivered the Hunterian Oration on the occasion of the centenary of the death of John Hunter. Mr. Bryant paid a high tribute to Hunter's efforts to establish surgery as a science, and referred to his wide knowledge of zoology and comparative anatomy. The Prince of Wales and the Duke of York honoured Mr. Bryant by attending.

WE learn from the *Athenæum* that a Natural History department is being arranged in the Imperial Museum at Constantinople.

MR. GREENE SMITH'S collection of North American birds and of Humming Birds has been given by his widow to the Museum of Comparative Zoology, Harvard College.

THE plans of the new building for the Departments of Comparative Anatomy, Palæontology, and Anthropology in the Paris Museum of Natural History have been approved, and it is expected that the extension will be completed by the summer of next year.

WE regret to record the sudden death, on February 14, of Sir Charles Wathen, through whose generosity the transfer of the Bristol Museum and Library to the city is being effected. On the proposition of Sir Charles, the Bristol Town Council unanimously decided, on the date mentioned, to adopt the Museums and Gymnasiums Act, but before the conclusion of the business the city's benefactor died suddenly from heart disease.

THE annual report of the Curator of the Museum of Comparative Zoology at Harvard College for 1891-92 has just reached us. Professor Agassiz complains that "the time which our professors give to elementary teaching is entirely out of proportion to that allowed to them for higher instruction. Thus the facilities for original investigation which might be attained at the museum, and for what it was primarily intended, have been thrown away for many years, owing to the inability of the authorities to appoint men whose duties should lie in this direction." He complains that it is not the province of the museum to provide instructors; that belongs to the University; and he strongly protests against undergraduate instruction, which threatens to overcome the higher purposes of the institution. Professor Agassiz also has some strong remarks on the ignorance of those who, though unconnected with the staff, compile Government reports or "circulars of information." In this country the interference of the Government is confined to expenditure of money, on which matter Treasury clerks have power to overrule the decisions of the heads of the various scientific institutions. The description of the "Blake" and of the "Albatross" specimens goes on apace. The final portion of the report describes the progress of the Newport Marine Laboratory, of which some photographic views of the rooms are given, and it points out the advantages of certain specified extensions of this institution.

ON the 18th of the month, the "Report of Proceedings with the Papers read at the Third Annual General Meeting, held in Manchester, July 5, 6, and 7, 1892," of the Museums Association was published. The volume (142 pp.) bears the misleading date of 1892, and is edited by Messrs. E. Howarth and H. M. Platnauer. The list of associates is small; thus we notice only three members of the staff of the British Museum (Natural History), but of these one is the Director, who, we are glad to learn, will preside this year—the Association meeting in London. The list of articles in the report is as follows:—The Organisation of a Botanical Museum, by F. E. Weiss; The Cultivation of Special Features in Museums, by Rev. H. H. Higgins; The Colouring of Museum Cases, by E. R. Waite; Museum Notes, by J. W. Carr; Dust in Museum Cases, by T. P. Teale; Arrangement of Rock Collections in Museums, by H. M. Platnauer; several articles relating to Art Collections, and, more important to the specialist, a "Catalogue of Types and Figured Specimens in the Manchester Museum," by H. Bolton. This last and exceedingly useful list is an outcome of the suggestions of a Committee formed at the British Association meeting of 1889, and adds another to the lists of types contained in the Bristol, Bath, Brighton, Cambridge, and other centres already published. But why did Mr. Bolton put in *Cyclus scotti*, H. Woodw., and *Myriolepis hibernica*, Traquair, as new species? In the first place, new species ought not to be described in a publication of this kind, and in the second place they have both appeared before this report was published (*i.e.*, distributed to subscribers), though the unwary are not given this information.

THE Croonian Lecture for 1893 will be delivered before the Royal Society of London on March 16, by Professor Rudolph Virchow, of Berlin. The subject is "The Position of Pathology among the Biological Sciences."

NATURAL Science is represented in the programme of the Royal Dublin Society's afternooon lectures by Professor W. J. Sollas and Dr. V. Ball, the former of whom is to speak on "The New Geology," and the latter on "The Scilly Islands."

THE study of ethnology and anthropology is being taken up in Ireland with some enthusiasm. An anthropometric laboratory has been, for several months, established in Trinity College, Dublin; it is under the supervision of Professor D. J. Cunningham and Mr. C. R. Browne. The latter observer joined Professor A. C. Haddon last autumn on an ethnological expedition to the Aran Islands, in Galway Bay, when numerous measurements of the islanders were obtained. Professor Haddon and Mr. Browne have lately contributed their results to the Royal Irish Academy. A course of popular evening lectures on Anthropology is in progress at the Royal College of Science, Dublin, by Professor Haddon, who has also been lecturing on the subject at Belfast.

THE Natural History Society of Northumberland, Durham, and Newcastle-on-Tyne has organised a series of popular Saturday evening lectures, delivered in its Museum. Dr. Embleton, Canon Tristram, Professor G. S. Brady, and Professor M. C. Potter are among the lecturers, and the public are taking full advantage of the facilities afforded them.

THE hundredth anniversary of the foundation of the Literary and Philosophical Society of Newcastle-on-Tyne was celebrated by a Conversazione on February 7. The President (Lord Armstrong) performed and explained some electrical experiments illustrating his recent researches; and the senior Secretary (Dr. R. Spence Watson) read a brief historical sketch of the Society. Next to the Literary and Philosophical Society of Manchester, which was founded in 1781, that of Newcastle-on-Tyne is the oldest in Britain. At the time of its foundation, there were, even in London, few learned bodies. The Royal Society was a hundred years old, and the Society of Antiquaries and the Society of Arts were at work, but the Linnean Society, founded in 1788, was the only association in England devoted to the investigation of any single branch of natural science. During the century of its existence the Newcastle Society had formed a great library, containing many almost unique works, and it is sad to have to relate that, on the morning after the Conversazione, nearly the whole of this library, except the Reference Department, was destroyed by fire.

AT the anniversary meeting of the Geological Society of London, held on Friday, February 17, Mr. Hudleston was re-elected president, and Mr. J. J. H. Teall elected secretary, in the room of Dr. Hicks retiring. Owing to the feeble health of the President, the ordinary official annual dinner was not held this year. In view of our remarks in January on "Scientific Dinners," we are glad to observe that Dr. Henry Woodward, a vice-president of the Society, initiated a less costly entertainment, which, we understand, was quite informal, and was attended by no less than 90 Fellows and their friends. Dr. Woodward occupied the chair, and was supported by Sir A. Geikie, Sir H. H. Howorth, Professor Maskelyne, Dr. Hinde, Professor Rupert Jones, Professor Lapworth, and the secretaries of the Geological Society.

OBITUARY.

FRANCIS ORPEN MORRIS, M.A.

BORN MARCH 25, 1810. DIED FEBRUARY 16, 1893.

BRITISH Naturalists will learn with regret of the death of the Rev. F. O. Morris, which took place at his residence, the Rectory, Nunburnholme, Yorkshire, on February 16. Of those devoting themselves to the spread of an interest in Natural History pursuits among the unlearned, Mr. Morris had long been a conspicuous figure; and his success was gained both by the charm of his personal enthusiasm and by the numerous, well-illustrated popular writings which enabled him to reach a wider circle. His best known and most valuable works are his "Histories" of British Birds, their nests and eggs, and of British Butterflies and Moths, the publication of the first commencing in 1851. The new school of Biology inaugurated by Darwin and Wallace, unfortunately, never found favour with Mr. Morris, and his long series of miscellaneous writings antagonistic to this school began with a small volume on "The Difficulties of Darwinism," published in 1870.

CORRESPONDENCE.

A HYPOTHETICAL EXPLANATION OF THE RESULTS OF INOCULATION.

MR. BULMAN sets forth (NATURAL SCIENCE, vol. ii., pp. 100–109) the arguments for and against two theories—the "Exhaustion Theory" and the "Antidote Theory." Another explanation has been familiar to me for some time and to this he has not referred; I greatly regret that I am unable to say where I got it from, but I rather fancy some part of it was "evolved out of the depths" of my own "inner consciousness."

It is as follows:—

The attenuation of the virus by cultivation under conditions unfavourable to the microbe's very rapid multiplication amounts to an artificial selection, the too-rapidly-multiplying stocks being exterminated by overcrowding and the poisonous effects of their own products. After several cultures, each raised from the survivors of the preceding ones, an "attenuated virus" is obtained, *i.e.*, a pure culture of those stocks whose rate of multiplication is low.

The selection may, however, act in some other way—the net result being that the final culture consists of those stocks which, however they may behave on gelatine or boiled potatoes or in chicken-broth or the blood of living apes or guinea pigs, are

different from those with which we started: *by selection* a new stock has been produced, in some cases it is more virulent, in other cases less so.

The dependency of the protective influence of inoculation upon the rate of multiplication of the pathogenic microbe will be seen when we leave ectasines and anectasines out of account and consider only phagocytosis.

Whatever the nature of the stimulus may be, Cohnheim's demonstration that diapedesis of leucocytes is the dominant phenomenon of the resultant inflammation, together with their action as phagocytes in presence of microbes, may be taken for granted. At the seat of inflammation, then, there is a terrific battle between two armies, the microbes attacking, the phagocytes defending. In both armies there is a great mortality, and the ultimate result of the battle depends upon the rate at which each army is reinforced—*i.e.*, upon the rate of multiplication of microbes on one hand and on the rate of migration of fresh phagocytes to the scene of action on the other. So long as the microbes have the best of it the fever increases. So soon as the phagocytes become sufficiently numerous to cope with the microbes, destroying them more rapidly than they are produced, so soon does the fever begin to abate.

The hypothesis in question is that the stimulus of the poison not only increases the diapedesis of leucocytes (*i.e.*, transportation of the defending army of phagocytes to the scene of action), but also leads to an increased rate of production of leucocytes within the body, so increasing not only the size of the standing army provided for the defence of the body, but also increasing the rate at which new recruits are added to it.

If the microbe army introduced be only an "attenuated virus," then that army will not have time to increase to unmanageable dimensions before the phagocytes appear on the scene; the disease will, therefore, be easily checked, and the net result will be an increase in the strength of the army of phagocytes, whose duty is the protection of the animal against the attacks of armies of microbes.

I am not a pathologist, and only bring this to Mr. Bulman's notice that he may subject it to that same criticism which has apparently destroyed the two theories with which he dealt in his article in the current number of NATURAL SCIENCE.

C. HERBERT HURST.

Owens College, Manchester,
February 13, 1893.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, *now removed to* 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.*

All ADVERTISEMENTS *to be forwarded to the sole agents,* JOHN HADDON & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

ERRATA.

P. 86, line 39. For "pashs" read "parks."
P. 87, line 20. For "Oházal" read "Dhúyal."

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

Prehistoric Man in Switzerland.

Those interested in the study of prehistoric man should read an important paper which has just appeared in the "*Nouvelles archives des Missions scientifiques et littéraires*" (vol. iii., pp. 1–25, pls. i.–iv.) In this paper M. Marcellin Boule gives a clear and readable account of the excavations undertaken by Dr. Nüesch in deposits under a rock-shelter at Schweizersbild, near Schaffhausen, in Switzerland. The explorations are particularly valuable, not only from the interest of the objects discovered, but on account of the care that has evidently been taken to note the exact conditions under which each object was found.

The highest deposit is of recent origin, and contains Neolithic and Palæolithic implements mixed pell-mell with coins, etc. Bed 2 is entirely of Neolithic age, and yields not only implements, but graves with human skeletons (nine have already been found.) It contains, also, bones of various animals, all still living in Switzerland with the exception of the reindeer, the bones of which, however, are thought by M. Boule to have been dug out of the lower strata when the Neolithic graves were excavated. Next comes a rubbly deposit, without trace of man, but containing a loamy seam full of bones of small rodents. Though this bed 3 is of great importance, separating as it does Neolithic from Palæolithic strata, we are not told what the rodents are, or whether they point to warm or cold conditions. The omission is unfortunate, for we should like to know whether any climatic change coincided with the change of races. In bed 4 the reindeer is the dominant form, and with it are found bone tools, har

poons, and needles, hearths and traces of cookery, and worked flints of the patterns commonly found in deposits of the reindeer period. A few sketches of horse and reindeer, incised on flat pieces of bone or limestone, have also been discovered. Bed 5 has as yet yielded no trace of man, but is extremely rich in remains of rodents, like those characteristic of the extinct German "steppe-fauna" of Professor Nehring, who has also identified the specimens from Schweizersbild. Directly beneath lies a mass of rolled pebbles, forming part of the widespread sheet of flood and moraine gravel usually referred to the latest glaciation of the Rhine valley.

These excavations show that part at least of the Palæolithic deposits are newer than the last glaciation of the district. We have, however, no account of the contents of bed 3, which may possibly show a less marked recurrence of arctic conditions, such as seems to have taken place in Britain after the Palæolithic period and before the incoming of Neolithic man. The oldest types of Palæolithic implements, and the large extinct mammals, such as elephant and rhinoceros, usually associated with them, have not been found at Schweizersbild, and it is still a moot point whether they should be looked for above or below the moraine gravels which mark the climax of the Glacial period. It is satisfactory to learn that half the area under the rock-shelter at Schweizersbild remains to be excavated, so that there is still a possibility of clearing up these doubtful points. We shall watch with great interest the results of Dr. Nüesch's excavations during the coming season, after which we hope to receive a fuller report.

Early Flowering of Plants.

Now that the season has begun, we would like to suggest the advisability of a more scientific method in the observation of dates of flowering of plants. One constantly comes across notes recording the exceptionally early appearance of certain flowers. It is forgotten, however, that the premature opening of the flowers may often mean death to the plant, or at any rate a serious waste of material. The production of seed is the final end to which all parts of the plant are modified. If by early flowering the plant is enabled to seed more freely, the date of flowering will tend to become earlier and earlier; but if the flowering is premature, and is not followed by the formation of seed, then the date will become later and later, as the too hasty individuals are killed off.

Observers should not pick these early flowers, they should mark the specimens and return later on to see whether they have, or have not, produced any seed. No one appears yet to have noted whether the buttercups and dead-nettles, which flower more or less all through the winter, produce seed from the winter flowers.

The Great Barrier Reef of Australia.

In our issue for November, we called attention to the work of Mr. Saville Kent on the Great Barrier Reef, and were enabled, through the courtesy of Messrs. W. H. Allen & Co., to give a specimen plate from his remarkably interesting book. We have now received from the publishers a set of twelve beautiful enlargements in permanent photography that have been prepared for the use of Museums and Natural History Societies. These pictures, which measure 15 ins. by 11 ins., show, in a striking manner, the appearance of a coral reef at low water, and, in many instances, even the generic relation of the coral masses can be determined. From a geological or zoological point of view, these photographs will be invaluable. They are published, ready mounted for framing, at half-a-guinea each, or four guineas for the twelve.

Dredging on Rocky Ground.

So much has been written about our knowledge of the deep-sea and its inhabitants, that it is scarcely realised how little we do know. We let down a dredge or trawl at haphazard into the dark, and scrape off certain of the creatures that are to be found on the surface of the muddy bottom; but of those that bore deeply we have no knowledge. On rocky or stony ground we have not even these small opportunities for learning what creatures live there, except the few that may be brought up by the hempen tangles; yet in all probability it is on the irregular rocky bottom, rather than on the monotonous mud flats, that life is the most varied.

If we want to obtain some measure of the extent of our ignorance, we must put ourselves in the place of the inhabitants of the deep-sea. Let us imagine some dwellers in upper air to be anxious to learn what sort of creatures are to be found in its lower strata, or crawl about on the earth's surface, beneath the mantle of cloud and fog. An exploring balloon is sent out, with instructions to dredge carefully. The first haul is made in the dark on rocky ground—say among the houses of London. What would be the result? The dredge would leap from roof to roof, missing the varied life of the streets below. It would bring up some slime out of the gutters, a tangled mass of telegraph wires, some weather-cocks and ventilators, broken chimney pots, a stray cat or two, and perhaps a casual burglar with his luminous apparatus.

The scientific men of upper air, asked to report on the haul, would remark on the wonderful advance that had been made in the knowledge of the deeper air, and of the customs of its inhabitants. They would reconstruct in imagination the world below, and would probably suggest that telegraph wires and chimney-pots are the snares and traps used by burglars for the capture of cats.

No doubt scientific men are very sensitive to any remarks on their ignorance; but we feel impelled to make these comparisons for their own sakes. It should clearly be recognised that the study of the habits of marine animals and of the interdependence of the various species in any fauna, needs something beyond museum work. Properly to understand the natural history of the deep-sea we must adopt a different method of procedure; we must examine the creatures alive, and under natural conditions. This has no doubt partially been done with a few of the species that will live in aquaria; but most marine animals have never been studied in the living state and cannot thrive in confinement.

Submarine Photography.

There seems, however, to be an unused method of research, which ought to yield satisfactory results, and should help us to understand what sort of life is led by the inhabitants of the deep-sea. Photography could, we believe, be used for submarine exploration without much difficulty, and we are surprised that so little has yet been done. Our ordinary photographic lenses would, of course, be useless for this purpose; but lenses to act through a denser medium could easily be made. In the second place, the pressure at considerable depths is so great that a camera of the usual pattern would not do; we must have the camera full of water, and with no air-spaces anywhere. This brings us to the last, and probably most serious, difficulty. How can the sensitive plates be preserved from injury if they are immersed in water? It could be done by laying another glass plate on the film, and photographing through glass, though this would tend somewhat to blur the image. A better way would be, if possible, to waterproof the sensitive film, and dissolve away the varnish before developing the plate.

By some such mode of procedure it is probable that we could obtain photographs as far down as light penetrates. We might even use the electric spark to illuminate the surroundings at great depths; for a hollow sealed sphere of thick glass will stand an enormous pressure. For the study of the manners and customs of the deep-sea fishes, a few sub-aqueous photographs would be invaluable. Coral-reefs also, though so carefully studied and well photographed down to low-water mark, are not understood. A series of photographs of the steep, submerged portion of the reef ought to throw a great deal of light on the vexed question of the origin of coral islands. We hope that some enterprising amateur photographer will take up this question, and begin by photographing in their native haunts the inhabitants of some of our lakes or sheltered sea-lochs.

Fruit-Growing at the Cape.

Fruit-Growing at the Cape is in an unsatisfactory way, according to Professor Macowan's account in the January number of the

Kew Bulletin. The methods of the fruit farmers are "antiquated and conservative," they lead an isolated life, and there is no effective interchange of ideas and information on the subject of their industry, nor do they care to learn what is being done in other countries, or might be done in their own. Moreover, the demand for cheap, coarse fruit among the coloured lower orders in Cape Town is a serious check on any desire for improving the output. "So that the grapes are dirt cheap it does not matter to them how dirty they are, nor are they disgusted at seeing the same baskets that carried the grapes into town piled up among the stable manure the cart takes back to the farm in the afternoon." So little is quality thought of that the producer can grow saleable crops from seedling trees fit only for stocks. "Upon the very few enlightened men who grow fruit on European principles, of the best sorts and in the best manner, the stolid indifference of the market and its pernicious ring of half-coloured middlemen reacts with cruel force. They cannot, somehow, reach the educated purchaser who wants fine and clean fruit, at its best maturity, fit for dessert." This is a sad state of things, and, moreover, the Professor tells us, it is the same with fish. "We all have to be content with what suits the Malays, dispensed Malay fashion." What a chance for someone, with a little capital and patience, to grow and retail his own fruit, and break this vicious circle of middlemen. According to Mr. William Tuck's report from Grahamstown, the growers are quite as casual in the Eastern Province.

The seasons on the two sides of the colony are differentiated, much as in India, by the rainfall occurring conversely, the west having its maximum in winter (June to August), while the east has generally two maxima in the warmer months, November or spring rains, and February or autumn rains. These peculiarities are important in fruit-growing, wine, grape, and raisin production being limited to the Western Province, where alone the summer is sufficiently hot and dry for the proper ripening of the fruit.

Atropin as a Plant Manure.

In a recent number of the *Revue Générale de Botanique*, M. Henry de Varigny describes some experiments on the value of the alkaloid atropin as a plant manure. Goppert (1834) was apparently the first to test the action of alkaloids in general, and atropin in particular, in this connection. He found the result to be the same whether he watered the earth, in which wheat, peas, oats, or cress were sown, with pure water or an infusion of belladonna. The infusion neither accelerated nor retarded germination.

P. B. Reveil (1865) came to a different conclusion. He used a solution containing a known quantity of the soluble sulphate of atropin, and found that it favoured the germination of barley sown

in calcined and washed sand. Moreover, for some plants, he does not say which, it is a "veritable manure"; when watered with a solution they were finer and more vigorous, and flowered better and more quickly than under normal conditions.

In 1887, however, M. A. Marcacci, in his "L'azione degli Alcaloidi nel regno vegetale et animale," asserts that atropin has an injurious effect on germination.

M. de Varigny has made an extensive series of experiments, growing his plants either on washed and heated sand or cotton-wool, and using solutions of the sulphate of atropin of various strengths, from $\frac{1}{15}$ to 1 per cent. Seeds of a considerable number of species were tried, wheat, barley, oats, and other grasses, cress, lettuce, radish, turnip, buckwheat, and others. In every case the experiments were checked by growing the seeds under conditions in every way similar, but without the alkaloid. As a result, he found that almost invariably there was certainly no advantage in the presence of atropin. For instance, several experiments were made with Indian cress, and in almost all the weight of the crop in the control experiment exceeded that in the others, and the greater the proportion of atropin the smaller the crop. This was equally well marked with wheat. Out of 75 seeds sown in each case, 64 germinated in the control experiment, 39 in the $\frac{1}{2}$ per cent. solution of the salt of atropin, and 41 in the $\frac{1}{15}$. In some cases, such as the beet, cabbage, lentil, buckwheat, and pea, the alkaloid had apparently no effect, germination and growth being almost the same in the two series of culture, while in the carrot and lettuce it certainly seemed favourable to growth, but M. Varigny thinks the experiments with these species must be repeated more frequently before arriving at any definite conclusion.

On the other hand, it is quite clear that there are a number of species whose germination and growth is retarded, or even stopped by the drug.

Jade in Upper Burmah.

Dr. Noetling, of the Indian Geological Survey, has issued an interesting report on jade in Upper Burmah. Speaking of the geology of jade, he says that two facts are now established—one, that jade is found in association with and enclosed in an eruptive rock closely resembling serpentine; the other, that this serpentine pierces strata of perhaps lower, but more probably of upper Miocene date. He observes that it is now proved that the jade found in Burmah belongs to a group of eruptive rocks of late Tertiary age, and as it is intimately connected with serpentine, it is probable that it will be found at other places where the latter occurs, when once the outer shell of the serpentine has been pierced. Formerly it was extracted only in the Uru Valley, where it was found in boulders in the alluvial deposits

of the river. Sometimes it was embedded in laterite, and was then particularly appreciated on account of the peculiar red crust which enveloped a core of jade. The boulders were found by digging holes along the banks of the stream, or by diving to the bottom; but recently an enterprising Chinese has imported a diving-bell for the purpose. At the quarries at Tawmaw the mode of extraction is primitive and destructive. The surface of the rock is heated by large fires, and the cold at night is sufficient to crack it without pouring on water. By using crowbars and wedges in the cracks, large blocks of jade are obtained, which are broken with mallets to make them of a size suitable for transport. This rude treatment naturally damages the stone, and therefore the alluvial jade is greatly preferred. Jade, says Dr. Noetling, is a curious example of articles highly prized by certain people, and regarded with complete indifference by others. The Burmese and the Chinese, especially the latter, value a good piece of jade as much as, if not more than, so much gold. Thus, they will pay for a piece large enough for a signet-ring 400 to 500 rupees, while the same piece sold in Europe will fetch little or nothing.

Anæsthetics and Plant Transpiration.

In the February number of the *Botanical Gazette* Albert Schneider gives an account of experiments on the influence of anæsthetics on plant transpiration. Jumelle concluded that sulphuric ether affected this function differently in the light and in the dark; in the former it was increased, in the latter retarded. The increase in the light was supposed to be due to the action of the ether on the chlorophyll bodies, while the retardation in the dark was not explained. Verschaffelt, on the contrary, maintains that ether increases transpiration both in the light and dark. Schneider claims that as these two experimenters used only parts of plants, their conclusions are of little practical value, the natural absorbent organ, the root, being absent. He himself uses small but entire plants of the potato, fuchsia, or geranium. Moreover, they may possibly have confounded *evaporation* with *transpiration*. The former—the mere passive loss of water—is much more rapid in the case of dead than living tissue, while the latter, the active giving up of water vapour, can only occur in living tissue, and is dependent upon protoplasmic activity. Prolonged contact with ether may kill the leaves of the plant under experiment and the result be confused by substitution of evaporation for transpiration.

Experiments on protoplasmic movements in the hair cells of *Primula sinensis* and other plants showed that ether vapour reduces their activity temporarily when exposed for a short time, and permanently if for a longer.

Experiments with whole plants of the potato seemed to show conclusively that exposure to the vapour of ether, amyl nitrite, or

chloroform materially diminishes the transpiration; fifteen minutes with amyl nitrite sufficed to kill the greater part of a plant, while it could withstand a much longer exposure to the other vapours. Ether and amyl nitrite reduced the transpiration both in light and dark, and the different colours of the spectrum had no effect on the influence of ether vapour. Red, yellow, and green lights were obtained by use of bell-jars, the hollow walls of which were filled with fuchsine, "diamond dye" yellow, or a mixture of diamond dye yellow and blue respectively, the conditions of temperature and moisture being constant. The average results of a series of experiments under otherwise normal conditions, showed a gradual slowing of the rate of transpiration from diffused white light, through yellow, red, and green, to darkness.

A set of experiments with the potato and a fuchsia showed that moisture did not affect the influence of ether and amyl nitrite; moreover, at no time, though the atmosphere was practically saturated, did the transpiration approach zero. Kohl, on the contrary, claims that in the saturated atmosphere it is zero.

Finally, to show the cause of Jumelle's apparently erroneous conclusions, Schneider made a series of experiments with leaflets of the potato. He found that, when exposed to ether for three hours or less, either in light or darkness, transpiration, estimated by loss of weight, was retarded, but with an exposure of 3½ or 6 hours the loss of weight increased in both cases. Owing to the long exposures, the ether had stopped the protoplasmic activity, and hence increased evaporation in both cases, but most in the dark, because in the dark the leaflets were killed earlier.

The Landslip at Sandgate.

Sandgate, on the Kentish coast, was a good deal damaged by landslips on March 4 and 5, though the accounts of the mischief done have been considerably exaggerated. We are informed by Mr. Topley, who is intimately acquainted with the geology of the district, that the whole town is built on an old landslip, and that the western part of this disturbed mass has slipped again, owing principally to the exceptionally heavy rainfall experienced during the past winter. Mr. Topley will give an account of the landslip, and will exhibit photographs, at the meeting of the Geologists' Association, to be held at University College, on April 7.

Target Practice and Fisheries.

From the Report of the Target Practice (Seawards) Committee, just issued, we observe that some interesting evidence was given by Mr. Calderwood, of the Plymouth Marine Biological Station, as to the position of favourite fishing grounds on the South Coast. The Committee say: "The evidence of Mr. Calderwood . . . . satis-

fied us that it is of great importance that the military and naval authorities should be furnished with accurate information respecting the position of the favourite fishing grounds in the localities where target practice is carried on, and of the times of tide and seasons when fishermen in the neighbourhood are ordinarily engaged upon particular grounds, and of the character of the pursuits followed. And of the gear used by fishermen at different times of the year in fishing for various kinds of fish." Mr. Calderwood has furnished to the Committee, who have reproduced it in their Report, a map showing the localities for special fish off Plymouth during the month of April. Of course, these localities vary from week to week, and it would be desirable, and no doubt important, if a large series of such sketch-maps could be published. The evidence offered as to the effect of the electric light in attracting or frightening fish is conflicting, and the same may be said of the noise of the firing of heavy guns. We note that the index shows a great improvement on the general run of indexes to Blue Books, it is really a useful *précis* of the whole Report.

Tertiary Mollusca of Florida.

We have received from Mr. W. H. Dall the second part of his "Tertiary Mollusks of Florida" (*Trans. of the Wagner Free Institute of Science*, vol. iii., pp. 201–474). This part completes the account of the Gasteropoda, and places in our hands, at last, better materials for the study of former zoological provinces. No group of Tertiary fossils has been so well examined in many different parts of the world as the Gasteropods, and by a careful comparison of the different monographs that have lately appeared, we ought to be able to obtain information as to the former continuity or isolation of the different areas. An account of the Tertiary marine gasteropods of the Pacific slope of America is still wanted. Until they have been examined, it will be difficult to speak with confidence as to the dates of submergence of the isthmus of Panama, and we also cannot deal with the possible diversion of the Gulf Stream at different epochs, and its effect on the climate of Western Europe.

Tertiary Mollusca of Piedmont.

We are glad also to observe that the splendid monograph on the Tertiary Mollusca of Piedmont, left unfinished on the death of Professor Bellardi, is steadily progressing under the hands of Professor Sacco, of Turin. Another part has lately appeared, and we may expect before long to see the completion of the gasteropods. The Tertiary molluscan fauna of Italy is so prolific, that the monographing of those found even in a single region is a herculanean task. We hope that Professor Sacco will receive sufficient encouragement and

assistance to enable him to complete the work. When will the Palæontographical Society complete the monograph on our own Eocene Mollusca, left unfinished on the death of Messrs. Edwards and Wood?

Sea Pellets.

In the February number of the *Revue Générale de Botanique*, W. Russell has an interesting note on these peculiar felt-like balls thrown up by the sea, or sometimes found on the shores of inland lakes. They occur in great abundance round the Mediterranean and seem to have puzzled the old naturalists. The Greeks knew of them, for Galen and Aristotle recommend the use of their ashes as a remedy for scrofula. Later, Constant and Cornarius placed them in the genus *Alcyon*, a group which, however, included sponges and corals as well as sea-weeds. Cesalpinius, in his "De Plantis" (1583) describes them as formed of the down of the sea, but Imperato, nearly a century later, refused to rank them as plants, more nearly recognising their true nature as "balls of chaff and hair made by the rolling of the sea," and showing that they must not be confused with the hair-balls found in the stomach of goats and other ruminants.

At the end of the last century, Draparnaud found the pellets collected on the Mediterranean shores to be composed of fibres felted round a fragment of that curious marine flowering plant the sea-wrack (*Zostera*), and, later on, Bory de St. Vincent described similar ones from the Straits of Dover.

In 1857, Germain de Saint Pierre remarks on the spherical pellets of brown felted fibres found on the coast of Provence, and used by the hunters of the district as a wad for their guns, and finds that they consist "of fibres persisting on the stems of *Posidonia Caulini* after the destruction of the leaves, of which they represent the nerves."

Weddel, in 1867, explained their formation thus: The old root-stocks of the plant are torn into shreds and broken by the continual shock of the waves, while larger fragments rolled up and down the carpet of fibres, especially where the shore forms a gentle slope, catch them up, and become the nucleus of often very large balls.

Frequently, however, the core is absent, as in many of the cases quoted by Russell from Italy, where they were usually spherical or ovoid, solidly felted and remarkably homogeneous, but sometimes fusiform with a rigid axis, easily recognisable as a fragment of the rhizome. The author goes on to describe some pellets collected in the Island of St. Marguerite, which he has found to consist of scales of pine cones in all degrees of alteration, some intact, others reduced to a few fibres, and numbers of filaments generally rather thick, and only 5 or 6 centimetres long at the most, but mingled with others long and threadlike but less numerous. In the short filaments the bordered pits and resin canals characteristic of the pines were easily seen under

the microscope, but the threads were of a quite different structure, consisting of long cells, almost all of the same form, and, according to the author, evidently belonging to a monocotyledon presumably *Posidonia*, numerous rootstocks of which occurred on the beach mixed with the pine cones. These long filaments had played the part of a thread uniting the smaller *débris*, which otherwise would probably have remained separate.

Other cases are mentioned in which the *Posidonia* fibres act in this way. Bits of sponge may form a nucleus round which they group themselves, while seaweeds may get involved. An example exists in M. Bornet's herbarium, a green alga in the shape of a ball, in which the *Posidonia* filaments are caught like pins. The latter example is comparable with the instances cited by Masters, and found in certain small English lakes, of Confervæ interlacing and forming a ball with the leaves of the larch.

These recall the so-called aegagropilous algæ, which are merely species of the green alga *Cladophora*, detached at an early stage from their support, and rolled by currents. As Russell suggests, all submerged bodies, under certain circumstances, might form similar pellets. The pine needle balls from the lakes of the Engadine are well-known, and M. Jaccart, of the Lausanne Museum, states that in a little creek in the Lake of Geneva, where the water is unceasingly disturbed by currents, fine felted pellets may be seen formed by shavings from a saw-mill.

Evolution of Premolar Teeth.

That indefatigable worker, Professor W. B. Scott, of Princeton, has contributed to the last issue of the *Proc. Acad. Nat. Sci. Philadelphia* (1892, pp. 405–444), a very interesting paper on the evolution of the premolar teeth of mammals, especially those of the Ungulates and their allies. It is clearly shown that premolars start as simple cones, and receive their first accression by the addition of a smaller cone on the inner side. It is thus evident that the protocone in these teeth is external; whereas, according to the general interpretation, in the true molars it is an internal element. Professor Scott is thus led to conclude that in Ungulates like the horse, when the upper premolars exactly resemble the molars, the various cusps of these two series of teeth are not homologous with one another. He, therefore, proposes a new series of terms for the premolar cusps.

The idea that such precisely similar teeth have a totally different origin for their cusps is sufficiently startling to make us anxious to ascertain whether the inductions on which the theory rests are well founded. Now, at the conclusion of his paper, Professor Scott mentions that Herr Röse has recently come to the conclusion that in quadritubercular upper molars the cusps have been wrongly identified, and that the protocone is, after all, external. If this be so, adds Pro-

fessor Scott, the premolar and molar cusps are really homologous, and the long series of new terms proposed for the cusps of the former quite superfluous. The obvious comment on this is *festinâ lente*.

The Expedition to Lake Rudolph.

Advices have been received from some members of the Villiers Expedition to Lake Rudolph. This expedition, it will be remembered, left England last October for the purpose of exploring the country around the Lake, going through southern Somali-land, and returning through the northern part of that "horn" of Africa. At the same time, comes a despatch from Sir Gerald Portal to the Earl of Rosebery, containing the astonishing information that "This afternoon we reached Sambaru, 35 miles from the coast, and here have been overtaken by Lieut. Villiers, *who informs me that he has been authorised to attach himself to this expedition as one of my staff*, defraying, however, his own expenses throughout." We should like to know *who* authorised Lieut. Villiers to leave those whom he had taken out from England under promise of leadership, and who will be responsible should harm come to any of his followers?

Phosphates from India.

Sir J. B. Lawes has recently received, through the India Office, a consignment of Phosphates from Madras, with a view of their commercial value being ascertained. The specimens, of which we have been favoured with samples, are well-formed nodules, with a nearly smooth buffish coat, and internally appear very pure. Unfortunately, they show no traces of fossils; but they come from Utatur, where there are both Cretaceous and Eocene Beds.

Our Monthly Selection.

One does not expect accurate science in an ordinary newspaper, but yet it is not unreasonable to look for a general knowledge of the natural productions of our colonies in a paper supposed to be devoted to their interests. The following is taken from *The Colonies and India* of March 11, 1893, but we can assure would-be emigrants to New Zealand that they need not be afraid of meeting one of these birds, resembling an ostrich, but very much larger. Unfortunately for science, the Dinornis has been extinct for many years:—

"Dr. Reichenow read a paper at the last meeting of the Ornithological Society of Berlin, in which he gave some particulars of the finding of remarkable remains of gigantic birds in Argentina, double the height of the ostrich, which represent our living cassowaries and ostriches. In modern times many kinds of birds are becoming extremely rare, especially the dinornis races of New Zealand, which

are beginning to die out. Dr. Reichenow presented a specimen of one of these birds, a very rare and costly *Apteryx hasselti*, which inhabits the northern island of New Zealand, and while resembling an ostrich, is very much larger."

Dr. Nikitin has issued his seventh Record of the Geological Literature of Russia (for 1891). This record contains 452 items, the titles and a *précis* of the contents being given in Russian and French. Dr. Nikitin must be congratulated on the issue of this most useful work, and especially on the French translation, but for which, to the majority of foreigners, the rich geological literature of Russia would be unavailable. The most gratifying features, perhaps, in the book are the numerous and voluminous reports issued by the Governments on the nature of the soil and its agricultural value, and other matters of applied geology. This is the true aim and end of geology as far as the public is concerned, and we would like to see some works of this nature issue from our own Governmental office.

We are glad to observe, in the "Report of the Committee on the Present Condition of the Ordnance Survey," noticed under "Some New Books," that the Commissioners state (p. xxxii.) "that the absence of a path on the Ordnance Survey Map is no proof that there is not a right of way." This is a considerable improvement on the legend which appears on some of the recent one-inch maps, which reads thus: "The representation on this map of a road, track, or footpath is no evidence of the existence of a right of way," and we hope the legend will promptly be altered in all cases to the revised version.

Mr. John H. Cooke has an interesting paper in the February number of the *Mediterranean Naturalist* on "Some evidences of the occupation of the Maltese Islands by Prehistoric Man." He describes the evidences obtained by previous observers, and then relates his own results from the exploration of the Har Dalam Cave in 1892. Mr. Cooke found a stone implement and a human metacarpal associated with *Ursus arctos*, *Cervus*, *Elephas* and *Hippopotamus*, together with fragments of pottery of prephœnician page. The main results have been contributed to the Royal Society, and to the *Geological Magazine* (December, 1892), but the collating together of all the observations makes an interesting and useful article.

The leech *Hirudo brevis*, described by Grube from Chili in 1871, has been lately examined by Blanchard, who has given his opinion to the Academy of Sciences, Paris (*Compte Rendu*, 27th February, 1893), that it should form the type of a new genus, which he calls *Mesobdella*. After describing in detail the structure of the animal,

Blanchard goes on to say, by its peculiar characters, *Mesobdella brevis* (Grube) connects in a remarkable manner the Glossiphonidea with the Hirudinidea. Among the last, it approaches more nearly the Hemadipsinæ, as much by its mode of life as by the disposition of its eyes; but it is clearly distinguishable from them, as from all the other Hirudinidea by the great compression of the somites. The existence of this intermediate form shows that these two families are derived from a common stock, from which the Glossiphonidea have apparently deviated less than the Hirudinidea.

MM. CHEVREUX and DE GUERNE have some interesting observations on Crustacea and Cirrhipedes commensal on the turtles of the Mediterranean in the *Comptes Rendu* of the Academy of Sciences, Paris, 27 Feb., 1893. The observations were made during some excursions on the "Hirondelle," the "Actif," and the "Melita," in 1892. To a specimen of *Thalassochelys caretta* were attached *Lepas hilli*, some young *Conchoderma virgatum*, and a *Platylepas hyssexlobata*, as well as the following crustacea: 16 *Hyale grimaldii*, 1 *Platophium chelonophilum*, 1 *Caprella acutifrons*, 4 *Tanais cavolinii*, and 3 *Nautilograpsus minutus*. On another specimen of the same Chelonian, which was abundantly garnished with the Alga, *Polysiphonia sertularioides*, no less than 259 Hyalæ, and several hundreds of Caprettæ, with other forms, were found.

MESSRS. WARNE & Co. announce the forthcoming issue of a new serial illustrated Natural History, edited by Mr. Lydekker The publishers have purchased electros of the greater number of the beautiful engravings in the third edition of Brehm's "Thierleben," and these, with the addition of coloured plates, ought to make the work highly acceptable to the public, altogether apart from the text. A large portion of the work will be written by the editor himself, but the assistance of specialists in certain groups has been secured.

Grevillea for March shows a steady improvement on its predecessors, especially in point of the illustrations. Mr. Batters makes a new genus, called Giffordia, in memory of the late Miss Gifford. It is made out of *Ectocarpus*. That old convenient genus is slowly breaking up, and in time will occupy a place like that of *Conferva*—the mother of genera. The reason, in the present case is the valid one of the discovery, by Dr. E. Bornet, of differences in important particulars between the male and the female cells, while typical *Ectocarpus* is isogamous. A great deal of work tending towards the unsettling of the present grouping of genera in Phacophyceæ has been recently done. There are the remarkable observations of Dr. Bornet on *Nemoderma Tingitana* (Algues de Schonsboe in Mem. Soc. Nat. Sc. de Cherbourg, 1892), and those of Miss Mitchell and Miss Whitting on

Splachnidium rugosum (Phycological Memoirs, part i.), besides many minor observations of great value when taken into relation with others. Mr. Buffham, in his excellent "Algological Notes" in this current number of *Grevillea* describes plurilocular sporangia of *Chorda Filum* (of interest in the connection just alluded to) and a new species of *Giffordia*. A most valuable observation is that of the conjugation of zoogametes in *Cladophora lanosa*. Mr. Buffham deserves very hearty applause for these admirable notes—the more so since they are put forward very modestly in the shape of notes.

A TRANSLATION of Von Kennel's paper on "The Affinities and Origin of the Tardigrada," will be found in the March number of the *Annals Mag. Nat. Hist.* Kennel agrees with Plato in considering that the Tardigrada can be brought into relationship with the tracheata Arthropods, but he does not agree with him in considering that they lie nearest to the root of the Tracheate stem, or that they show most clearly the transition from the Annelids to the air-breathing Arthropoda. He goes on to point out the many resemblances to be found with the dipterous larvæ, and says that though it is by no means his attention to put forward the dipterous larvæ as actual ancestors of the Tardigrades, no single form so simply and so readily enables the student to interpret the Tardigrade body.

MR. FOX-STRANGWAY'S Memoirs on "The Jurassic Rocks of Britain" (*Mem. Geol. Survey*), dealing with the Yorkshire areas, has just been issued, and we hope to give a notice of it as soon as the second part appears. In the meantime, it may be asked why the date 1892 is put upon the title-page? It is true that the preface is dated 28th April, 1892, but the tell-tale printer's date, December, 1892, occurs on the same sheet. Why this eight months' delay.

IN our January number, we referred to the visit of Sir H. Maxwell and Mr. Hasting to Thessaly, to investigate the value of Loeffler's method of destruction of field-voles by mouse-typhus. Sir H. Maxwell has an article on the subject in *Blackwood's Magazine* for March, in which he points out that the remedy is not effectual, on account of the difficulty of spreading the soaked bread, and, moreover, it is too costly to become general. Five francs' worth of the liquid suffices only for two acres, and to clear a farm of 6,000 acres would cost no less than £600 for typhus-broth alone, not counting bread and cost of labour.

IN his presidential address to the Geological Society of America (*Bulletin Geol. Soc. America*, vol. iv., pp. 179–190, February 27, 1893), Mr. G. K. Gilbert deals with a subject which has lately been discussed in NATURAL SCIENCE. He speaks of the permanence of

continents and ocean basins, and is strongly inclined towads the view that in the main they are permanent.

In the *Geographical Journal* for March, Dr. Hugh Robert Mill has a short paper on the Permanence of Ocean Basins.

In the *Journal of Botany* for March, Miss Barton makes progress with her "Provisional List of Marine Algæ of the Cape of Good Hope," and there is a paper by E. D. Marquand on the "Mosses of Guernsey."

There is a portrait and an interesting and full account of the career of Frederick Courteney Selous, the Nimrod of South Africa, in the March number of the *Review of Reviews*. The thirteen pages devoted to Mr. Selous bristle with adventures, and form altogether an instructive and readable record of the dangers and excitements of hunting big game. Mr. Selous is more than a hunter, for he has many times enriched the national collection with rare or new beasts; some of his most stirring experiences are illustrated in the article.

We learn from a telegram received from Berlin on the 15th inst. that Dr. Stuhlman, one of Emin Pasha's late companions, has arrived at that place. He has brought with him two female dwarfs from the Upper Ituri district of Central Africa, who will be examined scientifically by Professor Virchow. The telegram, however, does not contain any news of Emin.

Miss Agnes Crane informs us that the fifteen-spined sticklebacks (*Gasterosteus spinachia*) have just commenced their annual nest-building in the Brighton Aquarium.

Messrs. L. Reeve & Co. have in preparation a new work on the British Aculeate Hymenoptera from the pen of Mr. Edward Saunders, uniform with the same author's work on the Hemiptera Heteroptera just completed, and noticed in our last number.

On March 16, at a meeting of the Royal Society, Professor Rudolf Virchow delivered the Croonian Lecture, taking for his subject "The Position of Pathology among the Biological Sciences."

I.

The Mammals of Kilima-njaro.

GREAT interest attaches itself to Kilima-njaro, the mountain-mass in which equatorial Africa attains its highest development above the sea-level. At one time it was supposed, not unreasonably, that the investigation of its flora and fauna would materially assist in the solution of the problem of the former extension of northern forms of life into southern latitudes. Such has not proved to be the case, so far as our explorations have at present gone. But the study of the organic forms of Kilima-njaro, and of the high district that surrounds it, has, nevertheless, resulted in the discovery of many unexpected facts in distribution; and the recent publication of Mr. True's account of the mammals collected by a well-known American explorer—Dr. Abbott—in that region (1), invites us to offer a few remarks on that particular branch of its fauna.

The snows of Kilima-njaro were first observed, as is well known, by the German missionary, Rebmann, in 1848. Von der Decken, New, Fischer, and Joseph Thomson were the next four explorers who saw the mountain, and ascended its slopes to a greater or less extent; but, except some bundles of dried plants, and a few insects and bird-skins[1] (obtained by Dr. Fischer), little if any information as to its natural history was derived from these expeditions. In 1883 our ignorance of the fauna and flora of this specially interesting district was brought before the Royal Society and British Association by the writer, and a "Kilima-njaro Committee" was formed to endeavour to improve our knowledge of this subject. The result of the operations of this committee was the expedition of Mr. H. H. Johnston, in 1884. Mr. Johnston's instructions were to proceed direct to Kilima-njaro and pitch his camp there, high up, for six months, and to collect as much as possible in the vicinity of the snow-line. Unfortunately, Mr. Johnston, although he carried out the committee's directions as far as possible, was hampered by want of means. As he has explained to us in his most interesting and attractive narrative (2), being unaccompanied by European collectors, and failing to obtain the aid of native assistants, he was unable to accomplish all that could have been wished, in spite of his well-

[1] Among these were the first specimens of *Turacus hartlaubi*—a fine Touraco peculiar to Kilima-njaro.

known energy. But it is to Mr. Johnston that we owe our first acquaintance with the mammal fauna of Kilima-njaro, and, as will be seen by reference to Mr. Thomas's list, published in the "Proceedings" of the Zoological Society for 1885 (3), seventeen species of the mammals that inhabit this mountain thus first became known to us. The mammals obtained and observed by Mr. Johnston belonged mostly to well-known and widely-distributed species, such as usually fall into the hands of a first explorer; but Mr. Johnston made many good notes on the subject (4), and was the discoverer of the fine local form of the Guereza Monkey (*Colobus caudatus*), which is restricted to Kilima-njaro. Mr. Johnston remarks as follows on the monkey life of the district:—

"During eight months on the Congo, I only saw monkeys twice in a wild state, and that in one place only; and throughout my entire stay of sixteen months in West Africa, I can only remember six occasions on which I actually beheld these animals in a state of nature. On the other hand, I had scarcely left the East Coast to journey towards Kilima-njaro when monkeys showed themselves abundantly in the wilds.

"The first to attract my attention were the baboons, probably the species known as *Cynocephalus hamadryas*, *C. sphinx*, and *C. babouin*. They were generally found on the outskirts of native plantations, where they almost subsisted on the maize and other food-stuffs stolen from the gardens of their more highly-developed fellow primates. In the inhabited region of Kilima-njaro, generally known as the country of Caga, baboons were strangely abundant. They were generally in flocks of fourteen to twenty, of all ages, and both sexes. They were so little molested by the natives that they showed small fear of man, and, instead of running away, would often stop to look at me about twenty yards off, and the old males would show their teeth and grunt. I have frequently seen the natives driving them from the plantations, as they might a troop of naughty boys, and the baboons retreating with swollen cheek-pouches, often dragging after them a portion of the spoil. On one occasion, in a river-bed at the foot of Kilima-njaro my Indian servant, ordinarily a very plucky boy, met a troop of baboons, who, instead of fleeing up into the trees, came running towards him in a very menacing manner, and he was so frightened at their aspect that he took to his heels. The baboons followed, and, but that the boy forded the shallow stream, and put the water between him and his pursuers, he might have had an awkward contest. I killed a baboon once in Caga, one of a troop who were rifling a maize plantation, and its companions, instead of running away, surrounded the corpse and snarled at me. As I had fired off both barrels of my gun, and had no more ammunition, I went back to the settlement to fetch some of my followers, and upon the approach of several men the baboons ran off. We picked up the dead one and carried it back. It was a female, and apparently young and tender. Out of curiosity, I had its flesh cooked the next day and ate it, hoping in this lawful way to form some idea of the practice of cannibalism; I can only say that the succulence and quality of this creature's flesh were quite unexceptionable. I have noticed this with most of the species of Old World monkey I have as yet tasted. During my four months' stay in Mandara's country I ate the common *Cercopithecus* constantly, and found it made a very tooth-

some stew. The most remarkable monkey in all this region is probably the *Colobus*, which apparently offers a new variety or sub-species in the country round Kilima-njaro. remarkable for having an entirely white, heavily-plumed tail. The common species, with a black tail tipped with white, I have shot in the forested plains near the coast. The Colobus monkey is almost the only one that quite avoids the neighbourhood of man; the other genera frequent the vicinity of native plantations, and doubtless profit by the abundance of cultivated food.

Block lent by the [*Proc. Linn. Soc.*

Fig. 1.—GRANT'S GAZELLE (*Gazella granti*).

I never observed any *Galago* (a lemuroid animal) in this district, nor do the natives speak of one, although it is a genus well represented in other parts of Africa."

The sporting trip of Sir Robert Harvey and his friends to Eastern Africa, in 1887, and the consequent publication of Sir John Willoughby's "East Africa, and its Big Game" (5), resulted in another important contribution to our knowledge of the

mammals of the Kilima-njaro district, though no attempt was made to form a scientific collection. In the vicinity of what is well termed the "hunter's paradise" of Taveta, situated at an elevation of about 2,400 feet on the south-eastern slope of the great mountain, antelopes and other large "game-animals" were abundant. Speaking of one of his excursions from their headquarters, Sir John Willoughby says: "The grassy plain through which we marched was simply crawling with hundreds of *Gazella granti*, Wildbeest, Hartebeest, and Zebra." At that time, "any amount of game" was to be seen there, although these days are, we fear, already past and gone for ever. During their four months' stay in this earthly paradise, some 350 head of large "game" were obtained, amongst which were no less than 66 Rhinoceroses. The most abundant Antelopes in the district appear to have been the Coke's Hartebeest (*Bubalis cokei*), the Grant's Gazelle (*Gazella granti*), the Mpallah (*Æpyceros melampus*), and the Waterbuck (*Cobus ellipsiprymnus*).

The Hartebeest was "quite the most common Antelope on the plains, being found everywhere in immense herds." Grant's Gazelle, perhaps the finest species of this beautiful group, was also "common everywhere on the open plains," one male being accompanied by as many as from ten to fifteen females. The Mpallah is stated to be abundant in the bush as well as on the plains, and the Waterbuck to be "found everywhere near the rivers and marshes." Of other Antelopes met with by these fortunate sportsmen, we find the Eland (*Taurotragus oreas*) mentioned as "rather local." Both males and females in this district are more or less striped. The large Kudu (*Strepsiceros kudu*) was only seen on two or three occasions, and the Lesser Kudu (*S. imberbis*), though found in the bush near Taveta, is not common.

A very charming species, of which Sir Robert Harvey and his party were, I believe, the first to obtain perfect specimens, is Thomson's Gazelle, discovered by Mr. Joseph Thomson during his journey through Masailand in 1883, and discriminated by Dr. Günther from a pair of horns. This handsome little Gazelle was "found in large numbers in the plains to the south-west of the mountain, and occasionally mixed with herds of *Gazella granti*. Another graceful Antelope, "plentiful on the plains, and in thin thorny bush near Taveta," was the Oryx of the district. In his notes on the animals met with, Mr. Hunter refers to this Oryx as the Beisa (*Oryx beisa*); but, as has been recently shown by Mr. Thomas (6), the Oryx of Kilima-njaro is not the true Beisa, but an allied species, distinguished by its pencilled ears and different markings. Altogether, examples of sixteen species of Antelopes were obtained by Sir John Willoughby and his friends in the vicinity of Taveta, besides one or two others observed or heard of. At that epoch also the Giraffe and Zebra were very common in the plains near Taveta, and the Elephant was found in the forest of Kilima-njaro. Mr. Hunter's Appendix to "East Africa

Fig. 2.—Thomson's Gazelle (*Gazella thomsoni*).

Proc. U.S. Nat. Mus.

(vol. xv., pl. lxxvii.)

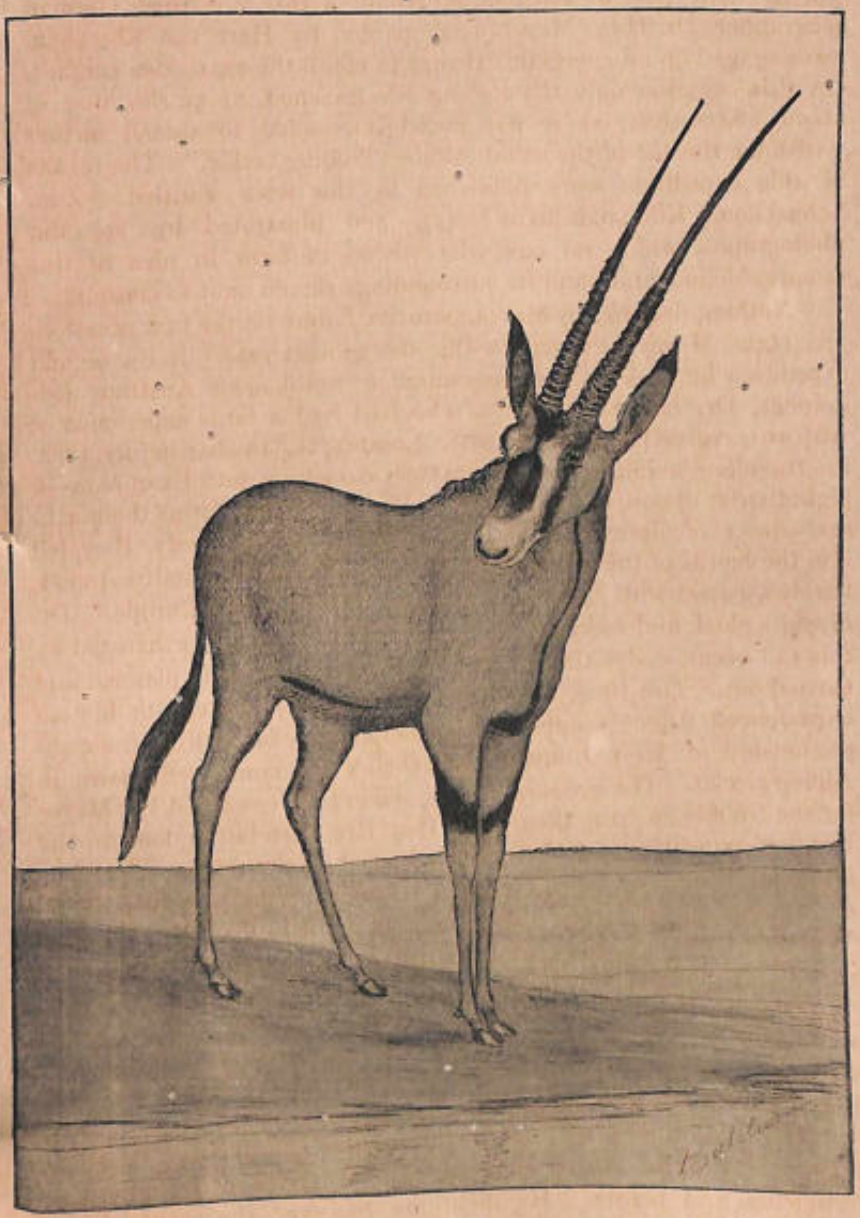
and its Big Game" also contains notes upon other mammals obtained and observed during this remarkable excursion.

A few months after Sir Robert Harvey and his friends had quitted their post at the foot of Kilima-njaro, a learned German geographer, Dr. Hans Meyer, accompanied by Herr von Eberstein, was engaged on an energetic attempt to climb the untrodden summit. On this occasion only the ice-cap was reached, at an elevation of about 18,000 feet, as it was found impossible to ascend further "without the aid of the usual Alpine climbing tackle." The results of this expedition were published in the work entitled "Zum Schneedom Kilimandscharo" (7), and illustrated by splendid photographs, which no one who wishes to form an idea of this remarkable mountain and its surroundings should omit to consult.

Nothing daunted by his comparative failure on the first occasion, Dr. Hans Meyer returned to the charge next year. In his second expedition he took as his companion a well-known Austrian geographer, Dr. Oscar Baumann, who had had a large experience of African travel on the western coast. Landing at Zanzibar in July, 1888, the travellers indulged in a preparatory excursion into Usambara—a high district opposite the island of Pemba, which forms the north-east corner of German East Africa; there, unfortunately, they fell into the hands of the revolted Arabs, and, after serious maltreatment, barely escaped with their lives back to Zanzibar and Europe.[2] Dr. Meyer's pluck and energy were, however, by no means exhausted by this sad event, and a third expedition was immediately planned and carried out. This time, Dr. Meyer determined to take with him an experienced Alpine climber, and was fortunate in securing the companionship of Herr Ludwig Purtscheller—a name well-known in Alpine circles. The event on this occasion fully rewarded Dr. Meyer for the trouble he had taken. All that had been left undone on the two first expeditions was fully accomplished on the third. The great crater of Kibo was discovered, the summit of the mountain at an elevation of 19,720 feet was attained, and named (of course) "Kaiser Wilhelm Peak," and an accurate survey of the whole district was carried out, besides which scientific collections in several branches of Natural History were formed. The results of this successful expedition were given to the world in 1891 in the splendid monograph entitled "Across East African Glaciers" (8), of the merits of which it is almost impossible to speak too highly.

Dr. Meyer does not seem to have paid much attention to the mammals of Kilima-njaro, although he made special collections of its butterflies and plants. He mentions, however, the occurrence of "The Eland" on the mountain at the extraordinary height of 15,000 feet. As, however, this animal was only recognised by its "footprints and droppings" in the boggy ground, I should rather imagine it might

[2] See Dr. Baumann's account of this perilous journey in his charming book "In Deutsch-Ostafrica wahrend der Aufstandes." Vienna: 1889.



Proc. U.S. Nat. Mus.] Fig. 3.—Pencil-eared Oryx (*Oryx callotis*). [vol. xv., pl. lxxvi.

have been the Koodoo, which Mr. Johnston also met with on Kilima-njaro (*op. cit.*, p. 354), and which Mr. Hunter believes that he recognised (at 9,000 feet) by "the crumbling core of an old horn."

Another passage in Dr. Meyer's work is of still greater interest as regards the mammals of Kilima-njaro. When climbing the ice-sheet of the Kibo crater from the west, at a height of 20,000 feet the travellers found the dead body of an antelope—one of the small species they had noticed on the pasture-lands below. This was, in all probability, *Cephalophus spadix*, discovered by Dr. Abbott, to whose labour we must now proceed to refer.

The American naturalist and explorer, Dr. W. L. Abbott, spent nearly eighteen months on Kilima-njaro and its vicinity in 1888 and 1889, and collected a splendid series of its mammals, which was presented to the U.S. National Museum at Washington. The specimens have been very carefully worked out by Mr. Frederic W. True, Curator of the Department of Mammals of that institution, and we must now devote our attention to his excellent memoir on this subject (1) lately published.

Mr. True tells us that Dr. Abbott's specimens are "prepared with much care, the skins being almost invariably accompanied by the skulls, and furnished with labels giving the locality and date of capture, sex, and other particulars." Adding to Dr. Abbott's series the names of the species recognised by former observers, Mr. True finds that the mammalian fauna of Kilima-njaro and the surrounding district includes about seventy species.

The Quadrumana of Kilima-njaro, according to Mr. True's list, are of four species—three *Cercopitheci* (this being one of the most abundant and most characteristic groups of Æthiopian Monkeys), and one *Colobus*—namely, *C. caudatus*—the localised form of *C. guereza* already alluded to. To these will have to be added a Baboon (*Cynocephalus*), of which Mr. Johnston saw numerous examples, and concerning which, I think, he can hardly have been mistaken, although we do not yet know the exact species.

Of the Lemurs, the only species yet recognised from the mountain is *Galago crassi-caudatus*, which Dr. Abbott found "common" in the forests of Taveta; but the natives state that there are three other kinds of these animals in the same district. One of them may be *Galago garnetti*, which the Zoological Society has received living from the Zanzibar coast.

The Carnivora are more numerous in Kilima-njaro. The Lion, Leopard, Serval, and Cheetah are all attributed to this district by Sir John Willoughby and his brother sportsmen, and, no doubt, correctly. The Leopard is stated to be common on the mountain up to six or seven thousand feet in altitude, and to be very arboreal in its habits. The Viverridæ are also well represented. Dr. Abbott obtained examples of seven species, among which is the Ratel (*Mellivora capensis*), said to be "rare upon the mountains," and a Genet

Fig. 4.—Abbott's Antelope (*Cephalophus spadix*). [vol. xv., pl. lxviii.

Photo. U.S. Nat. Mus.

(referred by Mr. True to *Genetta pardina*) which varies excessively in colouration. Mr. Hunter included both the Striped and Spotted Hyenas as met with on the plains surrounding the mountain, the former being "very common everywhere." The Canidæ of Kilima-njaro are not yet accurately made out. Mr. True refers Dr. Abbott's specimens of Jackals to *Canis mesomelas* of the Cape; Mr. Johnston and Mr. Hunter associate this animal with *Canis lateralis* of the West Coast. Mr. Hunter also speaks of a "Wild Dog" that "hunts in packs" on the plains, and of another "small, dark-coloured fox." The latter, however, may be *Otocyon megalotis*, of which Dr. Abbott obtained three specimens.

Two species of *Hyrax* are met with on Kilima-njaro, one belonging to the tree-loving and the other to the rock-frequenting section of this isolated group. Mr. True refers the former to a new species, *Dendrohyrax validus*, and the latter to *Procavia brucei* (Gray). He has not had the advantage of consulting Mr. Thomas's recently-published revision (9) of this very difficult family, so that the latter determination may be doubtful.

Of Rodents, Mr. True registers fourteen species in his list, and the number will, no doubt, be seriously increased when fresh explorers in Kilima-njaro turn their attention to these little animals. Among the Muridæ, a new species of *Dendromys* "extends the range of this genus from South Africa to East Africa."

The list of Kilima-njaran Bats is also very short up to the present time. Mr. True only mentions four species. It cannot be doubted that these creatures are much more numerous, but they are difficult to capture, and do not excite the enthusiasm of the ordinary collector. The Insectivora are not abundant anywhere. In Kilima-njaro only two species have been yet recognised. These are a Hedgehog (*Erinaceus albiventris*), and a Shrew (*Crocidura*).

We now come to the most highly-developed and interesting portion of the mammal-fauna—that is the Antelopes, abundant all over Africa in suitable localities, and especially so on the high plateau near Kilima-njaro. Mr. True includes the names of twenty-one species in his list, of thirteen of which examples were obtained by Mr. Abbott, while eight others had been recorded by Mr. Hunter, as already mentioned. The list must, however, be slightly reduced, as Hunter's Antelope (*Bubalis hunteri*) is only met with in Southern Somali-land on the north bank of the Tana, a long way from Kilima-njaro, and should be omitted altogether. Again, as Mr. True allows it is not at all likely that two allied species of *Madoqua* occur there, Mr. True's *Neotragus kirki* and *N. damarensis* are probably identical. Moreover, as shown by Dr. Günther (*P.Z.S.*, 1890, p. 604, x.), the Reed-buck of this part of East Africa is *Cervicapra bohor*, not *C. arundinum*, and the so-called *Cephalophus nigrifrons* is, no doubt, Mr. Thomas's recently described *C. harveyi* (xi.). This would make the Antelopes of Kilima-njaria at present known

about nineteen in number — namely, according to my nomenclature: —

1. *Bubalis cokei.*
2. *Connochætes taurinus.*
3. *Cephalophus grimmi.*
4. ——— *harveyi.*
5. ——— *spadix.*
6. *Madoqua kirki.*
7. *Neotragus moschatus.*
8. ——— *tragulus.*
9. *Cobus ellipsiprymnus.*
10. *Cervicapra bohor.*
11. *Æpyceros melampus.*
12. *Gazella granti.*
13. ——— *thomsoni.*
14. *Lithocranius walleri.*
15. *Oryx callotis.*
16. *Tragelaphus rovaleyni.*
17. *Strepsicerus excelsus.*
18. ——— *imberbis.*
19. *Taurotragus oreas.*

It should be noted that the Brindled Gnu of Uganda and of this district seems to differ from the typical form of South Africa in having the throat-mane and jaw-tufts whitish or white, and has been made a sub-species by Mr. Thomas, as *Connochætes taurinus albojubatus* (xii.). There seems, however, to be no interruption in the range of this animal, which occurs, wherever the country is suitable, from the north of the Vaal up to Uganda. North of the Zambesi it has been recorded in Mozambique by Peters, in Nyassaland by Crawshay, and on the Kirgani River, opposite Zanzibar, by Speke. Mr. Hunter tells us it is found on the high plains N.E. of Kilima-njaro, but, unfortunately, calls it *Connochætes gnu*.

Besides the Antelopes, one of the forms of *Bos caffer* is abundant on the plains of Kilima-njaro, and, according to Dr. Abbott, as quoted by Mr. Hunter, ascends the mountain to an altitude of 10,000 feet. This completes the list of Bovidæ. The Giraffe was also (a short time ago) "very common round Taveta." Passing on to the Suidæ, both the Riverhog (*Potamochærus*) and Warthog (*Phacochærus*) are abundant in Kilima-njaria, and the Hippopotamus is found in Lake Jipe, and in "every large swamp." The Perissodactyle division of the Ungulates is represented by the Rhinoceros (*R. bicornis*) and one of the Zebras. I have always supposed this Zebra to be the northern form of Burchell's Zebra, with the legs striped outside (*Equus burchelli chapmanni*), but Herr Matschie has recently made a new name for it, *Equus boehmi*,[3] and he may possibly be justified in doing so. At the same time, there is great individual variation in the markings of Zebras, and it is very hazardous to base species on single skins and on sportsman's sketches.

The long list of Kilima-njaro Mammals is closed by the Elephant, which "lives in the thick damp forest, from 6,000 to 9,000 feet," in the dry season, descending to lower altitudes during the rains.

After this sketch of the Mammalian Life of Kilima-njaro, it will be evident that, so far as this part of its fauna is concerned, there are no traces of northern forms in Equatorial Africa, even at this excessive elevation above the sea-level. We might well have expected a Wild Goat to occur on the summit of Kilima-njaro, as it does on the mountains of Abyssinia.[4] Instead of a *Capra*, however, we find a

[3] *Sitz. Ges. nat. fr. Berlin*, 1892, p. 133. [4] *Capra walie*, Rüppell.

species of *Cephalophus*, a characteristic Æthiopian genus of Bovidæ. Nor, so far as we know at present, are there any traces of Marmots, Pikas, or Mountain-hares on Kilima-njaro. The places of these Boreal forms are taken by *Hyrax* and various species of Muridæ. In short, we find on Kilima-njaro merely more or less modified representatives of the inhabitants of the surrounding districts. So far as this piece of evidence goes, the wave of Boreal life, impelled by the Glacial Period, did not in Africa advance so far south as the Equator. What we *do* find in Kilima-njaria, however, is the most splendid series of the larger mammals to be met with in any spot on the earth's surface, such as may well justify Sir John Willoughby and his friends in terming their headquarters at Taveta the "Hunter's Paradise."

[Three of the illustrations prepared for this memoir have been kindly lent by the authorities of the U.S. Nat. Museum for the present article.]

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P. L. SCLATER.

II.

Christian Konrad Sprengel.

FEW men have been more in advance of the age in which they lived, or have suffered so long an undeserved oblivion, as the subject of this brief sketch. No mention of him is made in the Biographical Dictionaries, nor in the histories of Botany prior to that of Sachs (1875). Had not his discoveries been re-enunciated and extended by Charles Darwin, his name might even now be wanting in the roll of famous botanists. Very little is known about his earlier life. Born in 1750, he became Rector of Spandau, near Berlin. There, under Heim, he began the study of Botany, and became so enthusiastic over it that he neglected his duties as Rector, and was ejected from his post. He migrated to Berlin, where he supported himself by giving lessons in languages and in botany, and took up a lodging on the Hausvoigtei-Platz in a back room at the top of the house. "Here," says one of his old pupils (from 1809 to 1813) in an enthusiastic account of him in "Flora" of 1819, "I always found him, in an old bedgown, with a nightcap and a long pipe, the room filled with clouds of tobacco smoke. He sat generally at the window, with a book or his herbarium. Shelves of books, his collection of plants, and a few old household goods completed the contents of the room."

He goes on: "In figure Sprengel was well-built, rather large than small, lean, and raw-boned. His face was full of expression, the colour fresh, the glance vivacious. He wore his hair, which was prematurely grey, uncut, hanging about his shoulders. His gait was firm and upright; he walked fast, and, in spite of his age, went for half-a-day without rest. He was simple and frugal . . drank only water . . was never married."

Sprengel's only contribution to botanical literature is his now famous work, "Das Entdeckte Geheimniss," which was published in 1793, just one hundred years ago. This book—of which more hereafter—was too far in advance of its time, and met with a chill reception, which did not encourage its author. Besides being despised as a visionary by the dry systematists of the Linnæan school who then held sway in botany, Sprengel seems to have been too fond of speaking the truth, regardless of consequences, and thus became very unpopular,

and more and more forced into the seclusion in which we have already seen him. He was beloved, however, by his pupils.

On Sundays he usually conducted botanical excursions to the country round Berlin; any one might join these trips on payment of two or three groschen. On these occasions Sprengel was not merely a botanist, but would give instruction in anything that turned up. "He explained equally well the inscription on a tombstone, the construction of a windmill, the course of the stars, or the structure of a plant. . . . Knowing the country well, he led us always to places where rare or remarkable plants were to be found. There were few places where he had not himself found something new, or noticed something special, and he gladly took the opportunity of leading us thither. Thus he showed us at one place the divided sexes of *Mentha aquatica*,[1] which he had noticed there first, and afterwards observed in other species of Mentha. . . . In the Zoological Garden the Scrophularia gave him opportunity to explain dichogamy (see below). He was firmly convinced of the fertilisation of most flowers by insects, and he could, on this theory, so clearly explain the structure of flowers that it was a pleasure to watch and to listen to him. . . . The commonest plant became new by what he had to say about it; a hair, a spot, gave him opportunity for questions, ideas, investigations. Much remained a mystery to him; he was most exercised over the structure of *Parnassia*. Here he was unable to catch nature in the act."

Towards the end of his life he abandoned botany and devoted himself entirely to languages; he took up English, and was full of its great advantages. "He often said that Linnæus did not understand Greek, and had brought many errors into the nomenclature. He also blamed Willdenow for introducing the long and incorrect name *Pelargonium*, which should have been *Pelargium*." He died in Berlin in 1816 at the age of 66.

The chief interest of Sprengel's life centres in his famous book "Das Entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen" (The Secret of Nature discovered, in the structure and the fertilisation of flowers). This was published in 1793 as a quarto volume of 222 pages, with 25 large copper-plates, the latter being wonderfully accurate and well executed. In this volume Sprengel gives details of his observations on most of the wild flowers round Berlin, and many cultivated ones, all leading to the general conclusion that flowers are, mostly, adapted for fertilisation by insects. He shows how the insects are attracted, rewarded for coming by the honey, &c., and how almost every minutest structure in the flower, down to hairs and spots on the corolla, can be explained

[1] This plant is gynodiœcious, *i.e.*, bears upon some plants large hermaphrodite flowers, and on other plants smaller female flowers with aborted stamens. The same phenomenon is seen in many other plants of the order Labiatæ.

with reference to insects. The best way, perhaps, of treating the subject will be to let Sprengel speak for himself. His book begins as follows :—

"In the summer of 1787 I was carefully studying the flower of the wood crane's bill (*Geranium sylvaticum*) and I found that the lowest part of its petals was provided, on the inner side and the edges, with fine soft hairs. Convinced that the wise Author of Nature had not created a single hair without a definite end, I reflected as to what purpose these hairs might serve, and it soon struck me that if one supposed that the five honey drops, which are secreted by the same number of glands, were designed for the nutriment of certain insects, one would find it, at the same time, not improbable that care should be taken to keep the honey from being spoiled by rain, and that to attain this end these hairs were created. Every honey-drop sits on its gland immediately under the hairs, which occur on the edges of the neighbouring petals. Since the flower stands upright, and is pretty large, raindrops must fall into it during showers. None of the drops, however, can reach and mix with a honey-drop, being prevented by the hairs, just as drops of sweat are prevented from running down into the eye by the eyebrows and eyelashes. An insect, on the other hand, will not be in any way hindered from reaching the honey. After this I investigated other flowers, and found that several of them had something in their structure which seemed to serve the same end. The longer I pursued this research, the more I perceived that those flowers which contain honey are so arranged that, whilst insects can easily reach it, it cannot be injured by rain. I concluded, therefore, that the honey of these flowers, at least primarily, is secreted for the benefit of insects, and, that they may obtain it pure and uninjured, is protected from rain.

"In the following summer I investigated the forget-me-not (*Myosotis palustris*), and I found that not only does this flower contain honey, but that the honey is fully protected from rain. At the same time, however, I was struck by the yellow ring which surrounds the mouth of the corolla tube, and contrasts so beautifully with the azure blue of the limb. Is this circumstance also, thought I, to be referred to insects? Has Nature coloured this ring for the express purpose of showing insects the way to the honey receptacle? I examined, with regard to this hypothesis, other flowers, and found that the majority supported it. For I saw that those flowers whose corolla, as commonly happens, is coloured differently in one place than in the rest, always have these spots, figures, lines, or dots of special colour at the place where the entrance to the honey is found. Now I reasoned from the part to the whole. If, I thought, the corolla is coloured in special places on account of insects, its general colour must also be on their account; and if the special local colouring serves to direct to the honey an insect which has already alighted upon the flower, the general colour of the corolla must serve to attract

from afar, by showing the presence of honey, the insects that are swarming about in the air in search of food.

"Examining, in the summer of 1789, some species of *Iris*, I soon found that Linnæus had erred in regard both to the stigma and the nectary; that the honey is fully protected from rain; and, finally, that there is a specially coloured place, leading insects to the honey. But I found more than this, namely, that these flowers cannot be fertilised at all except by insects, and then only by insects of fairly large size. Although I did not, at the time, find this idea confirmed by experience (this occurred in the following summer, when I actually saw humble-bees creeping into the flower), still I was convinced of its truth by the general appearance. I endeavoured, therefore, to discover whether other flowers are also so constructed that they can only be fertilised by insect agency. My investigations convinced me more and more that many—perhaps all—flowers which contain honey are fertilised by insects which feed upon the honey, and that, consequently, this nourishment of the insects is, in regard to them, the final end, but, in regard to the flowers, is only a means, and, indeed, the sole means, to a definite end, viz., their fertilisation, and that the entire structure of such flowers can be explained, if, in their investigation, one keep always in view the following principles:—

"1. These flowers are fertilised by this or that species of insect, or by several.

"2. This also will occur, that the insects, as they visit the flowers for honey, and in consequence of this, either rest upon the flowers in some indefinite way, or in a definite manner either creep into the flowers or run round upon them, will, of necessity, be smeared with pollen from the anthers, either over the greater part of their hairy bodies, or only over a part of them, and will rub this pollen upon the stigma, which, for this end, is either covered with short and fine hairs, or with a damp, often sticky, secretion.

"In the spring of 1790 I perceived that *Orchis latifolia* and *O. Morio* have, in all respects, the structure of a honey flower, but do not contain honey. I thought at first that this observation, if it did not actually overthrow my former discoveries, would at least make them very doubtful. For, since these flowers have a honey guide (so have I termed the differently coloured spot on the corolla)—and yet this cannot show insects the road to the honey, as there is none[2]—it appeared to follow that the honey guides also, on those flowers which do contain honey, were not really for this purpose at all, and that the whole thing was a mere fancy. I must, therefore, confess that this discovery was in no way pleasant to me. But just this spurred me on to study these flowers more attentively, and to observe them in the field, and at last I discovered that these flowers are fertilised by

[2] Darwin showed that insects visiting Orchis drill holes into the tissue of the spur and suck the juice therein contained, so that the flower is not really an exception to Sprengel's views.

certain flies which, deceived by the appearance, imagine the spur to contain honey, and creeping in, tear the pollen masses from their chambers and bring them upon the sticky stigma. Flowers of this type, which have fully the appearance of honey flowers, but do not contain honey, I call 'sham honey-flowers' (Scheinsaftblumen). That there are more of them, I saw in the same year on the common birthwort (*Aristolochia Clematitis*). I found, namely, that this flower also, which contains no honey, is formed in every way like a honey flower, and, on this account, small flies of all kinds creep into it. In the following summer I saw clearly that this flower is a true wonder of nature, namely, that these flies are led by the appearance of the flower to creep into it, that they may fertilise it, and that they are held prisoners in it until they have done so, but that as soon as this has occurred they are released from detention.

"In the summer of the year above mentioned I discovered, in *Epilobium angustifolium*, something upon which I had never happened before, namely, that this hermaphrodite flower is fertilised by humble and hive bees, not, however, each flower by its own pollen, but the older flowers by means of pollen carried by insects from the younger.[3] This observation shed great light upon many of my earlier discoveries. I was especially pleased when I found a similar method of fertilisation in the wild love-in-a-mist (*Nigella arvensis*). In the summer of 1788 I had perceived the beautiful arrangements of the nectaries of this flower. In the following summer observation showed me that it was fertilised by bees. At the time I thought I fully understood how this came about. Now, however, I found that I had erred in regard to the last point, because then I still believed all hermaphrodite flowers to be fertilised by their own pollen.

"Finally, last summer I studied the common spurge (*Euphorbia Cyparissias*) and found in it an arrangement the exact opposite of that described above, the flower being fertilised by insects, but in such a way that they carry the pollen of older flowers to the stigmas of younger ones.

"Upon these six chief discoveries, made in five years, is founded my 'Theory of Flowers.'"

After a discussion of the subject of nectar and its uses, in which he demolishes the older views, such as that the nectar causes the growth of the ovule to a seed by keeping it damp, or that it is an injurious substance, whose removal by insects is therefore a direct benefit to the plant, Sprengel goes on to point out how in all honey flowers the following five points may be observed, viz.:—(1) The honey gland or *nectary*; (2) the *honey receptacle*, in which is stored the honey secreted by the gland; (3) some arrangement to protect the

[3] The flower is *dichogamous*, *i.e.*, the stamens and pistil do not ripen simultaneously. In young flowers the stamens are ripe, the style folded back out of the way; in older ones the style occupies, with its ripe stigmas, the place formerly held by the now empty and withered stamens. This phenomenon is termed protandrous dichogamy or protandry; the reverse case (*Euphorbia*) protogyny.

honey from the rain; (4) arrangements to enable insects easily to find the honey, such as size and colour of the corolla, scent, and the honey-guides or "path-finders" formed by the differently-coloured spots near the entrance to the honey; (5) the general impossibility of spontaneous self-fertilisation, owing largely to dichogamy or other arrangements, and the necessity of insect aid.

The book contains an enormous mass of the most painstaking observations on numerous flowers, undertaken with these ideas in view. There is, however, one very serious blemish in it, which, perhaps, as much as anything, caused its rejection by botanists. Sprengel was most careful to find reasons for everything in the structure of the flower, and yet he did not try to give a reason for flowers being adapted to fertilisation by insects. Why should a flower go to the trouble and expense of attracting insects merely to effect fertilisation, which it might do for itself very easily? Sprengel observed that by dichogamy and other arrangements it constantly occurred that a flower was fertilised, not with its own pollen, but with pollen from a different flower. He even went so far as to say, "Since very many flowers are unisexual, and apparently at least as many of the hermaphrodites are dichogamous, Nature seems to have intended that no flower should be fertilised by its own pollen," and yet he did not suspect that this was the whole point of the fertilisation by insect agency. Darwin and others have shown the great benefits arising from cross fertilisation, and have thus explained why plants should have become adapted to fertilisation by insects in the various ways described by Sprengel. Had Sprengel been aware of this point, and incorporated it in his theory of flowers, it seems unlikely that his work could have fallen into the oblivion which soon overtook it. Even so late as 1850, the work could be bought for 1s. 6d. (it is now quoted by Friedländer at 22s.). In 1841 it came into the hands of Charles Darwin. "The book impressed him as being 'full of truth,' although with 'some little nonsense.' It not only encouraged him in kindred speculation, but guided him in his work, for in 1844 he speaks of verifying Sprengel's observations" (Life, by Francis Darwin). He rehabilitated Sprengel by his biological work, especially the "Origin of Species," the "Cross and Self-Fertilisation of Plants," where he recounted the results of a long series of experiments showing the benefits of cross fertilisation, and, lastly, by his brilliant work on "Fertilisation of Orchids," a book of observations on flowers, conducted much on Sprengel's lines, but with the flaws of his theory removed. Since this time, the whole subject of fertilisation, by insects and otherwise, has received much attention at the hands especially of Hildebrand, Axell, Delpino, Hermann and Fritz Müller, and recently Macleod and Robertson. To the writings of these authors, reference must be made for further information, and we must here take our leave also of Sprengel, a man deserving of a high place in the History of Botany, but most unjustly forgotten for nearly seventy years after the publication of his classic book.

John C. Willis.

III.

The Recapitulation Theory in Palæontology.

NATURAL SCIENCE is to be congratulated on the publication of an article so opposed to current belief as that of Mr. C. Herbert Hurst on "The Recapitulation Theory," for it has thereby shown that it will not burke views simply because they are unfashionable, but rather that it is ready to afford a free field to all genuine knights-errant who dare to smite the shield of authority. Whether the heterodox opinions prove ultimately right or wrong, their publication is of service as forcing us to consider more carefully than we are apt to do the reasons for the faith that is in us. No doubt there will be many to answer Mr. Hurst's challenge, some, perhaps, to support him; out of the *mêlée*, truth is most likely to arise if each confines himself to facts within his own knowledge; here are a few such.

The heaviest blows of Mr. Hurst fall on the embryologists, or, to speak more accurately, on those who study the embryology of living beings, "without the labour involved in palæontological research." Such an attack is undoubtedly deserved in many cases; but Mr. Hurst himself would have strengthened his position had he been able to bring forward any arguments from the actual history of extinct beings that should upset the conclusions of the neontologists, or that should definitely disprove the dictum—"Ontogeny repeats Phylogeny." This he has not done, and this I do not intend to do for him. In the first place, though it would be easy to show that the genealogies constructed by neontologists were contradictory both of one another and of the facts of palæontology,[1] this would merely prove that somebody had made mistakes, an argument admirably adapted for the daily Press, but not for a scientific journal. In the second place, the very limited amount of accurate palæontological knowledge that I possess does not enable me to produce any facts opposed to the theory of Recapitulation as understood by most modern biologists.

First, let us consider the case of *Antedon*, which Mr. Hurst dismisses so scornfully. The possession of a stem is, we may admit, an advantage to the larva, and Mr. Hurst's contention that the larval

[1] See a recent article by A. Smith Woodward on "The Forerunners of the Backboned Animals," NATURAL SCIENCE, vol. i., p. 596.

stem of *Antedon* is the equivalent of the *larval* stem of *Antedon's* ancestors, would be at least a possible explanation, were it not for one fact: the stem-ossicles of the *Antedon* larva are not at all of a simple type, they have a very peculiar specialised structure, and, broadly speaking, resemble the stem-ossicles of the Bourgueticrinidæ so common in the Upper Chalk. It is a mistake to call this larva "pentacrinoid"; if any genus is known to us with which it may be compared, that genus is *Thiollericrinus*, of the Jurassic rocks, many species of which show a gradual loss of the stem and development of cirri around the base of the cup. What the ancestors of *Thiollericrinus* may have been we cannot say with certainty; this only seems clear, that the structure of the stem is specialised. A somewhat similar structure was developed in the Carboniferous Platycrinidæ, from which, however, it is not likely that *Thiollericrinus* was derived. Such stems do not occur lower than the Carboniferous, and we must infer that the ancestors of both Platycrinidæ and Bourgueticrinidæ had stems of the primitive Palæozoic type, with round ossicles radiately ridged. From such ossicles all other types may easily have been derived. Now, to accord with Mr. Hurst's explanation of the *Antedon* stem, it must be supposed, either that the larval stem of all crinoids has always been of this specialised type, which is contrary to all available evidence, or that its structure is a special larval development. Surely it is hardly probable that an organ which is only in use for a few days of the creature's life should have become specialised in just the same way as the stem of the adult ancestors. It is certainly easier for the ordinary mind to imagine that, by acceleration of development (a principle ignored by Mr. Hurst), the structure of the adult ancestor has been pushed back to the larval stages of the existing *Antedon*.

But the stem is by no means the only structure in the larval *Antedon* that bears on the Recapitulation Theory. "Each transient stage in the development of an individual," says Mr. Hurst, "is a modification of the corresponding stage of development of its ancestors." Let us see how this is borne out by the development of the cup-plates.

The most important peculiarity of the larva as distinguished from the adult *Antedon* is the presence of a plate in the anal interradius. At an early period this plate appears between two of the radials, and on the same level with them; subsequently it is lifted out from between the radials, which close in under it as it passes upwards, so that at last the radial circlet is quite continuous, and the anal plate lies above it. At about this stage the crown is separated from the stem; shortly afterwards, the orals disappear, and a little later, the anal plate is itself absorbed and vanishes. Now, on Mr. Hurst's theory, each of these stages should be represented in the early development of the ancestors of *Antedon*; but this would raise far greater difficulties, for the oldest crinoids do not possess this anal plate at all, at all

events not in the position that it first occupies in *Antedon*. It appears from palæontological evidence that this plate first appeared above the level of the radials, that it gradually sank down between the two posterior radials, and that at a far later period, towards the close of the Palæozoic, it gradually passed upwards again, precisely as it does in the young *Antedon*, and eventually disappeared. The ontogenetic stages of the anal plate in *Antedon* are represented in the phylogenetic series by *Ceriocrinus*, *Erisocrinus* and *Stemmatocrinus*. Mr. Hurst is bound to suppose that such forms as *Merocrinus* and *Dendrocrinus* started with an anal plate in a line with the radials, a supposition contrary to all available evidence, or that the anal plate in *Antedon*, together with the remarkable changes that it passes through, is a special larval development. "Mystical" it may be, to suppose that we have here an epitome of the ancestral history, but Mr. Hurst can hardly say that it is not justified by evidence.

There are many other curious parallelisms between the growth of *Antedon* and the history of the earlier crinoids. It is, however, possible to explain them on Mr. Hurst's plan, so I merely allude to them here to show that they do not in any way run counter to the Recapitulation Theory. They are the large size of the basals, the peculiar shape of the radials, the well-developed orals, and the evolution of pinnules.

To turn to another passage and to a different group of animals. At the bottom of page 197 Mr. Hurst says: "In order to produce a 'record,' it is necessary that new chapters be added at the end of the pre-existing record. It is necessary that, as the adult structure varies in one direction, the late stages of development shall vary in another, so as to become, not more like the new adult structure than they were before, but more like the old one." This is hardly a fair statement of the case. It is not correct to say that the late stages of development must vary in *another* direction; for it surely is the case, in any series of parents and offspring, which are varying in a given direction, and which we may denote A^1, A^2, A^3, A^n, that A^6 is nearer to A^7 than A^5 is. Consequently, if the latest stage of development of the form A^7 resembles A^6, it is necessarily more like A^7 than a stage which resembled A^5. That is to say, on the Recapitulation Theory, the stages of development vary in the *same* direction as the adults. Mr. Hurst's statement of the method of variation, demanded by the Recapitulation Theory, should, therefore, be amended as follows:—Variation, or change from parent to offspring, takes place by the addition of features at the end of the ontogeny; and these features are, by subsequent successive additions, gradually pushed back to earlier stages of ontogeny, so that what is the ultimate stage of one form is the penultimate of the next, and the ante-penultimate of the next after that.

Although the adherents of the Recapitulation Theory will doubtless accept the above as, in the main, a correct statement of the

method of development, yet not the most sanguine of them will hope to find so perfect an epitome of phylogeny in the majority of cases. The necessary compression, the constant elimination of unnecessary stages, the modifications required by larval conditions—all these things make it truly remarkable that we should get as much recapitulation as we do. Yes, we do get it, and this is "the way in which it does actually occur," let Mr. Hurst deny it as much as he pleases. In fact, no more perfect example of this impossible and non-existent method of development could be imagined than that which is actually afforded us by the palæontological history of the Ammonites. Does Mr. Hurst dispute the reality of the facts made known to us by Würtenberger, Waagen, Branco, Hyatt, Buckman, and many others? Not everyone will accept the alterations in classification proposed by these workers, but no one has hitherto been found to deny their facts. The reason why no one has done so is obvious: anyone can verify them for himself. It has been shown over and over again, by workers approaching the subject quite independently and from different points of view, that the adolescent stage of any species of Ammonite resembles the adult stage of its immediate ancestor, and that the larval stage resembles the adult of a previous ancestor; while, on the other hand, the senile stages of the same species foreshadow the features of the adult offspring. These facts have not merely been worked out by students in museums, by dissection of well-preserved individuals, and comparison of large quantities of specimens, but have been confirmed by minute labours in the rocks themselves, and by the careful tracing of the species from zone to zone over large tracts of country.

A suitable example is presented by the descent of *Coroniceras trigonatum* from a smooth ancestor, as indicated in Hyatt's "Genesis of the Arietidæ," Summary-plate xii. Hyatt here gives the ancestry as follows:—*C. trigonatum*, *C. gmuendense*, *C. lyra*, *C. rotiforme*, *C. sauzeanum*, *C. kridion*, *Arnioceras kridioides*, *A. semicostatum*, and *Psiloceras planorbe*, var. *leve*. To obtain an independent opinion, I applied to my friend, Mr. S. S. Buckman, who informs me that he agrees with the first six names except as to the interposition of *C. sauzeanum*. He, however, "would not like to say that the species of *Arnioceras* are the actual ancestors; but they are the morphological equivalents undoubtedly of those ancestors." There is also considerable objection to taking a retrogressive type like *Psiloceras planorbe* as the starting point of a new series. At the same time, even those who refuse to regard this particular species in the same light as Professor Hyatt, will admit that the ancestor must have been a somewhat similar smooth, keelless, and round-sectioned form; in fact, Buckman favours *Arnioceras miserabile*, which Hyatt himself gives as an ancestral form. Accepting, then, this line of descent, we may draw up the accompanying table, which traces the development of three main characters in both ontogeny and phylogeny. The first column describes the infantine stage, the second the adolescent, and the

third the adult of each species. In none of these is the embryonic stage given, since it varies very little, being of the same smooth character throughout. It becomes, therefore, interesting to learn how Mr. Hurst explains the tuberculate infantine stage of *C. trigonatum*, which is certainly considerably less like its own adult than is the corresponding stage of *C. lyra*; it cannot possibly be explained as an attempt of the embryo to become like the adult. Indeed the whole table, the facts of which are given in full detail in Hyatt's monograph, and the main principle of which is maintained by two independent workers, is totally opposed to all Mr. Hurst's contentions. It should also be remembered that there are many details of structure, too complicated to be dwelt on here, but all confirming the other evidence.

	Infantine.	Adolescent.	Adult.
Coroniceras trigonatum	Well-marked ribs and slight tubercles. Quadrate section. Keel?	Broad ribs. Section unknown. Slight keel.	Nearly smooth. Subtriangular section. Very slight keel.
C. gmyendense ...	Well-marked ribs and slight tubercles. Subquadrate section. Slight keel.	Clear ribs and slight tubercles. Compressed quadrate. Keel.	Slight ribs. Compressed quadrate. Pronounced keel.
C. lyra	Ribs and slight tubercles. Broad subquadrate section. Slight keel.	Ribs and tubercles. Subquadrate section. Slight keel.	Tubercles. Slightly compressed subquadrate section. Keel.
C. rotiforme	Smooth, and then slight ribs. Roundish section. Very slight keel.	Ribs and tubercles. Broad subquadrate. Slight keel.	Pronounced tubercles. Subquadrate section. Keel.
C. kridion...	Almost smooth. Broad rounded section. No keel.	Slight ribs. Broad subquadrate section. Slight keel.	Tubercles. Subquadrate section. Keel.
Arnioceras semicostatum	Smooth. Roundish section. No keel.	Smooth and slight folds. Elliptical section. Trace of keel.	Slight ribs. Slightly broader section. Slight keel.
A. miserabile.	Smooth. Round section. No keel.	Smooth. Slightly compressed round section. No keel.	Smooth. Elliptical section. Slight keel.

Professor Huxley once said that if Evolution had not already been an accepted theory, the palæontologists would have had to have invented it. It might, with no less truth, be asserted that, if the embryologists had not forestalled them, the palæontologists would have had to invent the theory of Recapitulation. As a matter of fact, many of them seem actually to have arrived at it quite independently either of one another or of the neontologists, a circumstance that does not necessarily prove the truth of the theory, but that should, at least, have led Mr. Hurst to pay some attention to their opinions. With evidence such as has just been quoted before them, it would, indeed, have been difficult to have arrived at other conclusions; at the same time, it must not be supposed that all lines of descent, even among Ammonites, show the facts with equal clearness. There are, as has already been hinted, many modifying forces at work, and the chief of these may be conveniently included under the one head of economy. To this is due the disappearance from the record of those features that are not adapted to the needs of the animal in the early

stages of its life, and of those that involve unnecessary expenditure, such as a spinous stage in an Ammonite intervening between two costate stages. These explanations of the falsification of the epitome are barely alluded to by Mr. Hurst; for since, on *à priori* grounds, he denies any epitome whatsoever, he naturally does not stay to enquire whether he may not have sometimes been misled by these obscurations of it. So far, then, as Mr. Hurst is concerned, it is idle to discuss these matters, we must rather seek for flaws in his original argument.

His main fallacy appears to me to be simply this, that he substitutes contemporary relations for ancestors. After stating Von Baer's law, which obviously refers to existing species, co-existing, that is, and therefore not derived the one from the other, he proceeds to say:—"If similar comparisons could be instituted between an ancestral species and its much modified descendants, there is no reason for doubting that a similar result would be reached." But there is all the difference in the world between filial and fraternal relationship, and though Von Baer's law is undoubtedly true of the latter, there are many objections to supposing that it is equally true of the former. The only evidence that Mr. Hurst condescends to offer is the quotation from Darwin, which is, indeed, one part in favour of himself, but the other six parts in favour of his opponents.

Mr. Hurst seems to suppose that on the Recapitulation Theory a bird should begin life as a fish, then change to a lizard or other reptile, and finally burst into a bird. Such a harlequinade has never been imagined. The bird of to-day must be compared, not with the reptile of to-day, but with the bird of the past, and that in its turn with a form which may have had very few of the characters of reptiles as we now know them; as for fish, it is as likely as not that they barely entered into the phylogeny at all. Those who keep a more observant eye on the progress of vertebrate palæontology than I have leisure to do must surely have observed how day by day the ancestral stocks are pushed further and further back, so that the connecting link between, or common ancestor of, the great divisions cannot possibly be dragged into the discussion.

To glance at another example of Mr. Hurst's—the three gnats *Culex*, *Corethra*, and *Chironomus*, forms which, though alike in the adult, are dissimilar in the larval stage. Of course, it is possible that these genera may really be descended from a common ancestor, and that variation has chiefly affected the early stages. If so, it is clearly impossible that those early stages can reveal to us the past history of the genera in question. Such cases as this are admitted by everyone; they are consistent with the Recapitulation Theory, and as Mr. Hurst makes no further capital out of them, it is hard to see why he introduced them at all. On the other hand, instances are known, as in the Ammonites *Dumortieria* and *Grammoceras*, of adults which resemble one another so closely that they would actually be taken for

the same species, did not investigation of their ontogeny reveal stages resembling the adults of pre-existing species belonging to the two different genera. In these cases, it is the earliest stages that differ most and afford the best proof not only as to their affinities with contemporary species, but as to their perished ancestry. It would be rash for one who is no entomologist to hint that the resemblance of the adult *Culex*, *Corethra*, and *Chironomus* is a similar case of convergence; but it is clear in the case of *Dumortieria* and *Grammoceras* that "variation" apparently "affecting chiefly the structure of the individual in early stages of development" has actually preserved to us a record of the ontogeny.

Another mistake of Mr. Hurst, is his refusal to admit more than one method of variation. He appears to deny that variation occurs at the close of ontogeny; if an adult structure varies, he says in effect, it ceases to exist as an adult structure at all. It does eventually do so, no doubt, but not immediately. The very same "change in the constitution of successive generations of a species leading to the production of a new species" does take place in the life-history of a single generation. Ontogeny, in fact, forecasts phylogeny just as much as it repeats it. An Ammonite that is smooth in the adult, will become ribbed in old age; this change takes place at earlier and earlier periods in successive generations, till at last a form is attained that has ribs in the adult. Then we call it a new species. But this new species is spinous in its old age; gradually do the spines, in like manner, appear sooner and sooner, till another species, that bears spines in its maturity, is found in the rocks. A similar case is the tendency of modern civilised man to adopt baldness as an adult character, although one that has hitherto been regarded as purely senile. Yet all these well-known instances of variation (and I use the term precisely in Mr. Hurst's own sense) "occur in a way utterly unlike the way in which it does actually occur," *i.e.*, within Mr. Hurst's horizon. Of course, this is not the only method of variation; there are sports of differing degrees, and there is variation produced by sexual mixture of different strains. If these alone were to be considered, we might, of course, be led to different conclusions, that is, if we were persistently to shut our eyes to the plain facts of palæontology.

The fact that absurdities and contradictions have arisen from too zealous following of the Recapitulation Theory does not actually prove the conception false; nor, on the other hand, does the fact that under its guidance we have made remarkable discoveries and initiated fruitful investigations necessarily prove its truth; at the same time, it might possibly lead Mr. Hurst to reconsider his condemnation if he were to make himself acquainted with the brilliant researches of Hyatt and Buckman on Cephalopoda, R. T. Jackson on Pelecypoda, and Beecher on Brachiopoda, not to mention many writings in other tongues than our own, all which works have been inspired by the Recapitulation Theory.

F. A. Bather.

IV.

Ornithology in Relation to Agriculture and Horticulture.[1]

THE subject of agricultural and horticultural economics, considered in the relations of birds and insects to the produce of the soil, is one which, till recent years, has been singularly neglected in this country. In France, Belgium, and especially in America, as well as in other civilised countries, the most careful and practical investigations, under the assistance of the State, have led to the accumulation and diffusion of much valuable information on this and kindred subjects. In England, the researches of Miss Eleanor A. Ormerod (late consulting Entomologist to the Royal Agricultural Society of England) are recognised as of great value in enabling the farmer and gardener to detect the various insect pests which take tithe of his crops, and in teaching him how best to apply suitable remedies for their prevention or extirpation.

In the present volume the editor, Mr. John Watson, has brought together a series of useful papers and notes by various naturalists, whose names are a sufficient guarantee of the high practical value of the opinions expressed, in connection with ornithology in its bearings on agriculture and horticulture. On taking up the book we are somewhat disappointed to find that it contains neither preface nor introduction stating under what circumstances the various papers were written, we can only conclude, therefore, that these have already appeared either as newspaper articles or in some serial. The book is divided into twelve chapters, under the various headings of Hawks and Falcons, Owls, Wood-Pigeon, Rook, Starling, Miscellaneous Small Birds, Game Birds, and an appendix, with notes and additions. No less than five chapters are devoted to the sparrow, those "rats of the air," "ruffians in feathers," whose mischievous and destructive character are recognised and acknowledged by farmers and gardeners over half the world.

The larger birds of prey, buzzards, kites, goshawks, harriers, and the noble peregrine are now virtually extirpated in England, and

[1] ORNITHOLOGY IN RELATION TO AGRICULTURE AND HORTICULTURE. By various Writers. Edited by John Watson, F.L.S., &c. London: W. H. Allen & Co., 1893.

the smaller hawks and falcons, and also owls, are far less numerous than formerly. There are at present large districts in England where you may wander all day without seeing a single bird of prey. This has been brought about by the excessive rage for game-preserving. As a class, gamekeepers, considering the great opportunities at their disposal, are proverbially ignorant of Natural History, and seem quite incapable or unwilling to discriminate, even from their own narrow standpoint, between the good and the bad. The natural result, therefore, of so much misplaced zeal has been an enormous increase in wood-pigeons, sparrows, rats, and mice, which, now that their natural enemies, the birds of prey and weasels, have been destroyed, flourish and multiply unchecked, and yearly destroy great quantities of valuable cereals and other farm and garden produce.

So much has already been written in defence of the farmers' best friends, the owls, that any further allusion to the subject would only be hackneyed and out of place. We will only, therefore, mention one fresh fact in proof of the value of the owl as a vermin-destroyer. In the vole-plague districts of the south of Scotland, which includes a wide area of hill-pasture in Teviot and Hawick, Ettrick, Eskdalemuir, Yarrow, and Moffat, in 1892, 301 nests of the short-eared owl (*Asio accipitrinus*) were actually found on those farms from which specific information was obtained, and it is calculated that this number may be reasonably doubled to include those not seen.

The result is 602 nests, with, say, seven young to each nest, equal to 4,214 young birds on these farms. These owls have undoubtedly been attracted to the district from great distances by the enormous supply of their natural food, and are induced to remain and nest there, and the services rendered by them to the sheep farmers cannot well be estimated. To give three instances alone, twenty-nine dead voles were taken from the side of one nest, and the next day twenty-seven from the same place. In another case, a shepherd counted thirty-seven at a nest containing ten eggs. For some years past the short-eared owls are known to have nested in small numbers in the infected area, but in 1892, owing to the abundance of food, they have mustered, remained, and bred in extraordinary numbers, with the result that there has been a marked diminution of the voles over much of the districts named. The mouse-eating kestrel has also greatly increased since the commencement of the plague, and as many as thirty have been seen at one time. The whole question has been most ably treated by Mr. Peter Adair in a paper which appeared in "The Annals of Scottish Natural History" for October, 1892.

In the five chapters relating to the sparrow, the evidence for the prosecution greatly outweighs that for the defence. It is clearly shown that the depredations of this pest on fruit-tree buds, to fruit farmers, florists, young crops of vegetables, and more especially to corn in autumn, is enormous, and far in excess of any benefits con-

ferred by the consumption of injurious seeds and noxious insects. They entail also direct harmful consequences by their pugnacious and self-assertive nature in driving off useful insectivorous birds from the neighbourhood of their haunts. Yet it is by no means clearly proved that an utter and complete extermination of the sparrow-nuisance would be a benefit, for when man upsets the balance of nature, he very often has to pay for it in some form or other.

The sparrow certainly requires no Act of Parliament to protect him, and the plea of sentimentalists and humanitarians that he should be allowed to increase and multiply unchecked, will certainly never be listened to by those country folk who are best able to form a judgment in the matter.

There can be no doubt that, during the last half-century, the woodpigeon or ringdove (*Columba palumbus*) has increased to an enormous extent. The causes of this increase are, doubtless, the killing off of the falcons and hawks, which are the natural enemies of the race, the increase of woods and plantations, especially those of fir, and the abundance of winter food in turnips and other green crops. It is quite certain, too, that in the autumn the ranks of our local birds are greatly increased by immigrants from the Continent. In the autumn, woodpigeons congregate and attack the ripening corn, particularly in those spots where it is storm-laid, devouring great quantities, and crushing and trampling the heads to near the ground, so that in a wet season much becomes hopelessly sprouted. In winter they commit serious ravages in the turnip crops by eating the leaves, thus exposing the bulb to frost. They are also very partial to the young clover plant. The ringdove, however, has redeeming points, its plaintive coo, roo, coo, coo is a pleasant sound at early morning in spring woodlands, and in the winter it is a real sporting bird, and excellent eating.

The injury done by rooks has often been much exaggerated by farmers and others. If we put aside those periods of the year when it levies contributions on the newly-sown corn, especially when badly covered, the time when the corn is ripening, injury done to stacks and swede turnips in severe weather—we have pretty well enumerated all the charges brought against him. All the rest of the year he is ridding the pastures of injurious grubs, such as the larvæ of the cock-chafer (*Melolantha*), and of the cranefly (*Tipula*). In recent years, rooks, in those districts where they are too many, or short of food in a drought, have been accused of developing egg-stealing propensities, and harrying the nests of game birds and wild fowl, and killing the young, and we are afraid he is not altogether guiltless in this respect.

The starling, considered from an agricultural point, is the greatest possible friend both of farmer and gardener, its food during the whole of the year consisting of grubs, small molluscs, worms, and insects, and only very occasionally fruit and berries. In the autumn immense flights of migrating starlings come to us from the Continent;

these are an Eastern race, distinct from our common bird, and have purple heads and green ear-coverts, and they leave the country in the spring.

We are somewhat doubtful as to what Mr. Riley Fortune tells us in chap. x., that the starling picks the "ticks" from the wool on the backs of sheep. We rather think he uses the sheep's back as a convenient perch. At various times we have watched them through a glass at a short distance, when perched on the backs of thick-wooled Lincoln sheep, but have never seen them actually picking off the sheep fags.

Much more might well be written on this most interesting topic, the subject is practically an inexhaustible one. Out of the multitude of our miscellaneous small birds, the greater part are absolutely innocuous and are largely beneficial. In other cases, the injury done at certain seasons is slight and amply counterbalanced by the services they render during the rest of the year in destroying insects and the seeds of weeds.

In conclusion, to sum up the evidence both for and against, as placed before us by the able ornithologists in Mr. Watson's book, it is abundantly apparent that the case for the prosecution falls very far short of the defence, and that the verdict must be an acquittal for the birds, both as regarding individual species and in the aggregate, with an admission that the benefit they confer upon man is far in excess of the injury. There is one exception to this, and that is the ubiquitous and all-devouring sparrow.

"O wretched set of sparrows, one and all,"—

Perhaps no greater mischief is done than by that large class of sentimental writers who are ever ready to exaggerate the good qualities of their feathered favourites and to minimise the evil. It must, however, be apparent to the dullest intellect that no wild bird is able to draw a line between the natural production of the soil and those seeds and fruit which are the results of man's industry. Neither can it be expected that hawk or falcon can discriminate between the young of a wild bird and a coop-reared pheasant or partridge. In so highly a cultivated country as England, birds would often be put to great straits if they had to depend on wild fruits or seeds alone, and, having dispossessed them of their inheritance, we must be satisfied to give our little workers some small share of our produce as a return for the important services rendered in keeping down weeds and insects, and thus indirectly helping to increase the fertility and productiveness of the soil.

JOHN CORDEAUX.

V.

A Fish-Eating Rodent.

A VERY interesting new Mammal has recently been received at the British Museum, in the form of a Fish-eating Rat from the mountain streams of Central Peru. The animal is of about the size of a common house-rat, but has a flattened head, strong and numerous whisker-bristles, and very small eyes and ears, characters which give it a striking resemblance in its physiognomy to some of the aquatic genera of the Insectivora and Carnivora, such as *Potamogale*, *Myogale*, *Lutra*, or *Cynogale*. Its swimming powers are evidently very great, as is shown, among other things, by its broad, webbed and strongly-ciliated hind feet, far better developed for this purpose than are those of the ordinary swimming Muridæ, such as our English Water-vole, whose simple vegetarian diet does not necessitate the development of any exceptional swimming powers. In colour, like the common Water Shrew, it has a dark upper side with a whitish belly, and has a markedly bicolor black and white tail.

The chief interest of the new form centres in the fact of its being wholly a fish-eater, and in its having in connection therewith its incisor teeth modified for catching a slippery, active prey by the development of their outer corners into long sharp points, and its intestines altered by the reduction almost to *nil* of its cœcum, an organ in vegetarian Muridæ always of great size and capacity. The stomach of the single specimen obtained contains fish-scales, recognised by Mr. Boulenger as those of *Tetragonopterus alosa*, a fish whose average length is about six inches.

This animal represents quite a new departure in Rodent life-history, for although it is now perfectly well known that the North American Musquash (*Fiber zibethicus*) occasionally feeds on fish caught by itself, yet there is no other Rodent which, as in the case of *Ichthyomys stolzmanni*, as it is proposed to term the new form, wholly lives on fish, to the exclusion of a vegetable diet.

The general relationships of *Ichthyomys* are clearly with the ordinary South American Muridæ, perhaps more especially with those of the *Habrothrix* group, and there is certainly no direct connection with *Fiber*.

The type and only known specimen of this interesting form was obtained by the Polish collector, Mr. J. Kalinowski, at Chanchamayo, Central Peru, in the course of 1891.

OLDFIELD THOMAS.

VI.

Colour Changes in Insects.

THE question of the variation in colour and markings exhibited by many species of lepidoptera, in both their preparatory and perfect stages, has, during the last few years, received much attention, and given rise to not a little controversy. A general protective resemblance of insects to their surroundings has long been an accepted fact with most naturalists, and no one would deny that the special and often highly perfect likeness of caterpillars to twigs and imagines to leaves, is of value as a protection; but on the meaning of the variation in colour of a species with reference to its surroundings, less certainty has existed. This most interesting subject was brought into prominent and public notice in 1890, by Poulton, in his well-known book, "The Colours of Animals" (pp. 110–158). Some doubt has lately been expressed as to certain of his deductions. His recent publication (1) of the details of several years' experiments, of which only some of the leading results were given in "The Colours of Animals," is, therefore, of value, and a short review of the questions raised may be of interest.

The only larvæ of Noctuid moths on which Poulton experimented were those of *Hadena oleracea*, *Euplexia lucipara*, two species of *Mamestra* (*M. brassicæ* and *M. persicariæ*), and four of *Catocala* (*C. sponsa*, *C. nupta*, *C. fraxini*, and *C. elocata*). The *Mamestræ* showed no power of colour-adaptation, some green caterpillars of *M. persicariæ* turning brown on green leaves. Most noctuid larvæ feed by night, hiding in earth by day, and colour is consequently of minor importance to them. The *Catocala* larvæ, which in their form and habits approach geometers, were rather sensitive to environment, tending to become darker when black twigs were mixed with their food, than when among green leaves and shoots only. Poulton's results with these two genera are confirmed by Miss Gould (2). No certain results were obtained with the larvæ of *Hadena* and *Euplexia*.

The only geometrid caterpillar which was not proved sensitive to its environment was that of *Ephyra annulata* (*omicronaria*). All others on which experiments were made were darkened by placing black twigs among their food. Such were the larvæ of *Ennomos quercinaria* (*angularia*), *Selenia lunaria*, *Melanippe montanata*, *Phigalia pedaria* (*pilosaria*), *Hemerophylla abruptaria*, and *Crocallis elinguaria*. All these were light brown among green leaves and shoots. The last-named caterpillars were

placed among green leaves in darkness, and were darkened thereby, though not nearly so strongly as by dark surroundings in daylight. The larvæ of *Geometra papilionaria*, which hibernate, were sensitive before the winter, when various shades of brown in colour; in the spring, however, when they become dimorphic, brown or green, they were found to be no longer sensitive. The caterpillars of *Boarmia roboraria* were, before hibernation, dark grey or brown in a dark environment, and greenish among green leaves.

Still more satisfactory results were obtained with caterpillars of *Rumia cratægata*. These were dark brown among black twigs, and light brown with green patches among green leaves. Miss Gould (2) also obtained similar results with this species. A striking experiment by Poulton in the subsequent year seemed to show that there is no tendency of these acquired colours to be inherited; indeed, the caterpillars from eggs laid by moths reared from the darker caterpillars were more easily affected by green surroundings than by dark; while the offspring of moths reared from the lighter larvæ were more sensitive to dark surroundings than to green.

But the most remarkable results were given by caterpillars of *Amphidasys betularia*. These were nearly all dark when many dark twigs were placed with the food, mostly dark when some twigs were inserted, and all green when no twigs were used. The presence of white paper spills caused the larvæ to assume a remarkable whitish hue. In 1892, Poulton carried out a more detailed series of experiments on this species. Caterpillars among black twigs were found to be nearly or quite black, those among brown twigs were brown, those among dead leaves were mottled-brown with vein-like markings. Artificial dark surroundings (black paper or enamel) caused the larvæ to be dark, but were not so effective as the twigs. Dark objects not in contact with the larvæ (piled around the cylinders containing the food) had no effect. Among green leaves and shoots only, the larvæ always turned green (the early stages are always brown), except when they were much crowded, in which case they had rather a darkening effect on each other; it appears that dark surroundings act more readily than green. With dark and green environment in feeble light, similar results were obtained to some extent, but the differences were by no means so marked as in daylight. In darkness, no difference was produced, whatever the surroundings, the caterpillars being always black, brown, or grey of various shades. The presence of blue paper spills caused the larvæ to assume a dark purplish hue, while orange paper among the food leaves produced a green colour in the caterpillars.

Poulton has shown that the dark pigments (to which the various shades of brown, black, grey, etc., are due) are deposited in the cells of the epidermis, while the green colouring-matter is found in the subjacent fat. The presence or absence of both sorts of pigment is determined by the surrounding objects, through some quality in the

light reflected from them. The suppression of the superficial dark pigment thus leads to the green colour of the caterpillar.

From these experiments, the fact that geometrid larvæ have a very considerable power of colour-adaptation to their environment appears to be beyond dispute. There seems no reason to doubt that this power is of considerable protective value to the insects, especially when we consider that the larvæ in which it is developed are all of a form closely resembling the twigs of their food-plants, but that they need some power of colour-adaptation to render the resemblance perfect. Poulton lays stress on the fact that all the numerous varieties of *A. betularia* produced by the experiments would be in correspondence with some possible natural environment. The experiment of moving caterpillars from one set of conditions to another showed that the early larval stages only are susceptible to any great extent; after the last moult but one, little or no change was produced. Hence, the most sensitive caterpillar has not that power of changing and rechanging its hues which is possessed by many fishes and rept" but then, in natural conditions, the environment of caterpillars will very rarely be changed.

In "The Colours of Animals," Poulton gave several instances of variation in the colour of the cocoons spun by caterpillars before pupation, which appeared to correspond to changes in the environment. The cocoons of *Saturnia pavonia* (*carpini*), *Eriogaster lanestris*, and *Halias prasinana* were found to be brown among leaves, and white in paper bags. Bateson (3, 4) has, however, now shown that the white cocoons of the two former species are not due to the colour of the surroundings, but to the disturbance of the larvæ before spinning. Caterpillars which were removed from their food, when ready to spin, produced white cocoons whether placed in dark or light bags, while when white strips of paper were placed about the food, without disturbing the larvæ, they spun dark cocoons. Bateson has further established that the dark hue of the cocoons is due to a brown fluid contained in the alimentary canal, with which the caterpillar colours the silk as it is discharged from the mouth, perhaps, also, staining the finished cocoon by an ejection from the intestine. When a caterpillar is disturbed, this fluid is voided before spinning, and hence the cocoon is white. Tutt has discovered (5) a similar cause for the varying colours of the cocoons of *Halias chlorana*, which, however, do vary in hue with their surroundings to some extent. Poulton has now made further experiments on *H. prasinana*, and, carefully avoiding disturbing the larvæ, finds that the cocoons of this species do vary in accord with the colour of the surroundings. The caterpillars seem to exercise choice in the colour of the silk they produce, and to be less irritable than those of *Saturnia pavonia* and *Eriogaster lanestris*. A larva which had begun to spin a white cocoon in a chip box was removed, an attempt was made to cut out the egg of an ichneumon, and the caterpillar was injured thereby.

Nevertheless, this insect, when placed among oak leaves and twigs, spun a dark cocoon! Such difference in behaviour in different species should guard observers in future against too hasty generalisation.

Poulton's recent experiments on butterfly pupæ confirm the results given in "The Colours of Animals." The most striking effects were obtained with *Vanessa urticæ* and *V. io*. These chrysalids vary from dark brown or black to bright golden (*urticæ*) or metallic green (*io*). As in the larvæ, the latter effects are due to the absence of dark superficial pigment, and are produced by light, or bright metallic surrounding surfaces. As with the larvæ, dark surfaces lead to the formation of the dark pigment. The pupal colour is fixed in *V. urticæ* by the surroundings of the larva when in the early stages preparatory to pupation, wandering in search of a suitable spot for attachment, or resting motionless before attachment. These stages are lengthened by dark surroundings, and also by disturbance; Poulton believes that during them the "colourless precursor" of the dark pigment which will appear in the pupa is being formed. The last stage of *V. urticæ*, after the caterpillar has suspended itself, is not sensitive, and little or no effect is produced by changing the surroundings then; but in *V. io* this stage is longer, and some result can be obtained by transferring the insects to a different environment.

Caterpillars of *V. urticæ* placed in a gilt cylinder in darkness changed to dark pupæ. Dark surroundings, in a strong light, produced slightly greater darkening of the pupæ than the same environment in the dark. Some larvæ were allowed to pupate in boxes lined with alternate strips of black and gold paper, so that the insect in its motionless stage rested partly on black and partly on gold. These produced pupæ of an intermediate tint, but none were parti-coloured. As the position of the head made no difference in the result, it seems certain that the colour seen by the caterpillar has nothing to do with the effect.

Many experiments besides simple dark and bright surroundings were tried with *V. io*. The larvæ, when exposed to blue light, produced darkish pupæ; when placed on orange and yellow backgrounds, bright green pupæ. Chrysalids of intermediate tints were formed amid bright green surroundings. Dark green surroundings and red paper backgrounds led to dark pupæ, but light falling through green glass, or through red glass or gelatine, made the pupæ light or bright green. When the pupæ were exposed to black ventrally, and to white dorsally, or *vice versâ*, the effect of the dark surface was stronger than that of the white.

The nature of the light-rays reflected from or transmitted through the various substances used, was tested by careful spectral analysis, and it appears that the presence of yellow and orange rays checks the formation of dark pigment, and tends to produce light, green, or bright larvæ and pupæ. The difference between the results with green

leaves and with certain green pigments is thus explicable; the light reflected from the leaves possesses yellow rays, and, consequently, produces light effects on the insects, while the green pigments absorb these rays. So also the red paper backgrounds absorb the rays and produce dark effects, but red glass or gelatine transmits the rays and causes green or light colour in the insects. Petersen (6) seems to have independently arrived at similar results, and to have given the same explanation of them.

The only imagine made the subject of experiment by Poulton was *Gnophos obscuraria*. This moth is mentioned in "The Colours of Animals" as being commonly light on chalk and dark on peat. A number of these insects were reared from the egg, some amid dark, others amid light surroundings, but in no stage was the colour of the insect affected by its environment, and the moths all turned out a light-grey hue. Poulton is led, therefore, to suggest that the prevalence of varieties in nature appropriate to the soil where they occur is the result of natural selection.

The cause of the darkening of moths in various localities has been for several years past frequently discussed, and numerous theories have been propounded. Lepidopterists have noticed that British moths are, as a rule, darker than specimens from the Continent, and that, within the British isles, western, northern, and mountain varieties are darker than insects from the southern and eastern lowlands, the melanic tendency being most strongly marked on the west coast of Ireland, and in the northern and western islands and highlands of Scotland. Yorkshire naturalists have noticed also a special tendency to melanism near the large manufacturing towns of the north, and the dark varieties are said to be increasing, while the lighter types are dying out. It has been suggested that the darkening is, in this case, due to the soot which the unhappy larvæ are compelled to eat with their food!

Natural selection, moisture, cold, and the absence of sunlight, have been put forward as serious suggestions for the cause of melanism. The districts where the phenomenon is most marked are the wettest in the country, and it is evident that constant moisture tends to render dark most objects on which insects rest, and so to favour dark varieties. Natural selection would therefore tend to the preservation of melanic moths in moist districts, but the general impression is that something in moist districts tends also to the production of such varieties. Tutt (7) and many other naturalists, consider moisture of itself to be a cause of melanism. Merrifield believes low temperature to be effective in the same direction, and has, during the last few years, conducted numerous experiments on the effect of temperature in producing dark colouration (8, 9) analogous to the well-known experiments of Weismann. By cooling the pupæ of *Selenia illustraria* (summer brood) and *Ennomos autumnaria*, he has obtained striking results, the moths being rendered decidedly

dark, those of the former species approximating to insects of the spring brood, which are normally dark. He has also succeeded in producing very dark examples of *Lasiocampa quercus*, by cooling the pupæ, and very light specimens of its northern dark variety, *callunæ*, by heating the pupæ (10). In these and other species, there seems no reason to doubt that low temperature applied to the pupæ does induce darkening of the imagine. On the other hand, the upholders of the moisture theory deny that there is such a general darkening of lepidoptera in arctic latitudes as should exist if low temperature be the main cause of melanism, and insist that the climate of the districts where melanism prevails is often mild and equable. This is true, but the summer temperature of these regions is lower than that of southern England, and much lower than that of Continental Europe. It is very possible that moisture may be a true cause of darkening; careful experiment might go far to settle the matter. It seems clear, however, that moisture or cold, or both, tend to the production of melanic varieties, and that these varieties, when produced, are specially favoured by natural selection.

Such observations have been made on few insects except Lepidoptera. There are, however, instances of melanism in Coleoptera. My friend, Mr. H. K. Gore Cuthbert, informs me that in Irish specimens of the beetles, *Badister bipustulatus*, *Dromius quadrinotatus*, *Nebria complanata*, and *Agabus guttatus*, the pale spots and markings are generally smaller than in English examples. A small ground beetle, *Calathus melanocephalus*, with red pronotum, has, in mountainous regions, the pronotum clouded with black (var. *nubigena*). I have lately received this variety (which is considered a rarity in England) from the Aran Isles, in Galway Bay, but little above sea level, and also from the Faroës. This is a good indication of the similar darkening effects produced in mountain, western, and northern regions.[1]

Specimens of the common wheel-web spider, *Epeira diademata*, which I have received from the Aran Isles, also show the same darkening tendency. The large conspicuous white spots in form of a cross, so characteristic of typical examples of this spider, are reduced in these western individuals to small dots. A similar, but less marked, variation is to be noted in specimens from co. Donegal.

Observations on this spider have furnished me also with a few facts bearing on the subject discussed in the earlier part of this review. The ground colour of the abdomen is well-known to vary from light brown through various shades of brown and red to a nearly black hue. Among granite rocks I have always found the latter variety; as it crouches in its retreat in a crevice of the rock, its black and white markings harmonise admirably with the mica and white felspar of the granite; but among herbage, the red and brown varieties are the more common. Whether this correspondence

[1] On the other hand, the black carrion beetle, *Silpha atrata*, is represented in Ireland by the var. *subrotundata*, which is generally of a brown colour.

be due entirely to the action of natural selection, or to some direct influence of the environment, experiment may perhaps decide. There can be little doubt that, to spiders, such correspondence is of "aggressive" as well as of "protective" significance.

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GEO. H. CARPENTER.

VII.

Experimental Embryology.[1]

EXPERIMENTAL Embryology is the youngest department of biological science; for although the idea of artificially influencing the germ is very old, and although even Swammerdam is said to have experimented in producing monstrosities, all the important results are very recent. They are already so important, both in themselves and in their suggestiveness, that it is imperative on every biologist to take stock of them. In part this has been already done by Weismann in his recent book on "The Germ Plasm," and by Roux, in an address delivered to the congress of the *Anatomische Gesellschaft*, held last June in Vienna; but already there are further researches of moment to be taken account of, and a fresh survey may be permitted. A critical review I do not propose to attempt, partly because that may be more profitably left to the expert embryologists, partly because it may be conveniently deferred until the body of facts has had time to become integrated.

I. Just as pathology sheds light on physiology—for instance, in the case of the thyroid gland—so teratology may help us to understand normal development. The most successful worker along this line has been M. Camille Dareste. He is the acknowledged chief of artificial teratologists. To attempt an appreciation of his results would lead us far into morphological questions; suffice it to say that he has experimented with the eggs of birds, placing them vertically instead of horizontally, hermetically varnishing part of the shell, keeping them slightly above or slightly below the normal temperature of incubation, heating different parts of the egg unequally, and so on. He has not only shown that the germ is plastic in the grip of its environment, he has been able to induce various malformations which are of interest to the student of morphology.

Dr. Bertram C. A. Windle, Professor of Anatomy in the Queen's College, Birmingham, has followed up some of Dareste's investigations, observing on the eggs of the fowl the effect of (*a*) excluding part of the air supply by varnish, (*b*) incubating under electrical current, and (*c*) incubating in the proximity of magnets. He is very cautious in his conclusions. He confirms, however, the view of Dareste that the same anomalies may be produced by diverse

[1] A Paper read before the Scottish Microscopical Society, March 10, 1893.

methods. "Whatever the disturbing agent may be which is employed, the failures of development are generally of the same nature —a consideration which, if true, as I believe, points to a much deeper explanation of the variations and malformations produced than that which attributes them to the actual deprivation of air, or whatever the means may be which is used." "I think there is now good evidence to prove that these disturbing agents act, at least in the great majority of cases, on that part of the developing organisation which is concerned with the formation of the vascular system of the embryo."

Professor O. Hertwig has recently (1892) published an elaborate essay, entitled "Urmund und Spina bifida," based on a study of abnormalities which occur in the development of frog ova in consequence of over-ripeness and polyspermy. The segmentation may be strangely irregular, large parts of the yolk may remain undivided, and the formation of the gastrula may be so disturbed that the blastopore or the primitive groove remains partially or wholly unclosed. He believes that the malformations which occur in the frog embryos in consequence of the imperfect closure of the "Urmund" are analogous to the Terata mesodidyma and katadidyma in Teleostean fishes and to Spina bifida in higher vertebrates. In the over-fertilised egg opposing factors conflict; on the one hand, there are forces tending to development and incited by the fertilisation, on the other hand, there are forces of an inhibiting and disturbing nature, due to the injury the ovum has sustained by over-maturation, or by other abnormal conditions operative before fertilisation.

II. We shall now notice some results of the important work of Dr. W. Roux. It is well-known that, in a number of types, the first three planes of segmentation in the dividing ovum correspond to the three chief planes dividing the bilaterally symmetrical adult into right and left, rostral and caudal, dorsal and ventral regions. In Amphibians and Ascidians, the first plane of segmentation in the ovum normally corresponds to the median plane of the embryo. This was shown for Amphibians by Newport, Pflüger, and Roux, and for Ascidians by E. van Beneden and Ch. Julin. Of the first two cells into which the egg of a frog develops, one has in it the material for forming the right half of the body, the other has in it the material for forming the left half of the body. It is a separate question whether each cell has, by itself, the power of forming half a body.

For a time, there was considerable difference of opinion as to the possible influence of gravity in determining the first segmentation planes. Pflüger (1883) maintained that gravity determined the arrangement of the molecules, determining, for instance, in the unsegmented ovum the position of the future nervous system or dorsal line; but Roux showed that when frog ova developed on a slowly rotating vertical wheel, so that the direction of the force of gravity was continually being changed, the segmentation remained normal. In

other words, the orientation of the ovum is regulated by internal conditions. Born reached the same conclusion by another method.

Having settled this point, Roux attacked the question whether all parts of the ovum were necessary to normal development. After two or more divisions, certain cells of the segmenting egg were pricked by a needle, so that some of the substance was lost. Thereafter, the development was watched until the embryo became differentiated. The result was that a loss of even one-sixth of the substance of the ovum produced only circumscribed local defects or disturbances. To develop fairly well, an ovum need not be intact.

Then Roux proceeded to exclude from development one of the two first segmentation cells. This was done by puncturing one cell with a hot needle. The result was that a typical half-morula, or half-gastrula, or half-embryo developed. Thus, there might be half cerebral vesicles, one auditory vesicle, a half-gut, a single row of protovertebræ, and a notochord of half the normal thickness. Sometimes, however, these half-embryos were slightly abnormal. Thus it was proved that one of the first two segmentation cells may form half an embryo; that it has not only the requisite vital material, but the requisite power; and, that it can develop apart from its neighbour. It is a likely inference that in normal development, a similar independence does to a certain degree obtain.

What is true when the first segmentation-plane corresponds to the future median one, is also true when the first plane corresponds to the future transversal, as is sometimes the case in the segmentation of frog ova. In other words, Roux was able to produce not only a right and left half-embryo, but also an anterior and a posterior half-embryo. By destroying one of the first four segmentation-cells, he was able to rear three-quarter embryos; by destroying three of the four, he got quarter-gastrulæ. Indeed, he goes the length of maintaining that the gastrulation of the frog ovum is normally a kind of mosaic work, formed in at least four vertical, independently-developing pieces.

Very remarkable is the process by which the half-embryo may form a whole by vitalising the injured half-egg which has been lying passive while the uninjured half has been developing. To this process Roux applies the term post-generation. Nuclei and, perhaps, also portions of protoplasm migrate from the uninjured side into the passive unsegmented mass; a remarkable kind of cell-division is set up; and gradually, in a peculiar manner which Roux describes, the missing half is post-generated. Quite lately, however, Roux has been able to rear an entire frog embryo from half an egg without any co-operation on the part of the other injured half.

III. Professor C. Chun observed in 1877 that when the two first segmentation-cells of a Ctenophore ovum were shaken apart, each formed a half-larva, with four ciliated ridges, four meridional vessels, and one tentacle. The half-larvæ actually became sexual, and by a process of budding, the half awanting was formed. This observation,

which was only published (through Roux) towards the end of last year, is the more interesting since Ctenophora are far removed from Amphibia. In both cases there is unequal segmentation of the ovum, and in both cases the formation of the missing half is long delayed.

IV. L. Chabry, experimenting with the ova of an Ascidian (*Ascidia aspersa*), obtained results which are, on the whole, similar to those of Roux. His method is to place the ovum in a glass tube, of almost equal diameter, and to puncture one of the first two segmentation-cells, observing it, meanwhile, under the microscope. The cell dies; the survivor forms a typical half-morula, half-gastrula, a right or a left half-larva. If the two anterior cells of the four-celled stage be destroyed, a posterior half individual results. Quarter and three-quarter forms were also obtained. Moreover, Chabry believes that he has been able to detect the cells whose descendants give rise to eye, notochord, attaching papillæ, &c. On to the sixteen-cell stage, at least, each cell has a determined destiny, it represents a definite part of the embryo; if a cell be destroyed, the defect in the larva is a definite one. It may be noted that Roux does not think Chabry's figures of half-gastrulæ really justify the title, it seems to him as if a regenerating process had already begun to operate so as to complete the embryo. There is not, however, any revitalising of the injured half-egg, for the injuries are fatal.

V. Carl Fiedler (1891) experimented with the ova of Echinoderms (especially of *Echinus microtuberculatus*), first trying the puncturing method, afterwards following the Hertwigs' method of shaking the eggs. When one of the first two segmentation-cells was punctured, so that it merely lost some of its substance, it did not die: it recovered and divided as usual, except that the cells to which it gave rise were smaller than those of the other side. The blastula was unsymmetrical, the embryo normal. But when one of the first two segmentation-cells was punctured so that the nucleus was fatally injured, the cell died; the surviving cell formed a half-morula, a half-blastula, and, perhaps, even a half-gastrula. When two cells of the four-cell stage were injured, a half-development also resulted, and that the same whatever pair of cells remained. Therefore, the first four cells are equivalent; but at the eight-celled stage this equivalence is lost, for different groups of four turn out differently.

VI. Hans Driesch (1891) also experimented with the ova of sea-urchins, pursuing the shaking method. When the first two segmentation-cells were shaken apart, one of them usually died, the survivor divided into a half-morula, as was the case in Fiedler's experiments; but the half-morula formed a closed blastula of less than the normal size. Then followed a small gastrula stage, and a minute Pluteus larva. Thus, from one of the first two segmentation-cells a normal but minute larva may develop. While Roux got half-embryos from half-an-egg, Driesch got half-sized but otherwise complete embryos.

Driesch is strongly opposed to the conception first clearly stated by His, that there exist in the germ organ-forming regions, for the marginal material of a left half-morula may become part of the median region, and eventually part of the right flank of the Pluteus. Moreover, in some cases, the first two segmentation-cells shaken apart give rise to distinct twins, or if the separation be imperfect, to double organisms, at least, to double blastulæ. In one case a double Pluteus was obtained from the imperfect separation of the halves of a blastula.

In a second paper, Driesch gives the results of further experiments on the eggs of sea-urchins (*Sphærechinus granularis* and *Echinus microtuberculatus*). Vejdovsky, in his account of the development of *Allolobophora trapezoides*—an earthworm whose eggs very frequently form twins—had suggested that the twinning was, perhaps, influenced by warmth, for it was most frequent in warm weather. This suggestion (for speculative suggestions are often valuable) prompted Driesch to try the effect of increased warmth on the developing ova of sea-urchins. The result was very striking. Almost all the eggs of *Sphærechinus* formed distinct twin blastulæ, gastrulæ, and even Plutei. In one case a connected twin gastrula was observed. The eggs of *Echinus* did not respond to the warming; and sometimes, strange to say, all the warmed eggs of *Sphærechinus* turned out single normal embryos. Still, that there is a relation between warmth and twinning seems likely.

Many experiments were made with the four-celled stage. If one cell was shaken off, or pricked to death, the three-quarters left almost always developed quite normally into Plutei. Yet the details of segmentation were of course different. To remove one of the first four cells does not hinder development to any appreciable extent.

But, more than that, an isolated cell of the four-celled stage, isolated by bursting the other three, develops as if it were still in its natural alliance. Very few of these, however, got beyond the blastula-stage, but two became Plutei. A quarter, a half, or three-quarters of the four-celled stage may therefore form a fully developed larva.

Selenka, in his studies on the development of marine Planarians, had observed that increased temperature produced deviations from the normal segmentation. This led Driesch to experiment. He found that with increased temperature the formation of smaller cells or micromeres is wholly or partially inhibited, and that in other ways the segmentation may be disturbed, but the striking fact is that slight changes in the position of the segmentation-cells, and even a modified type of segmentation, do not hinder the development of a normal organism.

Driesch proceeded to experiment on the effect of pressure, which was supplied by the weight of a cover-glass resting in part upon the ova. The nuclear spindles were disposed at right angles to the direction

of the pressure. The formation of micromeres was suppressed; the sixteen-cell stage was a plate; and yet typical Plutei were formed when the pressure was removed. In other cases, a two-layered plate of sixteen cells was formed, and yet normal Plutei resulted, a fact which seemed to Driesch definitely to contradict the conception of His that there are definite germinal areas. What should have formed one pole of the embryo formed the two sides, and what should have formed the other pole of the embryo formed both poles. In fact, the segmentation-cells of sea-urchins are on to the sixteen-cell stage markedly homogeneous, for even after a very abnormal development normal larvæ may result.

The Hertwigs have shown that when doubly-fertilised the ova of sea-urchins divide simultaneously into four. In such ova, which Driesch believed to have been doubly-fertilised, the whole rhythm of cell-division was double, so that the sixteen-cell stage was not a true sixteen-cell stage (with four micromeres) but a double eight-cell stage. But none of these forms developed.

VII. MM. Pouchet and Chabry, experimenting in 1889 with the developing eggs of sea-urchins, found that when some of the lime-salts in the sea-water were replaced by potassium or sodium oxalate, the skeleton of the larva was incomplete or entirely absent. An apology for a Pluteus with food-canal and other organs, but without skeleton, was formed, even when nine-tenths of the lime was got rid of. Further reduction of the lime inhibited all development, even gastrulation.

VIII. Following the researches of Pouchet and Chabry, the Hertwigs, and others, Herr Curt Herbst of Zürich has recently made an interesting series of experiments on the ova of sea-urchins. To the sea-water in which these were developing he added solutions of various salts, usually in the proportions of 3·8 grms. to 100 cm. of sea-water. Each experiment was checked by a control experiment in which the conditions were the same, excepting the addition of the salt-solutions. Fertilised ova were always used, to avoid the complications of polyspermy. The ova were those of *Sphærechinus granularis*, *Echinus microtuberculatus*, and *Strongylocentrotus lividus*, and in the three cases the results were practically the same.

Forty-four experiments were made in which so much of the sea-water, *e.g.*, 140 c.cm. out of 2,000 c.cm., was replaced by a 3·7 per cent. solution of KCl in ordinary water, which contained a considerable quantity of lime. So the observer had to deal with the addition of something new rather than with the removal of much lime.

The formation of the calcareous needles was delayed; when they appeared they were abnormal and always incomplete. The internal structure of the Pluteus was distinct, but the characteristic processes were small and rounded off. If there was less than 7 per cent. of the KCl solution the abnormality of the larvæ was less pronounced.

These results are not peculiar to KCl, for experiments with

KBr, KI, KNO_3, K_2SO_4, NaI, $NaNO_3$, $MgSO_4$ gave somewhat similar results. It is therefore possible by adding these salts to the sea-water to disturb the metabolic processes by which the calcareous skeleton is formed and the growth by which the peculiar Pluteus processes are formed, while the fore-, mid-, and hind-gut, the vaso-peritoneal vesicles, and the ring of cilia, develop uninfluenced.

The inhibition of the characteristic Pluteus outgrowths is interpreted by Herr Herbst as due to absence of the requisite stimulus naturally provided by the calcareous rods; for he believes, it seems, to me with much likelihood, that the rods afford a stimulus to growth in those parts where they occur.

When the solutions of the added salts were made in distilled water, with the obvious result of reducing the amount of lime in the aquarium, no needles of lime were formed even by the seventeenth day, although the cells which ought to have contributed to form them were then seen arranged in their typical disposition.

Another remarkable result was the production of larvæ (gastrulæ) in which the tuft of long, stiff processes, which normally occurs at the so-called animal pole, and is probably directive, was hypertrophied into a protruding knob with small mobile cilia. This observation was made at Trieste in 1891, but it did not succeed at Naples in 1892, nor again at Trieste in 1892, a divergence which the observer accounts for by the different temperature-conditions which obtained.

But, perhaps, the most remarkable results which Herr Herbst was able to make sure of, were obtained by adding salts of Lithium to the sea-water. The normal blastula elongates, constricts into two portions—one thin-walled, the other thick-walled. They differ also in pigmentation and ciliation. Between these two primary parts a connecting region is formed. In the majority no calcareous needles appeared. Very frequently the thick-walled portion grows at the expense of the thin-walled portion, which dwindles to a mere cap. In rare cases a gut and a mouth appeared. Now, had these larvæ lived a little longer, it seems quite likely that they would have reached the Pluteus stage to which they were approximating. If so, they would have reached it by an entirely abnormal path. But, without making any suppositions, the results are remarkable enough. It seems that the thick-walled portion is nothing more nor less than the protruded archenteron—a sort of embryonic hernia; that the thin-walled portion is the outer wall of the gastrula, and that the connecting portion is the hind-gut of the Pluteus.

It is interesting to observe that the potency of the Lithium salts decreases from the Chloride to the Iodide; in fact the potency is in inverse ratio to the molecular weight. So is it also with salts of Sodium and Potassium. The rule only applies to salts of one and the same metal.

It seems to have been this fact, taken along with researches of Hofmeister, Heidenhain, and others, that led Herr Herbst to the

conclusion, into the demonstration of which we shall not enter, that the influence on the developing ova was not directly chemical, but was due to the altered osmotic pressure of the sea-water. The changed chemical composition produces a changed osmotic pressure, this acts as a stimulus on the cells of the larva in such a way that they are diverted from their ordinary course of development.

IX. Mr. Edmund B. Wilson, of Columbia College, New York, who is well known for some valuable contributions to the embryology of Invertebrates, has recently published a preliminary account of experiments on the developing eggs of Amphioxus.

It is well known that the first stages in the development of Amphioxus are simple. The fertilised egg divides into two, into four, into eight cells, and so on until a blastula is formed whose cells are slightly larger in the lower hemisphere. By invagination the blastula becomes a gastrula.

By shaking the water in which the two-celled stages floated, Mr. Wilson produced two quite separate and independent twins of half the normal size. Each of the isolated cells segments like a normal ovum, and gives origin, through blastula and gastrula stages, to a half-sized metameric larva.

If the shaking has separated the two first segmentation-cells incompletely, double embryos—like Siamese twins—result, and also form short-lived (twenty-four hours) segmented larvæ.

Similar experiments with the four-celled stages succeeded, though development never continued long after the first appearance of metamerism. Complete isolation of the four cells resulted in four dwarf blastulæ, gastrulæ, and oval larvæ. Separation into two pairs of cells resulted in two half-sized embryos. Incomplete separation resulted in one of three types—(*a*) double embryos, (*b*) triple embryos —one twice the size of the other two—and (*c*) quadruple embryos, each a quarter size.

The eager observer proceeded to shake up the eight-celled stages, but in no case did he succeed in rearing a gastrula from an isolated unit of the eight-celled stages. Flat plates, curved plates, even one-eighth-size blastulæ were formed, but none seemed capable of full development.

This is most interesting. A unit from the four-cell stage may form an embryo, a unit from the eight-cell stage may not. "The inability to produce a complete embryo may be due either to quantitative or to qualitative limitations." Perhaps the one-eighth-cell has too little living stuff; perhaps it is already too much differentiated to regenerate the whole. We know that there is a size limit to the fragments of Hydra which will regrow an entire organism.

According to Mr. Wilson, two facts tell against supposing that the limit is quantitative. In the first place the one-eighth products swim actively, and live as long as the quarter embryos. In the second place, minute gastrulæ may be produced from two- or four-celled

stages, which are even smaller than one-eighth the normal size. These probably result either from fission of the half or quarter blastomeres, or by some fragmentation due to the shaking-up. "These minute embryos prove that a mass one-eighth the size of the normal ovum, or less, is capable of producing a gastrula," yet the one-eighth blastomeres did not.

It seems to me, however, that this is not quite conclusive; for although I am not one of those who exalt the nucleus at the expense of the cell-substance, it seems that we cannot exclude the supposition of a quantitative limit until we know more about the size and state of the nucleus (*a*) in the fraction or fragment of the two- or four-celled stage which did form a gastrula, and (*b*) in the one-eighth blastomere which did not form a gastrula. Yet, on the whole, I should agree with Mr. Wilson that the limitation is most likely to be qualitative, for by the eight-celled stage the difference between micromeres and macromeres has become pronounced. In short, the embryonic cells are beginning to be specialised.

It is very important to notice that the segmentation of the isolated blastomere of the two- or four-celled stage agrees exactly with that of the entire ovum. "The isolated blastomere develops as a unit, not as a half-unit, and the two cells to which it gives rise cannot be individually identified with those of a normal embryo-half. The development is transformed from the beginning; but in sea-urchin (Driesch) and frog (Roux) the development is at first that of an embryo-half, which subsequently, much earlier in *Echinus* than in *Rana*, gives rise to a perfect embryo by regeneration."

X. It is well-known that the entrance of a spermatozoon into an Echinoderm ovum is at once followed by the appearance of a thin enveloping membrane, which rises from the surface of the ovum and prevents the entrance of other spermatozoa. The spermatozoon which has entered influences the ovum so that others are excluded. It is not, however, certain that this membrane is really necessary for the prevention of polyspermy.

Now, the Hertwigs showed in 1887 that if unfertilised ova were placed in sea-water shaken up with chloroform, a protective membrane was formed. Herr Herbst has recently repeated the experiment, shaking up 1 c.cm. of chloroform with 50 c.cm. of sea-water. All the unfertilised ova placed in this mixture formed the protecting pellicle. He also succeeded when he used, instead of chloroform, benzol, toluol, xylol, creosote, or clove-oil, but the two last produced a pathological appearance in the cell-substance of the ova.

The protective membrane is really referable to the hyaline marginal layer of the ovum, which seems to have a greater consistence than the internal cell-substance; it has its analogue around the larval stages; in short, it is not in itself in any way remarkable. What is remarkable is the manner in which this marginal layer is delimited and hardened after the entrance of a spermatozoon. The hardening must

be due to an influence exerted on the cell-substance by the entrant spermatozoon. The delimitation is due, according to Fol, to the coagulation of a gelatinous substance between the envelope and the ovum. The Hertwigs and Herbst have shown that the hardening and the delimitation may be artificially evoked.

Furthermore, Herr Herbst shook off the membrane from fertilised ova, and found that in a benzol mixture a second membrane was formed. He even succeeded occasionally in producing a second membrane in addition to the first in fertilised ova, and in producing two concentric membranes around unfertilised ova.

The net result of this interesting little research is to show that the conditions productive of the hardening and delimitation of the cortical layer of the ovum are to be found in the ovum itself, and are normally due to a stimulus associated with the entrance of the spermatozoon, but which may be artificially replaced by the stimulus of certain chemicals.

Yet Weismann asks us to believe that the spermatozoon has no dynamic or other than merely quantitative influence on the ovum.

There are many other recent researches which should be considered in a full discussion of experimental embryology, but I shall refer only to two more.

XI. Very striking is Boveri's experiment, on which Weismann lays great emphasis, and which one would like to see confirmed. Boveri artificially removed the nucleus from the ova of a species (A) of sea-urchin, and poured over them the spermatozoa of a related species (B). The ova, without any maternal nuclei, seemed to have their deficiencies supplied by the spermatozoa, and the larvæ which resulted had the characters of species B.

To Weismann, this case supplies direct proof of the all-importance of the nucleus in transmission. I must confess to some doubt as to the certainty with which specific characters of sea-urchins can be discriminated in the larvæ. Moreover, hybridisation has sometimes strangely one-sided results, and from the non-appearance of any maternal characters, one cannot conclude with certainty that the cell-substance is only of nutrient importance in development. Furthermore, until definite proof to the contrary is forthcoming, it seems well to continue to believe that fertilisation is due to the spermatozoon and not merely to its chromatosomes.

XII. Finally, we may refer to Heape's remarkable experiment, in which, from an Angora doe rabbit (fertilised 32 hours previously by an Angora buck) he transferred two ova into the upper end of the Fallopian tube of a Belgian doe rabbit (fertilised three hours before by a Belgian buck). When the Belgian doe gave birth, four young were Belgian, two Angoras.

A speculative discussion of some of the researches cited above will be found in Weismann's "Germ Plasm" (pp. 134–144), and to this the reader may in the meantime be referred. I prefer in this summary to leave the facts to speak for themselves.

In regard to the scientific utility of these researches in experimental embryology, it may be noted that a number of general results are very clear. (1.) There is a great deal of life in an egg. Three-quarters, a half, a quarter will in favourable conditions form a complete larva. (2.) There is no little plasticity in the germ; the segmentation may be profoundly altered, the shape of the young embryo may be greatly changed, and a new type of larva may be produced. Yet the inherited characteristics are strong, for the experiments show a marked tendency in the germ to reach a normal result even by an abnormal path. (3.) To analyse out a definite factor—say osmotic pressure—in teratogeny is beyond dispute useful, for it may help us not only to understand malformations occurring among higher vertebrates, but also to get nearer an understanding of the conditions of normal development. (4.) As everyone knows, there are few facts, or, some would say, no facts, which can at present be cited with confidence as direct evidence of the transmission of environmentally-produced variations; and Weismann's case against the possibility of acquired somatic variations specifically affecting the reproductive cells is strong enough to lead many to depreciate, except in the case of simple Protozoa and Protophyta, the direct influence of the environment as a factor in the origin of transmissible variations. Be that as it may. How many ova are there which float in the sea and in other media; these are now, as similar ova have been in the past, exposed to the influence of very complex physical and chemical conditions; that their living stuff may be greatly affected the results of experimental embryology show; it is likely that the same is true in Nature's great laboratory; and the results, being germinal, may be transmissible. We need not be in haste to exclude the direct influence of the environment from among the primary factors of evolution.

One practical application occurs to me. Many abnormalities in the segmentation of the ova of littoral animals, *e.g.*, Gasteropods, have been noticed by various zoologists. The other day, in examining the ribbons of eggs which Doris lays in such abundance on the low-tide rocks, I noticed the great frequency of twins and triplets. In some cases they seemed to preponderate. Is it not likely that an explanation may be found in the fact that the ribbons are battered to and fro by the surge? What is done in the laboratory may also occur on the shore. The shaking may separate the segmentation-cells and mechanically produce twins. This is, of course, nothing more than a suggestion, in default of the obviously necessary experimental verification, but I find an interesting confirmation in C. Chun's observation that twin Ctenophora were most abundant after stormy days.

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20. ———.— (2) Ueber die Entwickelung der Froscheier bei Aufhebung der richtenden Wirkung der Schwere. *Breslauer ärztliche Zeitschr.*, 1884.

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27. **Windle, Bertram C. A.**—Investigations in Artificial Teratogeny. *Proc. Birmingham Phil. Soc.*, vii. (1890), pp. 220–232.

J. Arthur Thomson.

SOME NEW BOOKS.

ÉLÉMENTS DE PALEONTOLOGIE. By Félix Bernard. Première Partie. 8vo. Pp. 528, with 266 figures. Paris: Baillière & Sons, 1893. Price 10 francs.

THE position of Palæontology among the sciences is somewhat doubtful, and even a palæontologist may question its right to have a text-book at all. Do we not rather need books of the following kind?—first, a systematic zoology, embracing all creatures whether living or extinct, and arranged, according to the best modern lights, on a phylogenetic basis—a work that should illuminate the obscurities of the present by the light of the past, and show the true meaning of perished organisms by reference to their living descendants; secondly, a good stratigraphical geology, correlating, as nearly as possible, the beds of different countries, without laying undue stress on those of the author's native land, then tracing through these beds the histories of the various faunas all over the world, and, from the combined evidence, sketching out the evolution of land and sea masses from the earliest times to our own day. To such ends, at least, our endeavours are directed, and to the ultimate perfection of both these as yet unwritten books every palæontologist makes his contribution. The worth of that contribution is proportional to the amount of zoological and geological knowledge possessed by the contributor. It is idle to select from the garner of the zoologist just those facts relating to the hard parts of animals, and from the geologist's note-book such information as bears only on locality and horizon. What is a palæontologist without a knowledge of embryology, of geographical distribution, of marine life, sedimentation and currents, of some mineralogy and petrology, and of changes induced by metamorphism? And yet, even if it were possible, it would surely be absurd to collect all these diverse bits of information, however valuable, and, putting them between two bits of pasteboard, to call the whole a Text-book of Palæontology.

Nevertheless, the present dearth of competent British palæontologists—as evidenced by the failure of the Indian Survey to find other than Germans—when regarded in connection with the abundance of pure zoologists and geologists, seems to show that palæontology, in practice, if not in theory, must have its special study. Let us, then, concede that it may be convenient for the student to have his special text-book, and let us consider what it should contain. The contents may be arranged under three headings:—(i.) The general principles of the science; in other words, the principles of biology and of geology so far as they affect the history of extinct animals. (ii.) Practical aids to research, including an account of the way to collect, clean, dissect, and investigate fossils, a guide to bibliography, and hints on the working out of problems and the proper way of publishing results. (iii.) The main facts in the history of extinct animals, which should be related as to one already acquainted

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with geology and with the structure of living animals. Of course the above remarks apply with equal force to plants and the students of them.

These thoughts have been suggested by a Text-book of Palæontology from the pen of Dr. F. Bernard, of which the first part has just been published. We may now examine how far it fulfils our demands.

Of the three parts to be comprised in our ideal text-book, the second is here absent, if one excepts an occasional bibliographic reference and two or three pages on fossilisation. The first part, or "Généralités," though unduly compressed into 76 pages, is exceptionally good. It is well abreast of the times, and gives a fair and well-balanced account of the many widely-differing "doxies" that constitute modern palæontology. Thus, a chapter on Palæontology and Evolution contains excellent remarks on the conception of the species and how it has been modified by palæontological investigations, on the character, methods, and causes of variation, on adaptation, correlation and parallelism, and on almost every biological theory to which a name has of late years been given. Other chapters deal with phylogeny, with the distribution of organisms in past ages according to the conditions of environment, with methods of fossilisation, and with the main characters of the geological periods. In various places the author alludes to the minute research required of modern palæontologists, the investigation of every detail of structure in the face of difficulties that appal the zoologist, the comparison of all stages of growth and of every conceivable variation, the analysis of enormous masses of material, and the tracking of lineages from one horizon to another; and he rightly points out how all this can only be accomplished by the examination of innumerable well-authenticated specimens, and how progress must be continually aided by the increase of our collections.

The remainder of the volume is devoted to Animal Palæontology, which, for the present, only reaches the middle of the Mollusca. This part is by no means so satisfactory; the plan, indeed, is good, but the execution is faulty. In each group, after general remarks and a description of the morphology, the main forms are systematically alluded to, after which their distribution and relationships are treated of. This is right enough, but it is hard to see how much the reader is expected to know already. A person who accepts the term "zoophytes," which our author applies to sponges and echinoderms, would surely be puzzled by the word "polyp" suddenly introduced without explanation under the heading Hydromedusæ. In further support of our condemnation of this part it will only be necessary to quote a few sentences. Thus, the Echinoderms are not only called zoophytes, but their body is said to be "entirely covered with calcified dermal plates." How about most Holothurians? "Les interradiales n'existent pas chez les Crinoides actuels [*Thaumatocrinus*!]: elles se présentent toujours chez les fossiles [*Encrinus*!]." Pinnules are defined as "petits appendices creux"; when they are absent, as in *Cyathocrinus*, covering-plates are said to be present, and then the ventral groove is separated from the canal that contains the axial cord. It is hard to believe that a man who could write such incomprehensible nonsense is a pupil of a leading authority on Echinoderms—Professor Perrier. We will spare the author and our readers from further quotation. The excuse is ready enough. It is impossible nowadays for one man to write

an adequate systematic zoology, even though he confine himself to animals no longer living; the field is far too vast, the literature cannot be coped with, and the pity of it all is that young men fresh from the examination-room can only be taught their own presumption by a fatal experience.

Despite its inevitable faults, we recommend this book to students for the value of its introduction, and for its numerous good illustrations; while, to more advanced workers, it may prove useful, as setting forth the views of the French school on many subjects, some here published for the first time. We will only warn the unsuspecting against the specific names at the foot of many of the figures; the misprints, which this book shares with others issued by the same publishers, they will discover for themselves.

F. A. Bather.

Fauna und Flora des Golfes von Neapel: xix. Monographie; Pelagische Copepoden. By Dr. W. Giesbrecht. 4to. Pp. 532, pls. 54. Berlin: Friedländer and Sohn, 1892. Subscription price 50 marks.

The magnificent series of "Naples Monographs," of which the present is the latest, shows no symptoms of a falling off either in excellence of matter or in the beauty of illustration and printing. Nor do the volumes decrease in size; in fact, they seem to get bulkier with increasing age. Those of us who are interested in the work of the Marine Biological Association's Laboratory at Plymouth naturally feel some envy at the sight of the nineteen volumes which represent the industry of those who are officially connected with the Naples Station, or have enjoyed the great facilities for work there offered. It is greatly to be regretted that the list of subscribers to the funds of the Marine Biological Association is too small to permit of a sufficiently large expenditure of money upon publications. It is rather tiresome, too—the writer is naturally expressing his individual opinion only—that so much purely utilitarian work has to be done, to the partial, but, fortunately, not complete, exclusion of the more interesting lines of Biological enquiry, so admirably developed at Naples.

Dr. Giesbrecht's work is illustrated by fifty-four double plates, of which the first five are devoted to the representation of the living Crustacea with all their natural colours. These plates are so splendid that we feel ourselves unable to praise them adequately without indulging in language of too fulsome a character. The brilliancy of colour often developed in these small creatures is most marvellous; but while some, such as *Sapphirina ovato-lanceolata* (pl. i., fig. 7), exhibit literally all the colours of the rainbow, others are quite plain. We wait anxiously to hear authoritatively the reasons for this display of adornment. The variety of colour even extends to the eggs; they are blue, green, red, olive, or purple—in fact, they are only rivalled in variety by the eggs whose use is peculiar to that season of the year which has just passed. The remaining plates illustrate chiefly a collection of detached appendages of the various species dealt with. The development of these appendages is often quite extraordinary, while the long plumed hairs which they and the body-surface frequently produce are doubtless suggestive of the larval forms of the higher Crustacea. The volume is entirely systematic and faunistic in scope; the anatomy will be treated of in a second part.

ORDNANCE SURVEY. Report of the Departmental Committee appointed by the Board of Agriculture to inquire into the present condition of the Ordnance Survey. London: [Parl. Paper C.—6895]. 1893. Price 4½d.

THERE are, we suppose, few readers of this journal who do not take with them upon their perambulations sheets of the one-inch Ordnance Survey maps of the district they are visiting. As, however, there may be some such people, and as it a golden rule that one cannot too often be reminded of a good thing, we call attention to this Blue Book just issued. The report opens with a history of the Ordnance Survey, which may be said to have begun with the measurement of a base-line on Hounslow Heath in 1784. The one-inch to a mile map was commenced in 1797, and continued steadily till 1824, when the whole of the South of England and part of Wales were completed. In 1824 an Irish survey was commenced on a scale of six inches to a mile, and this was extended later to the whole of Scotland and the six northern counties of England; the one-inch maps issued since then having been reduced from those on the larger scale. Since 1855, moreover, the whole of the country has been surveyed anew on a different system, and the result of this work is seen in the "new series one-inch map," so well-known and so valuable to everyone.

After the "history" comes the "methods and processes of the Ordnance Survey," which need not detain us here. The Committee was appointed to consider (1) What steps should be taken to expedite the completion and publication of the new or revised one-inch map (with or without hill-shading) of the British Isles? (2) What permanent arrangements should be made for the continuous revision and speedy publication of the maps (1:500 (towns), 25-inches, 6-inches, and 1-inch scales)? (3) Whether the maps, as at present issued, satisfy the reasonable requirements of the public in regard to style of execution, form, information conveyed, and price; and whether any improvements can be made in the catalogue and indexes? To the first of these questions the answer given is that the continuance of the Temporary Edition ("Advanced Edition") of the maps by photo-zincography seems desirable.

To the second question, the Committee replies: "The 1-inch map is the one most used by the public for general purposes, both military and civil. We recommend strongly that its revision be carried out at all times independently of that of the larger scales, and that whatever funds are necessary should be provided to carry out these recommendations at as early a date as possible." That the "Cadastral Maps" (5 feet, 10 feet, 25 inches, and 6 inches to the mile), in consequence of the largeness of the scales, rapidly get out of date. "Scarcely any of them have been revised [since 1854], and it is urgently necessary that the advantage of so splendid a work as these maps of Great Britain and Ireland undoubtedly are should not be destroyed for want of a regular system of revision." With regard to the "town maps," they very properly advise that the municipal authorities "should be placed under a statuteable obligation to maintain and correct a copy of that map, showing all alterations in the town inserted in accordance with instructions and regulations to be issued by the Ordnance Survey Department . . . and that failing such proper maintenance, the ordnance surveyors should do what is found to be necessary, and charge the cost to the town." At present this revision is done in a few towns (*e.g.*, Edinburgh) by private publishers, who keep the maps up to date, and actually sell their own correct version to the displacement of the Government Survey.

Considering the enormous number of sheets these "town maps" occupy (13,860), and the small sale, the Committee seems to incline to the view originally put forward by Sir Charles Trevelyan in 1854, that the engraving of "town maps" is unnecessary, and that MS. copies might be supplied at a less cost than that entailed by the engraving and storing of so many plans.

With regard to the third question, the Committee deals with so many criticisms, complaints, and suggestions that it is impossible to quote them here; but they are all fully dealt with in the Report. The most sensible of all, perhaps, is the suggestion "That an edition of the 1-inch map, and of maps on a larger scale on *thin tough* paper be issued," and this the Committee says (p. xxx.) "might probably be serviceable for the 1-inch map." Another criticism, the faulty spelling of names, has been a long-standing grievance, but, as the Committee remarks, a most difficult one to cope with. They give some examples, but hardly any, perhaps, as curious as that of "Silex Bay," near Flamborough Head, the effort, possibly, of some astute surveyor with a smattering of geology, when glancing at the chalk cliff, to take down the local pronunciation of Selwick.

The Committee most truly says, "No country at the present time possesses anything as perfect and complete as our cadastral survey, published by the authority of the Government, and available at a moderate price to every person in the kingdom, showing every plot of ground and every isolated building, and having the new 1-inch map founded on it by the accurate copying power of the photographic lens." Whatever faults exist, this sentence is in the main uncontrovertible, and we sincerely hope that due provision will be made for the continuance and revision of our Ordnance Survey in as liberal a manner as the splendid results already achieved deserve.

A Manual of Bacteriology. By A. B. Griffiths, Ph.D., F.R.S.E., F.C.S. London: William Heinemann, 1893. Price 7s. 6d.

There is a suggestion of book-making about this volume. The frontispiece is a view of the outside of the Pasteur Institute; there is a ground plan and a view of the interior of the Edinburgh Bacteriological laboratory; a representation of a Zeiss' microscope; of microphotographic apparatus; microtomes; needles and knives, and much other delectable matter. The drawings of microbes themselves are not numerous or particularly clear. The suggestion of collation is by no means dispelled on reading. A fair account of microbes and the various methods of investigating them is given, but there seems no special reason why Dr. Griffiths rather than anyone else should have written the book. No doubt it is intended to be more elementary, but we miss the breadth of view and clearness of exposition to be found in Dr. Sims Woodhead's volume on "Bacteria and their Products." The publisher is, however, to be congratulated on the excellent get-up of the book.

Birds of Germany. "Deutschland's nützliche und schädliche Vogel." By Dr. Hermann Fürst. 8vo text, and fol. coloured pls. Pt. i. Berlin: P. Parey, 1893.

As a perfect marvel of cheapness, combined with the highest style of artistic excellence, this work, of which the first part is before us, can have but few if any equals; and as most of the birds of Germany are

common to our own country, it ought to command a large sale here, as well as abroad. The special object of the work is to enable all such as are engaged in rural pursuits to recognise the ordinary birds with which they may meet, and, at the same time, to distinguish between those which are harmful and those which are useful either to the agriculturist, the gardener, or the gamekeeper. With this object, it has been considered of prime importance that the illustrations should be on such a scale as to render the task of identifying a bird as easy as possible without the trouble of wading through descriptions. Accordingly, it is announced that the smaller and medium-sized species are to be figured of the natural size, while the larger kinds are to be reduced by one-half. Each part is to comprise four double folio plates, lithographed in colours, and accompanied by a sheet of 8vo text; the price of each part being three shillings (marks). The work is to be completed in eight parts; and if the execution of the plates is kept up to the standard of those in the first part, it will place trustworthy and artistic likenesses of a large number of European birds within reach of all, at an exceedingly low price. R. L.

L'AQUARIUM D'EAU DOUCE. By H. Coupin. (Bibliothèque des connaisances utiles.) 16mo. Pp. 348, 228 figures. Paris: Ballière et Fils, 1893. Price 4 fr.

THIS useful addition to Ballière's Library is written for young naturalists and those who take a casual interest in natural objects. The author treats of the aquarium and its forms, of water and its aëration, of the proper plants and their effect on the purity of the water, of methods of capture, and of the various groups of animals, giving a sketch of the life-history and other matters interesting for observation. The book is copiously illustrated, and although the figures are in some instances rather crude, they will be of considerable use to those for whom the book is intended. We are glad to see the footnote on p. 91 regarding the reversible Hydra.

CATALOGUE OF THE BRITISH ECHINODERMS IN THE BRITISH MUSEUM (NATURAL HISTORY). By F. Jeffrey Bell. London, 1892.

A CATALOGUE is hardly expected to furnish interesting reading; one does not exactly look into the British Museum publications of this kind with any expectation of finding easily assimilable tit-bits of zoological information; and yet this rule, like many others, is not without its exceptions. Dr. George Johnston's contribution to the series is full of highly readable and instructive notes concerning the "non-parasitical worms." Mr. Bell's catalogue is written on the plan of some of the more recently-issued volumes; that is to say, it is moderately learned and very arid, but, unlike many of them, it is restricted to an account of the inhabitants of the shores of Great Britain and Ireland. "One of its objects," writes the author, "is to supply the student of the British Marine Fauna with a handbook"—to the Echinoderms of our shores. Dr. Günther hopes in the preface that the study of Echinoderms will be encouraged by a perusal of this volume. We think, however, that the public, to whom the catalogue is addressed, will come to the conclusion that the study of the Echinoderms is fraught with too much contentious matter, and that it is as thorny as are the objects with

which it deals; for one quarter of the author's preface is devoted to an attack upon Mr. Sladen's "Challenger" volume upon the Starfishes. We are not concerned with the issue of this dispute, though, incidentally, we may remark that the opinions of a gentleman who has made material additions to the morphology of the Echinoderms are not treated with the respect that they should have met with at the hands of the author of the catalogue before us. The bracketed remarks in the following phrase strike us as not being at all in place. Mr. Bell says that Mr. Sladen repeatedly expresses "views for which he does not give (I do not say, does not possess) adequate reasons." The bellicose tone which is adopted throughout is not suited to the nature of the publication; and we desire to protest against its use in an official catalogue.

As to the catalogue itself, the list of genera and species is preceded by some "Introductory Remarks," occupying ten pages, and consisting of a sketch of the anatomy of the group. This sketch can hardly serve any useful purpose. It cannot be pretended that it is an adequate account of the structure of the Echinoderms; a more detailed and illustrated description of the hard parts alone would have been useful, as they are principally used for classification purposes; the catalogue is written by a systematist for systematists, and we cannot see why he should have attempted anything more ambitious.

In fairness to the author, we ought to say that the actual catalogue itself seems carefully and conscientiously done; he is here in his natural element; and there is a freedom from misprints which indicates much laborious work on the part of the author and (we presume) the editor. We cannot be too thankful that persons exist who are capable of doing and willing to do this necessary but uninteresting work for us.

THE EARTH'S HISTORY. An introduction to Modern Geology. By R. D. Roberts, M.A., D.Sc. [University Extension Manuals.] Crown 8vo. Pp. 365, with 8 plates and 51 illustrations in the text. London: John Murray, 1893. Price 5s.

UNDER the above somewhat ambitious title, Dr. Roberts adds one more to the numerous geological text-books. The volume is nicely got up, and contains several useful maps, printed in colours; but beyond this we cannot praise the book, for it contains only the well-known materials worked up again. One does not, of course, expect much originality in a text-book, but the student has a right to ask that the author shall make a good selection, and arrange the matter systematically.

HANDBOOK OF THE IRIDEÆ. By J. G. Baker, F.R.S., F.L.S. 8vo. Pp. 247. London: George Bell & Sons, 1892. Price 5s.

WE gladly welcome another addition to the valuable series of monographs by Mr. J. G. Baker, of Kew. The present volume is the fourth of a series devoted to certain groups of plants that enter largely into horticulture. To many it will be the most interesting of the series, for it deals with such handsome and well-known genera as Iris, Crocus, Ixia, and Gladiolus, besides a host of less familiar forms, unknown except to the systematic botanist.

Few, even of the professional gardeners, will have any idea of the wealth and variety to be found in this group of plants. Iris, for instance, has no fewer than 161 species, Crocus has 66 species, Ixia 24, and Gladiolus as many as 132. Plants of this order have, usually, such large and showy flowers, and many of them cross so freely, that they are particularly adapted for experiments on cross-fertilisation. For this reason, as well as for their beauty, we should like to see the group more fully represented in our gardens and greenhouses. Mr. Baker's "Handbook," we may observe, is in English. The author gives, besides the technical descriptions and habitat, full references to the earlier authorities, and, in the case of rare forms, he mentions the original discoverer.

THOSE who work upon the Tertiary Fossils of the United States will be gratified to hear that Dr. W. H. Dall has successfully grappled with the difficult task of determining the dates of publication of Conrad's books—"The Fossils of the Tertiary Formation" and the "Medial Tertiary." These books were issued in separate parts, all of which are now exceedingly scarce. Dr. Dall has given in his paper (*Bull. Phil. Soc. Washington*, vol. xii., 1893, pp. 215–240) full and exact descriptions of these separate parts and their contents, and has thus rendered a service to conchologists, after an amount of labour known to few. The paper gives an excellent idea of the difficulties met with in determining and establishing an exact nomenclature when dealing with publications the dates and history of which are involved and obscure. The author hopes to be able shortly to reprint both of Conrad's works. Mr. G. D. Harris, of the Smithsonian Institution, is publishing a reprint of "Fossil Shells of the Tertiary Formations," at three dollars a copy, and invites subscriptions.

We have received the first fascicule of Fernand Priem's "La Terre" (Ballière, Paris), a book which deals with Seas and Continents, Physical Geography, Geology, and Mineralogy. This is, of course, a more or less popular account of the subjects mentioned, but the numerous and excellent engravings, which appear to be chiefly taken from photographs, make it an exceptionally useful book for the student. It will be completed in four parts, at two francs fifty centimes each, and will contain 700 figures.

SENHOR CAZURRO concludes his elaborate study of the actinian *Anemonia sulcata* in the third part of volume xxi. of the *Anales Soc. Espan. Hist. Nat.* The structure of this form is worked out in the most elaborate detail, and important light has thus been thrown on the group. In the same number, Westerland prints a "Faunula Molluscorum Hispalensis," and Girard a paper on "Céphalopodes des côtes de l'Espagne."

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

We learn that the Plymouth Marine Biological Station has lost the services of Mr. Calderwood, who has resigned.

Dr. Preuss, at the instance of the German Foreign Society, has gone to Victoria, in the Cameroons, as director of the Botanical Gardens. (*Bot. Centralblatt*).

Sir Henry Ticehorne, who was attached to the Villiers Expedition for the exploration of Lake Rudolph, has returned to England in order to fulfil his duties as Sheriff.

Dr. William King, Director of the Geological Survey of India, has been granted an extension of service to July, 1894, notwithstanding the working of the 55 years' service rule.

Dr. D. Riva who took part in Professor Schweinfurth's last expedition to Eritrea, has undertaken a journey of exploration in the Ginba River district, East Africa. (*Bot. Centralblatt.*)

The fourth Report of the Epsom College Natural History Society, for 1892 has just appeared, and we see that the study of Natural Science is well appreciated in the school. The field-notes relate principally to dates of first bloom of flowers, Lepidoptera, first appearance of birds and dates of nesting, and meteorological observations.

Among the additions to the Herbarium of the Missouri Botanical Garden during 1892, we find mention of about 3,000 duplicates from the Herbarium of the late John Ball. The institution is also rejoicing in the donation by Dr. Sturtevant of his entire botanical library, which, according to the *Botanical Gazette*, "is undoubtedly the most complete and valuable American collection of pre-Linnæan botanical books."

On Monday, 27th February, the "Malacological Society of London" was founded, at a meeting held, by the kind permission of Mr. G. F. Harris, at 67 Chancery Lane. Mr. W. H. Hudleston took the chair, and the meeting elected Dr. Henry Woodward as the first president, Messrs. Hudleston, Melville, Godwin-Austen, and E. A. Smith as Vice-Presidents, Mr. E. R. Sykes as Secretary, Mr. G. F. Harris as Treasurer, and the following as its first council:—H. W. Burrows, J. H. Ponsonby, G. C. Crick, W. Crouch, Rev. A. M. Norman, B. B. Woodward and G. B. Sowerby.

A LIST of seventy names was read by the Secretary of those who had expressed their willingness to join the Society, and it was resolved that they be the original members. The annual subscription was fixed at half-a-guinea, and the entrance fee at the same sum; all those joining before December 31, 1893, will be elected by ballot in the customary manner, but will be exempt from the entrance fee.

THE meetings of the new Society will be held on the second Friday in each month (November to June) at 8 p.m., and, for the present, Mr. Harris has courteously permitted the Society to meet at 67 Chancery Lane. The Council as at present appointed will draw up the rules of the Society, which will be presented to the first General Meeting, to be held on April 14. Letters to the Secretary (Mr. E. R. Sykes) should be addressed to 13 Doughty Street, London, W.C.

THE objects of the society will be to study Malacology in all its branches, both recent and fossil, anatomical and skeletal, and if we may judge from the list of names furnished to us as founders of the society, has a great future before it. We hope, however, that it will do real scientific work, and leave the publication of local lists and small matters of that kind to the field clubs.

THE Geologists' Association will start for Norwich on Thursday evening, March 30. On Friday the members will visit the Norwich Crag and Chalk at Bramerton and Thorpe, on Saturday the cliffs at Mundesley, Trimingham, and Overstrand and on Monday those from Cromer to Sherringham and Weybourn. On Tuesday those who have the time still free will proceed to Lowestoft and visit the Pakefield Cliffs.

WE have received from Professor Jules Marcou a pamphlet, entitled "A Little More Light on the United States Geological Survey," containing bitter personal attacks on many of the geologists. We do not profess to understand the rights of these personal disputes, but are sorry to see, from various papers that have lately reached us, that party feeling is affecting the value of much of the geological work done in the United States, both by the official geologists and those outside. Some comments on the work of the United States Geological Survey will be found in our last volume (p. 644).

CIRCULARS from the World's Fair at Chicago arrive with startling rapidity. The latest we have received comes from the "General Division of African Ethnology," and unfolds a gigantic programme of work to be accomplished, dealing with Geography, History, Arts, Language and Literature, Religion, Natural Science, and Social and Political Science. Natural Science, of course, interests us the most, and we read that the following papers are promised:—"Astronomy," by W. W. Payne; "Structure of Africa and its Geological Systems," by James Geikie (invited); "Economic Geology of Africa," by Jos. Thomson (invited); "African Anthropology and Ethnology," by Heli Chatelain; "African Flora and Fauna, with Economic Botany and Zoology," by G. Schweinfurth and E. Holub (both invited). What the word "invited" means we are not told, but we only hope the desires of the promoters will be realised. There is a long and important list of "Advisory Council" printed at the end, and among those who are representatives of the different countries of the world, we read "H. M. Stanley, representing Humanity," and this entry comes first.

A SYSTEMATIC and alphabetical index of new species of North American flowering plants and ferns published in 1892 is in preparation at the United States National Herbarium. What a boon to systematists if all national Herbaria would follow this example!

A PRELIMINARY circular announces the organisation of a botanical survey of Nebraska, to be conducted by the Botanical Seminar of the State University at the expense of the members. Systematic botany evidently holds a far higher position across the Atlantic than in our English Universities.

PROFESSOR HERMANN CREDNER, of Leipzig, has issued a pamphlet, in English, explaining the methods and rate of progress in the Geological Survey of Saxony, of which he is Director. We observe that the greater part of the maps are already published, and that two or three years hence we shall see the completion of the Survey on the scale of 1 : 25,000.

ACCORDING to the *Botanical Gazette*, Professor Bolly, of the State University at Fargo, will exhibit at the World's Fair jars containing the tubercle-bearing roots of about forty species of North Dakota leguminous plants. The monumental work by Engler and Prantl (the progress of which, by the way, has somewhat slowed down of late), "Die Natürlichen Pflanzenfamilien," will be specially displayed by the publisher, W. Engelmann, of Leipzig.

FROM the same journal we learn that Beloit College has recently dedicated a new building, known as "Pearson's Hall of Science," in which admirably arranged botanical laboratories find a conspicuous place.

PROFESSOR BURDON SANDERSON, F.R.S., has been nominated to preside at the Nottingham meeting of the British Association. The following gentlemen have also been nominated to act as presidents of sections at Nottingham:—Section A, Mathematical and Physical Science, Professor Clifton, F.R.S.; Section B, Chemistry and Mineralogy, Professor J. Emerson Reynolds, F.R.S.; Section C, Geology, Mr. J. J. H. Teall, F.R.S.; Section D, Biology, the Rev. Canon Tristram, F.R.S.; Section E, Geography, Mr. Henry Seebohm, Sec. R.G.S.; Section F, Economic Science and Statistics, Professor J. S. Nicholson; Section G, Mechanical Science, Mr. Jeremiah Head; and Section H, Anthropology, Dr. Robert Munro.

Indian Engineering for February 18th has some remarks on "The Directorship of the Indian Geological Survey," from which we reprint the following:—"The Geological Survey has a lot of stiff geology before it yet in India which should be cleared off; though, as a matter of fact, the present director has been obliged to keep this geological progress back, in deference to the wishes of the Government to have mineral areas more thoroughly explored, and this doubtless goes against his reputation with his brother Geological Directors in Europe and America." The writer does not at all like the idea of "amalgamating the small scientific departments under a covenanted civilian," and prays, in view of such a contingency, for a special Mining Department. He goes on to say "The Geological Survey has been very judiciously kept away from the latter [gold and other mines]; though we are not so satisfied about the Department being kept, as appears to be the general policy, from making a proper *survey* of the gold and other mineral areas."

THE *Report of the Meeting of the British Association*, held in Edinburgh last year, has just been issued. Among the reports of the Committees we notice the following, which may be of interest to our readers:—12th on the "Earthquake and Volcanic Phenomena of Japan"; 19th on the "Rate of Increase of Underground Temperature downwards in various localities of Dry Land and under Water"; 18th on the "Circulation of Underground Waters"; 20th on "Erratic Blocks"; 3rd of the Committee to arrange for the "Collection, Preservation, and Systematic Registration of Photographs of Geological Interest in the United Kingdom"; final

report on "Cretaceous Polyzoa"; "Volcanic Phenomena of Vesuvius"; Zoological Station at Naples"; 5th on the "Zoology and Botany of the West India Islands"; 2nd on "Biological Association of Plymouth"; 6th on "Botanical Laboratory at Peradeniya, Ceylon"; "Teaching of Science in Elementary Schools."

The papers read in the various Sections are mostly published in short abstract, and have appeared in a more complete form elsewhere. Amongst the geological papers of interest, and containing original observations, we note one by Mr Peach "On a Widespread Radiolarian Chert of Arenig Age, from the Southern Uplands of Scotland," and another by Mr. Horne, "On the Contact Metamorphism of the Radiolarian Chert in the Lower Silurian Rocks along the Margin of the Loch Doon Granite." Mr. Dugald Bell criticises the "Alleged Proofs of Submergence in Scotland during the Glacial Epoch"; Mr. Clement Reid gives a list of the "Fossil Arctic Plants found near Edinburgh"; and Messrs. Peach and Horne write on "The Ice-Sheet in the North-West Highlands during the Maximum Glaciation," and on a "Bone Cave in the Cambrian Limestone in Assynt, Sutherlandshire." The remains found in the cave at Assynt seem to be of Recent or Neolithic date.

In the section for Biology, the chief botanical papers are Dr. Goebel's "On the simplest form of Mosses (*Buxbaumia*)"; George Murray's "On a comparison of the Marine Floras of the warm Atlantic and the Indian Ocean"; H. W. T. Wager, "On the structure of *Cystopus candidus*," the parasitic fungus of the shepherd's-purse; and Gustav Mann's, on "The Embryo-sac of Angiosperms." The papers of most interest in Zoology are those of E. W. Carlier, "On the structure of the so-called Hibernating Gland in the Hedgehog"; and "On the Skin of the Hedgehog"; of Gustav Fritsch, "On the origin of the Electric Nerves in the *Torpedo*, *Gymnotus*, *Mormyrus*, and *Malapterurus*. Several interesting economic papers on Fisheries—Calderwood, "On the Destruction of Immature Fish"; Holt, "On the Relation of Size to Sexual Maturity," and "On the Destruction of Immature Fish in the North Sea," and Cunningham "On the Protection of Immature Fish." Of the purely Physiological papers, the most interesting are—Marcus Hartog, "On Rabl's Physiological papers, the most interesting are—Marcus Hartog, "On Rabl's Doctrine of the Personality of the Segments of the Nucleus," and Weismann's "'Idant' Theory of Heredity"; and Gustav Mann, "On the Origin of Sex,"

A Guide to Sowerby's Models of English Fungi in the Department of Botany has been published by the Trustees of the British Museum. It has been prepared by Mr. Worthington Smith, who, a few years ago, restored the models to their original colour. The little book of 82 pages, with 93 illustrations, costs only fourpence, and deserves particular notice from the fact that it is not merely a guide to the models, but to the larger forms of our native fungi. All the prominent genera of the *Hymenomycetes*, *Gasteromycetes*, and *Ascomycetes*—prominent from their size—are represented here in their typical species with adequate descriptions, and a woodcut for each genus. The so-called microscopic forms are not dealt with, and since their study is not by any means so popular as that of the larger fungi, there will be no popular regrets on this head. The search for, naming, and drawing of mushrooms and toadstools is a very favourite pastime, apart from its scientific interest, among large numbers of leisured country folks, as well as the Saturday afternoon naturalists, and the issue of this little book for fourpence, with its well-executed figures and simple descriptions, is sure to be popular, apart from its interest as a guide to the models. In fact it is much more likely to be utilised away from the models than in presence of them. Mr. Worthington Smith's magnificent series of drawings exhibited beside the models is another attraction to the botanical gallery for students of Fungi, and the authorities are to be congratulated on the exhibition, as well as on the production of this Guide, which bears all the marks of honest work.

OBITUARY.

KARL AUGUST LOSSEN.

DIED FEBRUARY 24, 1893.

SCIENCE has sustained a severe loss in the death, on February 24, of Dr. Karl August Lossen, Professor of Geology at the School of Mines and at the University of Berlin, and chief Geologist of the Prussian Survey.

Dr. Lossen was an enthusiastic field geologist and an able petrologist. Most of his work was done in the Hartz, and his well-known map of that district is remarkable both for its accuracy and for the minuteness of the detail. He was one of the first to establish the immense importance of dynamic agencies in modifying the structure and composition of both igneous and sedimentary rocks, and he clearly recognised that the principles which he had established by detailed work in his own area were applicable to all parts of the Hercynian range in its course through Europe, from the West of England to the Sudetes. His early papers will be found in the *Zeitschift der deutschen geologischen Gesellschaft*, between the years 1864 and 1880. His later and, in some respects, most important work was published in the *Jahrbuch der königlich preussichen geologischen Landesanstalt*, and in the detailed memoirs on the separate sheets issued by the Prussian Geological Survey (1 : 25,000).

The great importance of his work has failed to attract the general attention which it deserves in consequence of his extremely involved literary style—a style which was probably due in part to his unfortunate deafness. All those who had the good fortune to meet him speak with enthusiasm of his charming personal character, and of his extreme readiness to communicate any information which he possessed. He was a Foreign Correspondent of the Geological Society of London.

WE have also to announce the death of Professor Schaaffhausen, of Bonn, whose name was prominently before the scientific world some thirty years ago in connection with the famous Neanderthal Skull. In 1880 he visited this country, bringing with him the original skull, which he exhibited and described at the Swansea meeting of the British Association. He was born in 1816.

WE learn with regret that Professor Karl Prantl died on February 24, at Breslau, where he was Professor of Botany and Director of the Botanical Gardens. He is, perhaps, best known in this country for his "Lehrbuch," of which an edited translation by Professor Vines has been widely used among students, who, while in need of a modern text-book, found that of Sachs somewhat too ponderous. He was also joint editor, with Professor Engler, of the valuable "Pflanzenfamilien," still in progress of issue, and sole editor of *Hedwigia*, the organ of those who love mosses. Professor Prantl was also the author of some Researches on the Morphology of Vascular Cryptogams, and an "Exkursionsflora fur das konigreich Bayern." Last year he started a new publication, entitled "Arbeiten aus dem Kon. Bot. Garten zu Breslau."

DEVONSHIRE has lost an enthusiastic and observant naturalist in the person of Edward Parfitt. Born near Norwich in 1820, the son of a gardener, he had from his earliest youth a passion for studying life of all kinds, which led him to go to sea in order to get some acquaintance with foreign animals. Wrecked near the Cape, he was obliged to make a long stay which increased his taste for Botany and Entomology, and allowed him to make a collection. On his return to England he devoted himself to Horticulture, and went to Devon about 1846. Since then Parfitt worked on the Natural History of the County, published numerous local papers, and has left a MS. on the Devon Fungi in 12 volumes, illustrated by 1530 plates, drawn and painted by himself. He was Librarian to the Devon and Exeter Institution for 32 years. Parfitt was entirely a self-made man, and Devon has lost a naturalist she could ill spare. He died on January 15.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, *now removed to* 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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ERRATUM.

P. 207, line 8, for "elder" read "alder."

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NOTES AND COMMENTS.

The Pollination of the Yucca.

MR. A. W. BENNETT, in his article in the March number of Natural Science, refers to the pollination of the Yucca, a well-known liliaceous plant, by a moth (*Pronuba yuccasella*), and those interested in the subject will find some "Further Studies," by William Trelease in the fourth annual report of the Missouri Botanical Garden. Hitherto, only two species, *filamentosa* and *glauca*, have been directly observed under cultivation, and the latter wild in Colorado, and the moth *Pronuba* is the pollinating agent in both. The present report describes further investigations in the field on these and other species.

The flowers of *Yucca baccata*, a widely distributed species, ranging in a variety of forms from southern Colorado into Mexico and California, are frequented by a moth somewhat larger and a little darker in colour than the *P. yuccasella* of the Mississippi Valley and Rocky Mountains, but otherwise indistinguishable from it, as is also the moth of the Gulf region. Like the Eastern representative, it rests within the flower during the day with the head directed towards the base of the stamens, and when depositing the egg, at night, "doubtless backs down between the upper ends of the stamens," piercing the ovary at about its middle in the thinnest part, viz., that between the internal partitions, which is not covered by the lower part of the stamen. "After thus depositing the egg in the ovule of the plant, the females behave as in *Y. filamentosa*, carrying loads of pollen from the anthers and thrusting it into the cavity of the stigma with the proboscis. This open canal is frequented by numbers of a white Thrips, which sometimes penetrate the cells of the ovary and doubt-

less assist fertilisation by scattering the pollen. Dried fruits of the previous season showed the perforations made by the escaping larvæ.

Yucca australis, studied in south-west Texas, shows seeds tunnelled in the manner characteristic of the moth, the pulp being perforated by the escaped larvæ. No flowers were observed, but large fruits, gathered some three weeks after fertilisation, bore none of the constrictions or indentations which so commonly mark the puncture of the moth, a fact which suggests that the eggs are deposited in the upper part near the stigma, as was found to be the case in the large tree yucca, *Y. brevifolia*. The agent in the flowers of the latter is the dark-coloured *Pronuba synthetica*, which is more active through the day than its eastern congener, and, unlike the other known species, slow to take flight.

This apparent disinclination to leave the flowers may, perhaps, be connected with the almost constant occurrence of high winds in the desert, and may restrict cross-fertilisation to flowers of the same plant more closely than in other Yuccas, though there must be frequent flights from plant to plant in quiet weather, and especially at night, when the wind sometimes falls. Moreover, the development of the stigma two days before the stamens of any one flower renders close fertilisation, in the strictest sense, impossible. Owing to the light yellow colour of the pollen, in marked contrast with the smoky tint of the moth, laden females can be seen from a considerable distance.

The female of most species of *Pronuba* seeks a fresh flower wherein to lay her eggs, preferring one in the first night of expansion, doubtless to ensure for her offspring a sufficient food-supply, the younger flowers being less likely to be already overstocked with eggs. This instinct is especially marked in *P. synthetica*, which was never seen to use any but the youngest flowers, while pollen-laden moths were repeatedly noticed forcing their way in the very narrow clefts between the rigid sepals of an opening bud, their flattened form facilitating this. When about to deposit an egg, the female runs to the bottom of the stamens much as *yuccasella* does, makes a rapid, more or less complete, circuit of their bases, and then quickly mounts to the very top of the pistil, and with her short, strong ovipostor cuts through the thin wall into the channel of the style close below the tip of the stigma, holding fast to the pistil meanwhile, the stamens being below her reach. The long extensile oviduct is then passed through the puncture, the egg being laid apparently within the ovary cell. The operation usually takes from two-and-a-half to three minutes longer than in other observed species. Sometimes two or more eggs are laid before the stigma is pollinated, but commonly, after laying each egg, the moth retreats to the bottom of the flower, and then again mounts the pistil till her head is even with the stigma, when she uncoils the large tentacles from their resting-place against her load of pollen and passes them back and forth in the stigmatic chamber with almost the same motion as *yuccasella*. The deposition

of the egg just below the stigma explains the remarkable symmetry of most of the fertilised pistils, contrasting with the constriction or indentation of the Eastern species at the point where the ovary has been punctured.

In another smaller species, *Yucca elata*, observed at Eagle Flat, Texas, *Pronuba yuccasella* was the agent of pollination. When about to deposit an egg, the moth, as in *Y. filamentosa*, runs nervously about within the bottom of the flower, then scrambles to the top of the pistil and backs down between two stamens by a succession of jerks, until her head is about level with the base of the style. Clinging to the pistil, she then punctures the ovary, and the egg is consigned to its place in the manner so well described by Riley for *filamentosa*. Usually each oviposition is followed by pollination, but in a few cases two eggs were laid before pollen was carried to the stigma, and sometimes so disturbed is the moth by the bright light of the observer's lantern, that she will leave a flower altogether without pollination. In one such instance her first act on entering another flower was to thrust her pollen-laden tentacles into the stigma, though it is unusual for this to precede oviposition. On several occasions, when disturbed in laying her egg, the moth ran upon a stamen, shaking it quite violently and making several passes at the anther with her tentacles, as if frightened from one of her occupations only to engage in another. The very distinct Californian *Yucca Whipplei*, as Professor Riley has shown, is pollinated by a distinct *Pronuba*, which he names *P. maculata*, of a general white colour, but mottled with black. In this species the pollen is not loose and powdery, as in Yucca proper, but glutinous, and frequent observation has shown that it may be deposited on the stigma directly from the anthers in closing flowers, so that self-pollination becomes possible, and Mr. Coquillet, of Los Angeles, records the seeding, in 1892, of a number of panicles which he had covered with gauze before any of the flowers opened. Owing to the more open and more diurnal character of the flowers in this species, the moth is far more active in the daytime than its congeners. Moreover, the resting position, with the head towards the stigma, is almost identical with that taken in oviposition, which, like pollination, may be witnessed at any time during the day. Standing on the side of the pistil the female pierces the ovary at the thinnest part. Generally not more than six eggs are laid in one pistil—one on either side of each septum—often fewer, so that even if they all hatch—an unlikely event—there is rarely more than one larva to each tier of seeds, thus allowing a fair percentage to ripen. Pollination was well seen in the variety *graminifolia*, where it was observed several times under a cloudless sky, the first time at about noon. Flying into a flower, the moth runs about the bases of the stamens in the usual way, then quickly clambers upon the inner side of a filament, and, with the tentacles stretched over the pollen masses, drags first one and then the other out of the anther cells, pressing

them together under the throat, and subsequently working them into a compact mass, much as *yuccasella* does the powdery pollen of other yuccas. The stamens are often quite violently shaken, so quick and energetic are the movements of the moth.

As the author suggests, the relationship of the Yuccas to one another and to the Pronuba moths would be far more intelligible could we trace their history, even a short way back, into geological time, but as no certainly identifiable Yuccas exist in even the latest deposits, this is as yet impossible.

He thinks it reasonable to suppose that an ancient type, represented in the tree-like *Y. brevifolia*, with an equally ancient type of Pronuba, has persisted in the Pacific region, perhaps, for the same reasons that have led to the preservation of the gigantic Sequoias in the same region, while the more widely distributed species have become differentiated, and, with their pollinator, passed to the south under new conditions.

THE "ZOOLOGICAL RECORD."

IN the Report of Accounts of the Zoological Society for 1892, there is an item of £937 for "cost of *Zoological Record*." The same entry for 1890 amounted to £650, and for 1891 to £420. There is no statement as to the reason of this enormous increase—a somewhat unfortunate oversight on the part of those who drew up the Report—and it is well that the matter should be explained. The £937 not only includes the *Record* for 1891, but includes a great part of the *Record* for 1890. The "sales" for this invaluable compilation were, in 1890, £298; in 1891, £266; in 1892, £354; a net improvement in the last year of the three of £56.

It seems rather difficult to believe that a volume like the *Zoological Record* should only enjoy the ridiculous sale of barely 250 copies; and it is certainly most praiseworthy that the Zoological Society, despite the annual loss, refuses to allow the good work it is doing to fall to the ground. Surely there is no museum or laboratory in existence that can afford to neglect zoological literature to such an extent as to refuse to subscribe to the *Record*; and yet it is hard to believe that there are only 250 museums or libraries of repute in the whole world. There is certainly no other Record that is anything like so up-to-date as the Zoological Society's publication. It has always been a matter of regret that some amicable understanding has not been arrived at by the Royal, the Linnean, the Geological, and the Zoological Societies which will allow the proper listing and publication annually of a complete literature of the animal, mineral, and vegetable kingdoms. The Zoological Society, despite the enormous outlay required by the Gardens, cheerfully bears the whole of the expense of providing students of zoology with

an efficient yearly catalogue, and sets a worthy example to its sister societies, with their accumulated and accumulating capitals, and the additional benefit enjoyed by them of rent-free premises in Burlington House. We look for better things from scientific societies than heavy investments in the Funds.

Ice in Hong Kong.

The *Gardener's Chronicle* of April 8 gives some particulars of the unprecedented cold weather which occurred in Hong Kong last January. There is no previous record, says Mr. Ford, the Director of the Botanic Gardens, of ice occurring in the island below 1,700 feet, and hence, as it lies within the tropics, the result was most disastrous, indigenous, as well as cultivated, plants suffering.

"The continued low temperature," he says, "combined with a fall of rain from an apparently warmer stratum of air above, resulted in the formation of ice varying in quantity from a thin coating on the upper leaves of pine trees growing at 300 feet above sea-level, to a thick encasement of perfectly transparent solid ice of 5½ inches in circumference on the blades and bents of grass at the summit of Victoria Peak." Leaves of evergreen shrubs and trees bore solid coverings of ice three-eighths of an inch in thickness, the great weight of which caused the branches to assume a pendent form, and in many cases snapped off the limbs. All vegetation throughout the hill regions of the colony was covered in this way with ice, as were also most other objects. Telegraph wires had a casing more than half-an-inch thick, and bore icicles three inches long as close as they could be packed side by side, while the windward sides of the walls of the look-out at the peak were sheeted from top to bottom with a perfectly transparent coat three-quarters of an inch thick. All the hills on the mainland and Lantao Island were likewise white with ice.

The damage in the Gardens consists chiefly in injury to foliage, but some plants, natives of warmer regions, are quite killed, and many of the decorative plants which survived will be months before they reassume their ornamental appearance.

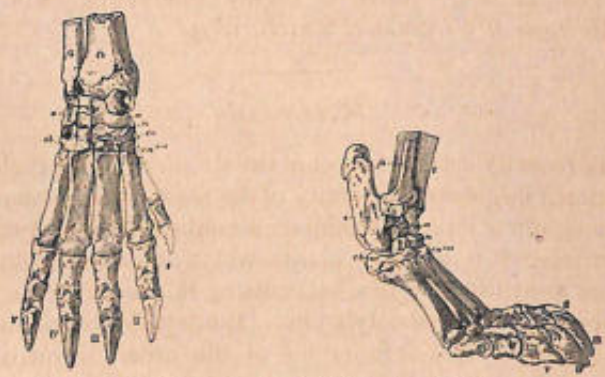
In spite of every possible precaution in matting-in the plant-houses and covering the roofs with straw, the contents suffered greatly, many of the best orchids being killed outright, and others so much injured that even if they survive it will be years before they regain their former luxuriance. It is interesting to note that, while a healthy plant of *Dendrobium aggregatum* received from Calcutta several years ago is apparently killed, plants of the same species growing by its side, and others on trees where they had no shelter, which were collected ten years ago on the Lo-fau mountains, about sixty miles from Canton, have escaped unharmed. Several exotic trees planted on the hills had all their leaves killed, while above 600

feet the indigenous vegetation was much injured, especially the tropical plants which here reach their northern limit.

Dr. Henry reports that the destruction of vegetation about Canton has also been very great. The banana plantations are ruined the bamboos have suffered, the candle-nut trees (*Aleurites triloba*) are all shrivelled up, while begonias, euphorbias, crotons, and scores of others, are quite destroyed.

A Clawed Artiodactyle-like Mammal.

From several references to the subject, readers of Natural Science are probably familiar with the fact that the remarkable clawed Miocene and Pliocene mammal known as *Chalicotherium* presents many curious resemblances to the ordinary Perissodactyle Ungulates. Messrs. Osborn and Wortman now announce the discovery of another American Tertiary mammal, for which they propose the name *Artionyx*, with almost precisely similar resemblances to the Artiodactyle divi-



Front and hind foot of *Artionyx*.—(From Osborn.)

sion of the Ungulates. The authors refer both these curious creatures to a single group, which they suggest originated from the primitive Ungulates before the acquisition of distinctly Ungulate terminal phalanges; and they call attention to the curious double parallelism existing between these two forms on the one hand and the Artiodactyle and Perissodactyle Ungulates on the other. All this confirms the view which we have so frequently urged as to the impropriety of separating *Chalicotherium* as an order from the Ungulates.

Mountain Chains as Barriers.

Dr. J. Regnault, in a lecture before the Ethnographical section of the Geographical Society of Paris, has drawn attention to the "Influence of Mountain Chains on the Distribution of the Human Race." His original observations resulted from a visit to Darjeeling and the Terai, but his conclusions are drawn from a study of the

whole of Europe and Asia. He states the following laws:—(1) When a chain of mountains, such as the Alps, the Pyrenees, separates two nations, ... one generally thinks that the crest, the water-shed, divides two races, each inhabiting his respective slope up to the summit. Nothing of the kind, and one can say that the crest of a chain does not separate two races. In support of this statement, Regnault says:—This error is due to the fact that the crest generally serves as a frontier between two States. (2) Ordinarily the race of the country towards which the lesser slope inclines inhabits not only this slope, but crosses the crest and colonises the more rapidly-sloping side down to the plain in which the slope is lost. It is the junction between the rapid slope and the plain where the contact of the two races is to be found. To the second law he adds two postulates:—(*a*) The mountain chain must be high and long, as the Alps and Pyrenees; (*b*) The law is modified by considerations of time, geology, climate, &c. Dr. Regnault discusses in some detail the Alps, the Pyrenees, Erz-Gebirge, Caucasus, and the Himalaya. The lecture, which is highly interesting, will be found printed in *Revue de Géographie*, March, 1893.

Myeloxylon.

In the recently-issued number of the *Annals of Botany* (vol. vii., p. 25), A. C. Seward discusses the affinity of the fossil plant genus *Myeloxylon*. Corda thought it a Palm, Brongniart a doubtful Monocotyledon, while Goeppert referred it to those plants which must be looked upon as prototypes or synthetic types, recognising in its structure the Fern, Gymnosperm and Monocotyledon. Binney, Williamson, Renault, and Kidston make it a fern rachis of the order Marattiaceæ, while Schimper, Schenk, and Felix prefer to regard it as the petiole of a Cycad. The present account embodies the result of an examination of some new material from the Millstone Grit, and elsewhere, and also of some specimens now in the British Museum, and once the property of Robert Brown. The author has had the good fortune to get sections showing very well the structure of the vascular bundles, the form of which closely corresponds with that in Cycads of to-day, while the position of the first-formed wood vessels on the side of the wood next to the bast is also in striking agreement with these. The bordered pits, characteristic of the recent Cycads, were not seen, but there are points of resemblance between the wood elements in both, while the resin canals of recent Cycads and the fossil petioles are practically identical.

On the whole, the author concludes that the fossil specimens approach more nearly to the Cycads than the Ferns. The few points of difference which distinguish them from the petioles of recent Cycads are, he thinks, far outweighed by the close parallelism in more essential characters, and he looks upon *Myeloxylon* as an

extinct genus not corresponding exactly to any recent family of plants, but coming very near the Cycads in anatomical structure, and probably holding a position between Cycads and Ferns, but nearer to the former.

EMIN PASHA.

THE *Berliner Tageblatt* for April 5 publishes a letter from its special correspondent, dated Fort Kampala, Uganda, December 14, 1892, which tends to confirm the rumours of the death of Emin Pasha. According to the letter, news had reached Fort Kampala that Emin Pasha marched from Kavalli to Masamboni, and thence to the Ituri river, on the banks of which he was attacked by a body of Manyema and killed. The correspondent of the *Tageblatt* further reports having met an Egyptian official, named Awad Effendi, on his way to the coast, who informed him that he was present with Emin in Masamboni's country, and added that Emin Pasha and all his followers had been murdered on the Ituri river by Manyema, under the leadership of an Arab named Ismail. Awad Effendi believed that the murder was committed on March 12 or 13, 1892.—*Reuter*. This may be another version of the rumour to which we referred in July; at present, we cannot be sure of the accuracy of the information.

THE AFFINITIES OF ZEUGLODON.

IN the last issue of the *Proc. Zool. Soc.*, Mr. Lydekker describes an interesting series of Cetacean remains from the Eocene of the Caucasus. Among these, special importance attaches to certain bones belonging to the imperfectly-known creature designated *Zeuglodon*, which, as our readers are doubtless aware, Professor D'Arcy Thompson has recently endeavoured to remove from its assigned position among the whales to associate it with the seals. The most important specimen among the new find is a humerus, of which but one example has been hitherto known, and the study of this leads the author to conclude that *Zeuglodon* is much more likely to be an ancestral Cetacean than a Seal. Doubtless, however, Professor Thompson will have a word to say on the subject. Scarcely less interesting is the discovery of a skull in the same deposits, indicating a Cetacean nearly allied to the existing *Inia* and *Pontoporia* of the South American rivers. We regret that the artist has scarcely done justice to the author's specimens.

NEW MONKEYS.

IT is probably a unique feature in a single number of the serial just mentioned to find three new species of monkeys described and illustrated with coloured plates. Two of these belong to the Oriental genus *Semnopithecus*, and the third to the Ethiopian *Cercopithecus*.

In regard to one of those belonging to the former genus, the describer observes that specimens may subsequently show a transition from the supposed new form to one which he has previously named *S. hosei*, in which case the former will have to be reduced to the rank of a variety. Nevertheless, he adds, "in the absence of such intermediate forms, and in view of the great constancy in the coloration of *S. hosei* already noted, it seems best to give a name to the striking variation from it now described." We venture to have doubts as to the wisdom of this course.

Pictures at Brighton.

It is not often that Art and Science go hand in hand, therefore two collections of pictures now exhibited on loan at the Brighton Corporation Art Gallery are of an unusually interesting and instructive character, from a scientific point of view. The series of African scenes, lent by Mr. Robert White, painted in oils by Mr. Thomas Baines, the well-known explorer, illustrate the geology, zoology, botany, and ethnology of the South and West regions of that continent. The successive river-terraces of the Great Zambesi, and its winding, deep-cut, cañon-like gorge, are well depicted by the artist, a brother of Mr. Geddes Baines, discoverer of the Dicynodonts. There is a picture of the spot where remains of one of those extraordinary reptiles were first found. Miss C. F. Gordon Cumming exhibits a series of water-colour sketches of Ceylon and Fiji, including a fine set showing the river-terraces, silica-deposits, sulphur, mud, and hot springs of the volcanic district of New Zealand, where the whole aspects of the region were so completely changed by a subsequent convulsion of Nature, that these pictures portray scenes no longer in existence. The luxuriant flora of Ceylon is well illustrated, and there is a capital sketch of the sacred Singalese relic "Buddha's tooth," whether "shed" or "removed" tradition is silent.

Fauna of the Plöner-See.

We are glad to welcome the first number of the *Forschungsberichte aus der Biologischen Station zu Plön* (Berlin: Friedländer), by Otto Zacharias, the Director. The number opens, as it should do, with a list of the Fauna of the great Plöner-See, the compilation of which has brought to light many new forms, all duly described and figured in the succeeding pages. These include a Rhizopod, a Heliozoan, a Mastigophore, an Infusorian, a Turbellarian, and eight Rotifers. If so good a result is achieved in making up a first list, we may expect a large addition to our knowledge of fresh-water life from the work done at the Biological Station at Plön.

THE PEOPLING OF THE WORLD.

THE last of those valuable "Ergänzungshefte" (number 107), published at intervals by the proprietors of *Petermann's Mitteilungen*, is devoted to the "Peopling of the World." Drs. Hermann Wagner and Alexander Supan have to be thanked for the prodigious labour involved in the compilation of this great census, the mass of figures in which is simply appalling. The number consists of 130 pp., and is crammed with information. The continents are taken in order, and, whenever possible, the census of the actual town is given in 1891. Details of the previous census are also given in each case, so that the reader may see at a glance the increase of population in a certain number of years. Towards the end comes a list of the large cities of the world and their inhabitants, and we extract the first six as having special interest:—

London (1891)	4,415,958
Paris (1891)	2,712,598
New York and Brooklyn (1890)	2,352,150
Berlin (1890)	1,763,543
Canton (1891)	1,600,000
Vienna (1890)	1,364,548

There are only 12 cities with over a million inhabitants, of which China rejoices in possessing four.

FRESH-WATER MEDUSÆ.

SPECIMENS of the fresh-water Medusa from Lake Tanganyika, the same probably as that referred to by Böhm in 1883, and Wissmann in 1887, have been received in good condition, and form the subject of a paper by Mr. R. T. Günther in the April number of the *Annals and Magazine of Nat. Hist.* The individuals vary in size from 1 to 1·8c. across the bell, while the largest measured as much as 2·2c. The umbrella is flattened; the tentacles very numerous, varying in length, and arranged to a sixth order with great regularity. Mr. Günther establishes a new genus, *Limnocnida*, for this interesting animal, and preserves Böhm's name of *tanganjicæ* for the species, that author having recognised the form as new, although he was unable to describe it in 1883 for want of the necessary literature. The fresh-water Medusa which thrives in certain years in the tank at the Botanic Gardens, Regent's Park, *Limnocodium sowerbii*, differs considerably from this new form.

THE PLAGUE OF VOLES.

WE have referred on several occasions to the subject of the plague of Field-Voles in Scotland, and we shall, therefore, merely chronicle the publication of the "Report of the Departmental Committee on Field-Voles (Scotland)," Parl. Paper—C.—6943, 1893, price

1s. 4d., which appeared early in the month. The Report traces the nature and origin of the plague; causes of the outbreak; effect on the pasture and on the stock; remedies; previous outbreaks, among which the Philistines and other biblical stories are quoted; natural enemies of the voles; conclusions and recommendations. Of course, the obvious remedy, and one which is hinted at in the Report, is the non-destruction of the birds and mammals that destroy the voles for food; but it is almost useless to insist on the preservation of the balance of life in Nature where the game-breeder and game-keeper are concerned. The owls, buzzards, and kestrels, the stoats, weasels, and foxes, are sacrificed to the game-god, and therefore the sacrificers bring other (and greater) evils on their own heads and on those of others. The Report is illustrated by figures of voles and their natural enemies.

A New Variety of the Pala.

In the last number of the *Proc. Zool. Soc.*, Mr. Thomas describes a new variety of antelope differing from the ordinary Pala by its much smaller horns and more slender skull recently obtained from Nyassa-land. The especial interest attaching to this form is that it occupies an "island" within the distributional area of the common Pala.

Costa Rica.

An excellent map of Costa Rica will be found in "Geografia de Costa Rica," by F. M. Barrantes (*Barcelona*, 1892). This work, which has been occasioned by the Exhibitions of Madrid and Chicago, gives a sketch of the territory, its limits and coast-lines, mountain and river systems, climate and productions. A list follows, of the birds, taken from Zeledon's papers, reptiles and fishes, and mammals. This last is a translation of Frantzins' paper, and occupies 72 pages. It is followed by a series of chapters on population, government, and religion, and then the book deals with the various provinces and islands. There are a great number of full-page illustrations, reproductions of photographs of town scenes chiefly, and one of the crater of the volcano Poas, which is most interesting, being taken from the edge, and showing the crater-lake. There is no index.

The Origin of the Dingo.

In a useful descriptive catalogue of the Mammals of Australia, recently issued by the Australian Museum, Sydney, Mr. J. D. Ogilby enters very fully into the disputed question of the origin of the Dingo, or native dog. After quoting the chief recent authorities who have written on the subject—more especially the late Professor McCoy—

the author observes that, in regard to the question whether the dingo "is an indigene or has been brought hither through human instrumentality, we consider, notwithstanding that the greater number of authors incline to the latter theory, that the recognition by Professor McCoy of fossil remains, in no wise differing from those of recent individuals, and contemporaneous with similar remains of *Thylacoles*, *Diprotodon*, &c., sets the question at rest, and goes far towards proving that the species is indigenous to continental Australia, and was an inhabitant thereof prior to its colonisation by man, no human remains of such antiquity having as yet been discovered." This conclusion is supported by the recent discovery of dingo remains in a stratum lying beneath 63 feet of volcanic sand, underlain by another 60 feet of blue and yellow clay, thus clearly indicating the great antiquity of the animal in question.

From our own experience of the specimens in the British Museum, we have hitherto believed that remains of the dingo only occurred in the Australian cavern-deposits, where, we believe, human remains also occur; but these new discoveries seem to indicate the existence of this dog at a period when it is almost impossible to conceive that man had reached Australia. Startling as it may seem, we are, therefore, inclined to believe that the dingo may really be an indigenous Australian mammal. There are, indeed, several indications that we shall ere long have to revise our views as to the origin of the mammalian fauna of Australia; and the number of placental land mammals indigenous to that country noticed in the catalogue before us shows how incorrect it is to speak of Australia as exclusively a land of Marsupials and Monotremes.

Admitting that Mr. Ogilby may very probably be right as to the indigenous origin of the dingo, we can, however, scarcely follow him when he states that "until proof to the contrary is forthcoming, we shall consider the honour of being the original progenitor of our household favourite as the due of the Australian warrigal" (another name for the dingo). Surely the author is aware that in Europe the remains of dogs closely allied to the wolf occur in Pliocene beds, and that there is every reason to believe that the Eskimo dog, as well as sheep-dogs, are more or less directly descended from the wolf, while the Hare Indian dog of North America is just as closely related to the coyoté. The origin of the numerous breeds of domestic dogs cannot, indeed, be disposed of in this off-hand manner.

NESTING OF THE GOBIES.

A PAPER, by F. Guitel, which has appeared in the last number of Professor de Lacaze-Duthiers's *Archives de Zoologie expérimentale*, contains interesting notes on the hitherto little-observed habits of one of our commonest coast-fishes: *Gobius minutus*. The Gobies belong to the few fishes which construct a nest for the shelter of

their eggs, a work which, as in our Sticklebacks, devolves on the male parent alone. Although the fact of Gobies making nests has been known since the time of Rondelet (1554), no sufficiently-detailed account of the breeding habits of any species of the genus had yet appeared. The male *Gobius minutus*, for the nest, in tidal pools chooses usually the shell of some bivalve, frequently the Cockle, under the concave surface of which the sand is hollowed out and cemented by a special mucilaginous secretion from the skin of the fish; a cylindrical tunnel gives access to the nest, which is covered over with loose sand. The female having deposited her eggs, fixed to the shell, the male watches over them, and courageously defends his nest during the whole period of incubation, lasting from six to nine days.

The Oligodynamics of Living Cells.

A posthumous paper of Carl von Nägeli has just been published at Zurich, with the mysterious title "Ueber oligodynamische Erscheinungen in lebenden Zellen."

Towards the end of the discussion, we read that living cells may succumb to three different forces. Of these the Physical and Chemical are by far the most frequent and fatal; a third, of comparatively rare occurrence, he chooses to term "Oligodynamic." The paper is occupied by an examination of this peculiar power.

The first startling announcement runs "fatal effect of nominally pure water on living cells." Nägeli was led to the conclusion that water possessed this property while testing the reduction of silver salts by living protoplasm. Solutions of the silver salts were made in the "nominally" pure water, either distilled or spring water, and the cells experimented on were those of Spirogyra.

In strong solutions, he noticed that the effect was chemical, the plants dying from poison; the cells lost their turgidity, the protoplasmic layer separated from the wall, and the chlorophyll bands changed colour.

With a solution so diluted that the merest infinitesimal trace of the salt only was present, the result was again death, but after a wholly different fashion; the protoplasmic layer still adhered to the wall, and the cells retained their turgidity, while the chlorophyll bands shrunk together into a round ball. The effect was sometimes very rapid, and in four or five minutes the cell was killed. This, then, is the oligodynamic phenomenon, and, as he discovered at the end of a long research, was caused by molecules of copper present in the water.

The "nominally" pure water was brought through leaden pipes ending in a brass tap. If the water was allowed to flow some time before being drawn for use, it was neutral; the experimenter could thus at will command oligodynamic or neutral water. In the case of

distilled water, the vessels employed in distillation were accountable for the fatal effects.

River, lake, or marsh waters were neutral, but were rendered oligodynamic by the immersion of coins, and again could be neutralised by the addition of some substance, such as sulphur, wool, cellulose, &c., which would expose a large surface, and so attract the copper molecules.

The paper is prefaced by Schwendener, who informs us that Nägeli intended to name the newly-discovered power Isagität, but later he changed it to Oligodynamic.

Professor Cramer, of Zurich, repeated the experiments, and in a most interesting paper gives us his results, which confirm those arrived at by Nägeli.

THERE is a brief and succinct account of the Sandgate disaster, by Mr. William Topley, in the April number of the *Geographical Journal*, which contains some curious references to earlier landslips in the same area, 1716, 1725, 1786. Mr. Topley delivered an interesting lecture on the subject to the Geologists' Association on April 7. A Report of this will be printed in the May number of the *Proceedings*.

IN the *American Geologist* for March, Charles Schuchert writes on the classification of the Brachiopoda. The genera (author and date quoted) are arranged in families, many of which are new, and there is one new genus, *Dallina* (Beecher), founded on *Waldheimia septigera*, Lovén. Following the classification, is a "Geological Distribution of the Brachiopoda," a most useful list, but which would have been of more service if arranged alphabetically, for the alphabet is stable, while systematists have their idiosyncrasies. Dr. C. E. Beecher has assisted Mr. Schuchert in his work.

WE have received, from the University Press of Chicago, the first number of a new "Semi-Quarterly Magazine of Geology and Related Sciences," called the *Journal of Geology*. It is excellently printed on hot-pressed paper, and has seven editors and thirteen associate editors. The first article is one on the Pre-Cambrian Rocks of the British Isles, contributed by Sir Archibald Geikie; most of the remaining space is devoted to questions relating to the glacial geology of America.

A PARAGRAPH has appeared in the daily Press reporting the discovery of coal in Essex. We understand that the report is without foundation, though it is by no means improbable that Coal-measures may occur at a moderate depth in that county.

THE *Mediterranean Naturalist* for March contains several interesting papers relating to the district around Malta. Surgeon-Captain M. L. Hughes continues his "Natural History of certain Fevers occurring in the Mediterranean"; Gaetano Platania publishes some "Geological Notes of Acireale," and Dr. Alf Caruana Gatto writes on "The Vegetation of the House Terraces of Malta."

IN the *Naturalist* for April Mr. John Cordeaux continues his "Bird-notes from the Humber District in the Winter of 1892–1893." The arctic severity of the winter led to a great rush of various species on the East Coast, all wild fowl, such as geese and duck, having been exceptionally abundant.

THE same journal has a curious note by Mr. H. Moody Foster on herrings confined in a brackish-water pond communicating with the Humber. The fish are dwarfed, and are easily captured by rod and line.

MR. E. T. NEWTON will deliver a lecture on the Reptiles of the Triassic Sandstones of Elgin before the Geologists' Association at their next meeting on May 5. The remains, or restorations, of these extraordinary animals will be shown by the oxy-hydrogen lantern.

MR. ROBERT T. HILL, in the *American Journal of Science* for April, gives a description of the Cretaceous formations of Mexico. The geology of Mexico is so imperfectly known that this contribution will be welcome. We observe that the vast mass of blue or grey limestone, known in Mexico as "Mountain Limestone," which forms so important a constituent of the Sierras, is now considered to be entirely of Cretaceous age. True Palæozoic limestones are almost entirely absent from Mexico.

IN the April *Scottish Geographical Magazine* will be found an interesting account of the Nile Valley, its physical geography and geology. The outline is particularly valuable, being written by Colonel Justin C. Ross, who, for several years, was Inspector-General of Irrigation in Egypt, and has had occasion thoroughly to study the subject of Nile deposits and floods. The want of a contoured map, unfortunately makes it somewhat difficult to follow the details as to the areas liable to floods, those in which salt tends to accumulate in the soil, etc. Nothing has recently been done to test the depth of the ancient deposits of the Nile, the boring to a depth of 345 feet at Zagazig, made in 1888, being the latest recorded.

MR. MALCOLM LAURIE deals in the current part (vol. xxxvii., pt. i.) of the Edinburgh Royal Society's *Transactions* with some new forms of Eurypterid remains from the Upper Silurian Rocks of the Pentland Hills. So rich was a particular seam of fine-grained sandstone in these extinct "Sea Scorpions," that no less than five new species, including a new genus (*Drepanopterus*), were found. An interesting feature of these species was the large size of their eyes, and the author hints at a possible deep-sea habitat. It is known that many deep-sea creatures, particularly among Crustaceans, are endowed with abnormally large eyes, "as if to make the utmost possible use of the failing light," which (according to one naturalist, at least) does reach even the deepest abysses of the ocean.

THE Transactions of the London Meeting of the International Congress of Hygiene and Demography, held in 1891, have just been issued in the shape of thirteen volumes. These include numerous papers on Bacteriology and other subjects of interest to students of Natural Science.

IN a letter to M. Daubrée, recently read to the Academy of Sciences, Baron Nordenskjöld has called attention to the wide distribution of certain metals hitherto regarded as rare. His analyses of the ash of an anthracitic substance occurring in large nodules in the old rocks near the iron mines of Norberg and Dannemora, in Sweden, showed the presence of the oxides of nickel and uranium (the latter to the extent of 3 per cent.), and the rare earths of cerite and gadolinite.

THE committee of the Alpine Club are taking steps, at the suggestion of one of their members, Captain Marshall Hall, to collect information from colonies and other countries as to glacier motion and history. Such of our readers as are about to visit glacial districts can much assist. Photographs are especially desirable, of shrinkage or increase, and of other changes, and evidence as to any part taken by ice-action in lake formation. They will find an article upon rough surveying and cognate subjects in the *Alpine Journal* for February, 1891, written by the above-named gentleman, who has consented to deal with the materials which it is hoped will result from the action of the committee.

THE Geologists' Association has planned to visit Bath at Whitsuntide. Excursions are arranged to see the Fuller's Earth works at Midford; the Forest Marble, Bradford Clay, and Great Oolite, at Bradford-on-Avon; and the Corallian Beds and Iron works at Westbury, in Wiltshire. It is also expected that one day will be devoted to the classic region of Dundry Hill.

I.

Natural Selection and Lamarckism.

MR. HERBERT SPENCER still believes in the Lamarckian factor in evolution as strongly as ever. In the pages of the *Contemporary Review* he claims such an "Inadequacy of Natural Selection" as necessitates the introduction of the inheritance of the effects of use and disuse as the only means of accounting for the facts he brings forward. I propose to examine his latest arguments, and to show that they are too weak to bear the superstructure which he erects upon them.

The first proof of use-inheritance[1] that Mr. Spencer advances is the fact of the striking differences in the acuteness of the sense of touch in various parts of the body as estimated by the varying distances at which the points of a pair of compasses can be distinguished as yielding separate and distinct sensations. He urges that these differences could not be brought about by Natural Selection, or survival of the fittest, and, therefore, must be due to the cumulative inheritance of the functional modifications produced by use and disuse in the individual. It is admitted, however, that a widely-diffused sense of touch would be evolved by Natural Selection, as being absolutely necessary for safety and continued existence; and it should be equally obvious that Natural Selection cannot act in an indiscriminately unvarying degree in all parts alike, but would evolve local sensibilities solely in proportion to the varying degrees of the usefulness which causes its action. Mr. Spencer himself admits that the high perceptive power possessed by the end of the forefinger "may" have arisen by survival of the fittest; and there ought to be no difficulty in extending the concession to the other finger-tips, and to other joints of the fingers and parts of the hand where it would prove useful, and where its partial diffusion would be aided by the general principle of correlation. The general distribution of the sense of touch is in accordance with the requirements of Natural Selection.

[1] I employ this term to signify "the inheritance of the effects of use and disuse." Besides being brief and convenient, it also has the advantage of not necessarily including the inheritance of such acquired characters as mutilations—a subject on which the more prudent Lamarckians appear to be rather sceptical. One of them, indeed, has distinctly denied the inheritance of mutilations and injuries.

Tactile sensibility scarcely exists in the internal organs, where it would be worse than useless. It is most delicate in the tip of the tongue, which keenly examines food by touch as well as by taste, and in the tips of the fingers, which serve as tactile and manipulative organs. The face, though including highly important organs, does not need so discriminative a perceptiveness as the tongue and finger, and accordingly possesses much less even in the lips and the tip of the nose, where its tactile sensibility is finest. The body and limbs, which have never needed a highly discriminative sense of touch, have never evolved such keen sensibility, so far as is known, and the Neo-Darwinian has, therefore, no need to adopt the suggestion of degeneracy under panmixia so superfluously made on his behalf. The back of the head and body has less perceptive power than the front, where it has been more needed, since we move in a forward direction, and meet or touch objects in front of us more often than objects in the rear. Great part of our tactile equipment dates back to very remote eras. Tongue, fingers, toes, lips, and nose must have evolved special tactile powers even before our ancestors had arrived at the Simian stage of evolution; and in passing through foliage, branches, and thorny bushes a fairly developed sense of touch in the face would be far more serviceable than in the back of the head, and the chest and limbs might well become somewhat more discriminative than the back.

While the relative sensibility of parts can in the main be accounted for by the Neo-Darwinian, it must be freely admitted that we cannot fully and decisively explain every detail, every minor modification, and every gradation, by the principle of the survival of the fittest. We may be unable, for instance, to explain why the thigh and fore-arm are less sensitive in the middle than near the end, or why the skin over the malar bone is less discriminative than other parts of the cheek, or why the tip of the nose retains a measure of tactile perceptiveness exceeding that of the palm of the hand; but our failure to unravel and clearly demonstrate every obscure cause of the minor facts of variation and evolution proves nothing. Even if we accept Mr. Spencer's view of the inadequacy of Neo-Darwinian factors, we still have to inquire whether use-inheritance will fill the gap, or whether it is equally or still more inadequate to afford the desired explanation. Does it help us to account for the fact that the tip of the nose is four-and-a-half times as discriminative as the back of the hand?[2] Is it reasonable to suppose that the nose comes into contact with objects far more frequently than the back of the hand does? Does it touch things just about as often as the third or lowest joints of the fingers, three times as often as the lower part of the

[2] The numbers in these comparisons might fairly be squared so as to represent the relative number of separate tactile areas in a given space instead of linear distances. Thus estimated, the tip of the nose is *twenty* times as sensitive as the back of the hand.

forehead, and nearly twice as often as the palm of the hand?[3] Is it probable that the palm of the hand touches objects so much less frequently than the immediately adjacent finger joints as to account for its possessing little more than half their perceptiveness? Is the cheek touched twice as often or twice as impressively as the forehead, and nearly three times as often as the back of the hand? Mr. Spencer's hypothesis does not seem to offer any assistance in explaining the facts. While it can be reconciled with them in some respects, it is in direct conflict with them in others. All it can do, indeed, is partly to explain such facts as are already explicable by the natural selection of useful tactile powers. Nothing is gained by the introduction of the Lamarckian factor, which, accordingly, still remains as unproven as before.

Mr. Spencer's explanation rests upon the probably correct opinion that tactile sensibility slightly increases with use, and upon the theory that acquired characters are transmitted to posterity. Now, the greatly increased tactile perceptiveness found in various parts evidently depends, as Mr. Spencer fully recognises, on an increased number of tactile areas possessing distinct and separate communications with corresponding sensation tracts or stations in the brain. Mr. Spencer urges, not merely that practice makes perfect, which would be a legitimate conclusion, but that it actually multiplies the lines of communication with distinct endings or areas in the skin and in the brain. Foster, however, whose authority in matters of physiology is at least as great as Mr. Spencer's, tells us that "The improvement by exercise of the sense of touch must be explained, not by an increased development of the terminal organs, not by a growth of new nerve-fibres in the skin, but by a more exact limitation of the sensational areas in the brain" (*Text-Book of Physiology*, pp. 532, 533, third edition). If Foster is right, how could *extreme* perceptiveness be evolved by the inheritance of a slightly-improved limitation or clearer definition of the partially confused and intermingled areas of sensation? How could the transmission of a more perfect working order explain the formation of large numbers of fresh nerve-fibres provided with fresh sensational organs in the brain, and ending in the skin in corresponding tactile areas which, in the end of the forefinger, must be some 900 times, and in the tip of the tongue some 3,600 times, as small and numerous as in an equal area in the back of the neck? It has always been supposed that increased use caused increased size. In this case it is conveniently, though inconsistently, supposed to produce the opposite effect. The more a tactile area is used the smaller it becomes, until these areas are as minute

[3] If we hold that the discriminative sensibility of the tip of the nose is inherited from ancestral lemurs, which habitually applied the nose to objects as an organ of touch as well as of smell, then the long-continued retention of such seldom-exercised perceptiveness shows that the inherited effects of disuse have been so exceedingly slow and slight as to be altogether problematical.

as in the tip of the tongue, where points only one twenty-fourth of an inch apart can be felt as separate impressions. But is it likely that mere use will diminish the size and multiply the number of the separate structures on which distinct sensations depend? Mr. Spencer leads us to infer that such multiplication had taken place in four cases which he tested[4]; but the assumption that increased use converted twelve tactile areas into fourteen or seventeen has somewhat of the same intrinsic improbability as the idea that a man by using his five fingers sufficiently might convert them into six or eight. Though processes of multiplication by splitting or subdivision are perhaps conceivable, Foster's explanation seems much more reasonable, and the supposed value of the whole case as an inferential proof of use-inheritance becomes correspondingly doubtful.

The next example or proof of use-inheritance to which we are referred is the decrease of the human jaw in civilised races. I have already dealt with this case elsewhere,[5] and have attributed the changes more especially to the combined effects of (1), panmixia; (2), sexual selection; and (3), the natural selection of an economy of structure which would promote lightness and agility besides saving some slight amount of nutriment. *Panmixia*, it seems, is a factor which Mr. Spencer had "excluded as impossible." But it is obvious that Natural Selection may favour large and efficient organs of mastication and strong unbreakable jaw-bones among low savages almost destitute of tools and cookery, living at times on the rudest and harshest food, and often falling victims to violence or accident. If Natural Selection has done this—which I presume will not be disputed—then it is perfectly adequate to cause *some* part of the difference between the jaws of savages and of long-civilised races; and it is only saying the same thing in a slightly changed form when we contend that panmixia, in the form of *non*-selection of large teeth and jaws, is one of the causes of the relative smallness of the teeth and jaw in civilised races. How far panmixia alone can reduce size is a matter not easily susceptible of clearly demonstrative proof, but it is at least obvious that the comparative cessation of selection of large strong teeth and jaws leaves the field more open than before for the action of reducing factors such as economy. If it still be said that Neo-Darwinian factors cannot reduce the jaw, we may point to the decisive fact that the other bones of the skull are distinctly lighter in Europeans than in Australians and negroes, although the skull as

[4] As no measurements were made *before* the increased use, the "clear proof" is clearly imperfect, for there is nothing to show that the sensitiveness of the finger-tips in the two blind youths and the two skilled compositors was not entirely congenital. Compositors capable of becoming "skilled," probably started with a sensitive tactile organisation. Weber's twelfth of an inch, too, is obviously only an approximate measurement, and ought not fairly to be made a standard of comparison for more delicate investigations.

[5] *Are the Effects of Use and Disuse Inherited?* ("Nature" Series.)

measured by its brain-capacity has greatly increased in size.[6] Mr. Spencer cannot possibly say that the thinness and lightness of the bones of the skull are due to disuse, or that the Italian skull has been shortened by disuse. Seeing that Neo-Darwinian factors can reduce skull-bones and soften the frowning glabella so conspicuous in Australian skulls, and that they can also absolutely or relatively shorten the whole skull, why cannot they reduce the jaw-bone and alter its shape and dimensions? Are we to suppose that a skull-bone that happens to be moveable is thereby exempted from the action of all evolutional factors except use-inheritance?

As panmixia and economy must be held to have reduced the weight of the mandible as of other skull-bones, so sexual selection must have modified its shape and size by repressing the brute-like muzzle and large prominent teeth seen in the lower and less humanised races. It has thus favoured the relative prominence of the chin, which is quite as marked a feature in the higher races as the reduction of the teeth and of the more purely dental portion of the jaw.[7] The effect of sexual selection—and of panmixia arising from the survival of delicately-constructed females under male protection and civilisation—is, I think, conclusively shown by the fact that the reduction in female European jaws, as compared with Australian female jaws, is about twice as great as in the case of men.

The reducing and disturbing influence of disuse *during lifetime* is also a further cause of decrease which cannot possibly be denied by the Lamarckian. When these various reducing factors are taken into account, what need is there, or indeed what room is there, for the introduction of the Lamarckian factor? The total reduction in the weight of the male European jaw as compared with that of Australian males, does not seem to be more than 17 per cent., and of this more than a third can be accounted for as being due to a much greater loss of teeth together with consequent alveolar absorption. Another third may fairly be attributed to the Neo-Darwinian factors which had

[6] Excluding the lower jaw, I found that the skulls of European females at the College of Surgeons (mostly Italian) were 12 per cent. lighter than those of Australian females, although the capacity of the cranium had increased by 9 per cent. The weight of male skulls was only reduced by nearly half as much.

[7] The reduction of the teeth appears to be greater than that of the jaw. Dr. Macalister found that the area of the crowns of teeth in Englishmen was over 16 per cent. less than in Australian males (*Nature*, Aug. 18, 1892, p. 380). This, in unworn teeth, would correspond with a reduction of about 23 per cent. in cubic measurement or weight—a much greater decrease than had occurred in the jaws that I weighed. A greater reduction of the teeth is exactly the opposite of what *should* have occurred if the Lamarckian explanation were the correct one—for as the teeth emerge from the gum already formed, and are but little, if at all, susceptible of alteration by subsequent use or disuse, they would either not be modified at all by use-inheritance, or at least would be modified much more slowly than the jaw—a circumstance recognised by Darwin, and often insisted on by Neo-Lamarckians, who say that the relatively rapid reduction of the jaw causes a frequent overcrowding of the teeth.

lightened the rest of the skull by over 5 per cent. in the fifty specimens (mainly Italian) which I weighed. Whatever estimate may be formed of the remaining portion of the reduction in the European jaw, we still have to deduct the effect of disuse during lifetime and the direct or transferred effect of sexual and social selection in repressing prognathism. What grounds can we have for assuming that any residue remains which can justly be claimed as a proof of the inheritance of the effects of ancestral disuse? [8]

In endeavouring to establish the inadequacy of Natural Selection, Mr. Spencer puts forth contentions or assertions which manifestly overpass the bounds of accurate and logical reasoning. He holds, for instance, that if Natural Selection does not cause "a *frequent* survival" of individuals possessing any particular trait, it can "neither cause nor aid" any change in that direction. But surely it is as rigidly inevitable as any arithmetical or mathematical fact that the *occasional* elimination of an unfavourable characteristic *must* proportionally affect the general average. Nay, the elimination of a single individual thus characterised has, *ipso facto*, altered the average among the survivors. There is not the least reason, therefore, for accepting the strangely unphilosophical dictum that an evolutional cause or factor will not act at all unless it acts in considerable or important degrees. We may safely allow science and reason to implant in us a perfectly firm and unwavering conviction that every cause, however weak, will produce its proportional effect; and we may expect that the long-continued and cumulative effect of even infinitesimal forces or tendencies may become perceptible in the course of sufficient time.

[8] Mr. Spencer has unintentionally exaggerated the extent of the diminution. A comparison of his statements (*Principles of Biology*, § 166, footnote) with the skulls which he inspected at the College of Surgeons will prove to any careful measurer that the jaw, which was too hastily assumed to be "an average English jaw," must actually have been the smallest English jaw in the collection—that, namely of a female skeleton (No. 70A) with exceptionally delicate features. Founded on such a basis, his ideas and statements concerning the "dwindling away" of the jaw go far beyond the sober facts. As Mr. Spencer's disciples thus learn to overrate the decrease of the jaw, so also they become apt to underrate the reducing causes. Thus Mr. F. H. Collins (compiler of "An Epitome of the Synthetic Philosophy," an abstract of Mr. Spencer's works and teachings, published with an approving preface from the hand of the master himself) represents the amount of nutriment required by the whole jaw as a grain a day and calls this "a large estimate" (*The Diminution of the Jaw in the Civilised Races an Effect of Disuse*, p. 12). A little arithmetic will show that the proportional share of daily nutriment for the jaws with their muscles, &c., is some sixty times greater than this, without including liquid. Master and pupil alike seriously underrate the extent of variation, and ignore the facts that the reduction in the jaw is largely correlated with a similar reduction in the skull as a whole, and that the reduction of weight to be moved would allow a proportional economy in the bones and muscles and limbs and body that carry the lightened head with its lightened jaw; and all this allows still further and complicatedly cumulative economy in the alimentary, circulatory, respiratory, and food-procuring organs, which only have to provide a lessened amount of duly prepared nutriment for the economised parts. Arithmetically estimated, the case for economy is made to appear hundreds of times weaker than a fair consideration of the relevant facts would justify.

Whatever may be said to the contrary, the "widely accepted" view that "Natural Selection will [tend to] increase *any* advantageous trait" is a sound one; for the very meaning of "advantageous" is that some advantage is conferred, that the welfare of the organism is aided, and thereby its survival and multiplication. Of course, the degree of power or influence of Natural Selection will strictly depend upon the degree of advantage, and in proportion as this is slight the corresponding effect will be slight, and may easily be counteracted or masked by the effects of other causes.

This idea that survival of the fittest can do nothing at all unless it is "frequent," is allied with further misconceptions of the way in which Natural Selection brings about evolutional changes. Mr. Spencer compares each generation with the preceding one and finds that the change is too slight to affect survival. I venture to insist, however, and with all possible emphasis, that the basis of comparison of advantage must *not* be the trivial amount of change occurring in each successive generation, but the much more notable variations seen in contemporary individuals. If individuals who are the most poorly adapted in any respect are occasionally cut off, while the more perfect are preserved, there may well ensue some amount of evolutional change, for the average may thus be altered, although the intermediate or mediocre majority were not appreciably affected by the eliminative and selective process. The differences or variations that appear are far greater than Mr. Spencer would allow us to suppose. Thus, some English jaws are two-and-a-half times as heavy as others, while he only concedes a possible variation of about 10 per cent. for Natural Selection to work upon, and makes this allowance, moreover, appear preposterously beyond the degree of variation which would actually be presented to the operation of selective factors. Such fanciful presentments of cases misrepresent the problem before us, and blind us to the actual methods of Nature, whose selective factors naturally act more decisively upon extremes than upon average mediocrity and the minuter degrees of variation. Those who think that Natural Selection acts only upon the almost imperceptible change in each generation, and that this change is too slight to be thus acted upon, ought logically to give up Natural Selection altogether. If the argument were valid, it is hard to see how any of our organs could have been evolved or influenced by Natural Selection. Even the evolution of our most rapidly-evolving organ, the brain, could not be thus brought about, for such evolution has been exceedingly slow, and the advance in each generation could be represented as much too insignificant to affect survival, besides being neutralised by the advantages of superiorities in other organs, as a Lamarckian might also urge.

Another curious limitation imposed on the potency of Natural Selection is its alleged inadequacy to bring about complex evolution. It is acknowledged that it can "keep all faculties up to the mark"

by destroying such individuals as have faculties in some respects below the mark. Natural Selection is thus credited with an all-round elevating power, for it raises all faculties to a higher level than they would otherwise have fallen to. Why, then, does its power abruptly cease at a certain line? If it can thus lift all faculties up to the mark under ordinary conditions, why cannot it raise them slightly above the mark if those conditions become more stringent? If it previously counteracted a general retreat all along the line, why cannot it now cause a general advance? Will an increase of eliminative and selective rigour produce no extra effect?[9] And if it raises aggregates of faculties, must it not raise the separate faculties of which the aggregates are composed? The complex evolution of many co-ordinated organs simultaneously is, of course, a much slower and more difficult process than the special evolution of a single organ or faculty which becomes exceptionally useful; but difficulty is not impossibility, and we are, therefore, fully at liberty to believe, with Darwin, that Natural Selection alone would suffice to bring about the many co-ordinated changes necessitated by the gradual development of the huge horns of the Irish elk or the long neck of the giraffe. As a matter of fact, complex evolution of special organs and complicated instincts *has* taken place without the help of use-inheritance, and in spite of the strongest opposition it can offer, as is seen in neuter insects, such as bees, and still more in some species of ants, where workers, soldiers, and another distinct caste, apparently of overseers, are descended from innumerable generations of helpless parents who do not develop various organs and instincts which the neuters alone can improve by exercise but cannot possibly transmit to posterity. To hold that Natural Selection is totally incapable of bringing about complex evolution is to hold that the evolution of bees and ants is impossible. Seeing, then, that Natural Selection has evolved the (relatively) huge head and fighting mandibles of the soldier-ant,[10] with all the complex co-ordinated changes involved, there is no reason for supposing that it cannot have effected similarly complicated changes in larger animals, such as the elk and the giraffe.

The influence of beneficial modifications in determining survival is not easily measured. Advantages may often be more decisive than we are apt to suppose. Thus the discriminative tactile sensibility of the tip of the tongue seems to have been of some importance in promoting welfare and survival. It greatly aids the sense of taste in securing fit and proper nutrition—a consideration entirely overlooked by Mr. Spencer. While constantly exploring small irregularities of

[9] This rigour might give itself fuller scope by first increasing fertility as a point of supreme importance or absolute necessity.

[10] Among termites, too, the soldier caste possesses "enormously large, hard, and strong heads, almost as big as the rest of their bodies, and these are armed with gigantic and very strong and sharp mandibles, while the heads and mandibles of the non-combatant workers are much smaller and weaker."

surface, it is minutely ascertaining the texture, so to speak, of the organic or inorganic substances submitted to the ordeal of its keen judgment, and by the general structure and softness of the particles or fibres it determines whether they shall be accepted as digestible food or avoided as injurious or innutritious. It detects and rejects sand, earth, grass, leaves, or other unpromising material with which men from mere laziness, or the pressure of famine, would otherwise gratify their hunger, to the injury of the whole system.[11] It discovers and enables us to remove such objects as fish-bones, hair, feathers, and pieces of grit (which latter, as I have personally experienced, may splinter a tooth) and so saves internal organs from injury by their accumulation or friction, and from waste of digestive activities. The tongue also delicately manipulates the food, and secures its proper mastication by pushing it between the teeth, while carefully avoiding being bitten between the little grinding mills and cutting machines amid which it works so deftly and yet so safely. It is the highly and wonderfully discriminative guardian and janitor of the whole alimentary canal, and the supreme judge of the quality, composition, and admissibility of the nutriment which is the foremost essential of life. If Natural Selection cannot evolve a very special degree of sensitiveness in the tongue, it is hard to see what it can evolve. Mr. Spencer, however, contends that "no advantage is gained" by the explorations of the tongue, that the extreme perceptiveness of its tip is not needed, that use-inheritance furnishes the only possible explanation, and that even if tactile sensibility is of use in detecting fish-bones and foreign bodies among the food, a degree of sensitiveness equal to that of the finger-tip (which is only half as sensitive as the tongue-tip) would have been sufficient. But who can fairly undertake to decide that the higher degree of sensitiveness in an important food-selecting organ would never promote the well-being of individuals so far as to lead to survival where others perished? If, moreover, lower powers occasionally led to elimination while surplus powers did not, there might easily be an evolution of surplus powers—a principle which will help to explain many of our keener and finer powers of eye and ear and brain, and the surplus powers of organs generally.

In all cases of minor advantages, especially those of economy and lightness, it must constantly be borne in mind that the survival test acts more specially and most strongly in occasional crises—such as those of illness, famine, war, accident, and other perils—through some of which all of us still have to pass from infancy onwards. In such cases, exceedingly minute advantages may sometimes turn the scales of life and death. Even now, although the rigour of Natural Selection is so greatly mitigated under civilisation, a slightly healthier, stronger, and better nourished body, due to lessened waste

[11] Some tribes, when hungry or starving, partly evade or deceive this tactile sensitiveness and repugnance by filling their empty stomachs with a fine clay, or a fine, flour-like powder composed of exceedingly minute shells.

of food or tissue, or to other slight advantages, may carry child or man through fevers otherwise fatal, or through physical dangers and hardships, or arduous labours and mental trials under which he would otherwise have succumbed. In the not very remote past, a lighter skull, by diminishing the weight to be moved, and so increasing a man's speed and agility, may have enabled him to save his life by flight from enemies, or to overtake and kill a less swift and agile foe. Considering correlated changes and advantages, and looking at the many and varied tests through which our species has had to pass, at the critical episodes of personal strife, the occasional narrow escapes from natural dangers, and the innumerable diseases, parasites, microbes, &c., that waylay us through life, and search out the weaker points, it becomes almost impossible to say that any advantage has been so minute as never to have affected survival. Under starvation, wounds, disasters, illnesses, men hover on the brink of the grave, and a hair's-breadth of advantage, a single grain of spare nutriment or lessened expenditure, may decide ultimate recovery, while any little disadvantage may prove the last straw that breaks the camel's back. All organisms pass through kindred dangers and diseases of which we know little and care less. As a rule, the lower animals suffer far more frequently and severely from starvation and enemies than man does; they multiply much faster, and they are eliminated with proportional frequency. In view of such facts, we are by no means bound to suppose that small economies can never influence life or death in their immediate or ultimate effects.

Darwin did not disdain to call in the effects of slight causes. He partly attributes the somewhat reduced size of our teeth to the principle of economy,[12] although a tooth is scarcely $\frac{1}{300000}$th part of the weight of a man's body. Mr. Spencer, however, thinks that we ought to feel certain that economy, panmixia, and reversed selection, all put together, cannot reduce the useless, exposed, and sensitive eyes of the proteus and other "blind fish and amphibia" living in dark caves. We are told, indeed, that "an economy of $\frac{1}{2000}$th of the creature's weight could not appreciably affect survival" (p. 166).[13] But if Natural Selection were really incompetent to secure economy in small parts, it could not secure economy in the body as a whole.

[12] *Descent of Man*, p. 562.

[13] Such an economy is equivalent to the saving of three-quarters of a pound in man. Many circumstances are conceivable in which death might be avoided and multiplication be favoured by slightly increased facility and quickness of motion, by surplus nutriment, and lessened liability to various dangers. Disused parts retain, or, apparently even increase, their share of liability to tumours, ulcers, and other diseased conditions which occasionally prove fatal by their direct or indirect results—of which many examples are known in man, the only animal with whose pathology we are really well acquainted. Probably, too, as Ray Lankester points out, there was a preliminary selection of animals whose imperfect eyes led them into the sheltering darkness of the cave, and prevented them from straying from it.

If no saving can be effected in the small separate fibres that make up muscle and nerve—if economy cannot be brought about in the separate cubic inches, or ounces, or grammes, of the bodily structure, how can it possibly be effected in the whole body, made up of those separate inches, or ounces, or grammes? Mr. Spencer allows that Natural Selection, and, therefore, panmixia also, can act upon aggregates; and he is, therefore, logically bound to admit that they can affect the minute parts of which aggregates are composed. The changes are not necessarily as gradual as Mr. Spencer represents them. Greatly diminished, or almost aborted, eyes will occasionally appear, and the possessors, no longer eliminated through failure of vision, will diffuse the degenerative tendency among the species by intercrossing.

Fortunately, the question of the possibility of minute reduction and minute economy in the shape and weight of organs, without the aid of use-inheritance, can be settled by a simple and direct appeal to facts. The advice of Solomon, that we should go to the ant to learn wisdom, is peculiarly applicable to the Neo-Lamarckian. He can test his arguments by an appeal to Nature herself. In neuter insects we are furnished with a happy opportunity of excluding use-inheritance from the problem. Now, in neuter insects, the effects assumed to be impossible have actually occurred. Neuter ants of various species possess eyes in all manner of grades of imperfection and smallness down to the most rudimentary, and even to the complete loss of the eye, of which only a trace of the socket remains. This, like the loss of wings, cannot be due to the inheritance of acquired characters, since the neuters cannot transmit their acquired characters to posterity, and the actual parents still retain the eyes and wings necessary for the nuptial flight. What, then, is the use of elaborately arguing that accomplished facts are impossible? It is the ways and works of Nature that we must study. She cares nothing for the theories of philosophers and their fancied impossibilities. She calmly *does* the thing declared impossible; in neuter insects she reduces and economises on the minutest scale without use-inheritance; she neatly and fitly develops, with all due co-ordination and economy, the special organs of neuter insects, together with wonderful mental faculties, and complex social instincts, which are never exercised, or, indeed, possessed, by the parents. Why, then, should we be asked to believe that, without the help of use-inheritance, minute economy and complex evolution are incredible? And what argument can possibly show that processes which certainly take place in innumerable species of ants, bees, termites, and wasps cannot take place in larger organisms where we do not happen to possess any special means of demonstratively separating the effects of Neo-Darwinian factors from those attributed to the transmission of acquired characters?

With reference to the arguments advanced against Weismann, I may say briefly that, although the bodily, or somatic, elements which

shelter and nourish the reproductive elements can produce some kind of effect upon them, and though reproductive elements may persistently affect other reproductive elements, as in the case of Lord Morton's mare, yet such and similar facts do not in the least decide the question whether the cells, or units, of the bodily structure can affect the reproductive substance, or germ-plasm, in such a way as to convey their particular acquired modifications, or changes, to posterity.

A few words are also necessary in answer to the claim that since congenital characters are heritable, the *onus* of proving that acquired characters are "not inheritable" falls upon the Neo-Darwinian. The implication that the one case affects the other cannot be sustained. It is perfectly certain and perfectly well-known that there is a wide difference between the two classes of characters, and that they are not comparable in their degree of effect upon heredity—otherwise there would be no more dispute concerning the one class than concerning the other. No one questions the immediate and decisive heritability of *congenital* mutilations and malformations, or of any other congenital peculiarity, however striking or minute. On the other hand, it is a matter of general experience that the loss of a tooth, or eye, or limb, or finger-joint, is no more transmitted to offspring than a sunburnt complexion, or a clipped beard, or a knowledge of Latin or cookery. In proportion as the nature of each case admits of clear decisive proof, we see that acquired characters are *not* inherited; no indisputable evidence to the contrary has ever been given. The presumption arising from the *proven* facts, therefore, is that non-inheritance of acquired characters is the general rule, and the *onus probandi*, therefore, rests with those who assert the contrary. We cannot prove a universal negative; and it is not easy to find or suggest absolutely decisive tests of the admittedly slight power of use-inheritance, or of the size-reducing power of panmixia. The effect of use-inheritance on offspring is imperceptible. Darwin acknowledges that several generations must elapse before any appreciable result follows;[14] and during this interval other factors would be at work. We perceive the apparently insuperable difficulty, or practical impossibility, of excluding all other factors in such delicate and complicated cases. But if the Neo-Lamarckian will accept his own cases, and allow us to adopt his own assumption that extraneous factors are adequately excluded, we can show that the effects of use and disuse are *not* inherited in some features just as conclusively (or inconclusively) as he shows that, in other respects, they are inherited. If, for instance, use-inheritance is proved by the shortening of the less used wing-bones of the domestic duck, or the leg-bones of the rabbit, then it is disproved by the thickening of these bones; and, conversely, if it is proved by the marked thinness or lightness of the wing-bones of the

[14] *Variation of Animals and Plants under Domestication*, vol. ii., p. 288.

domestic fowl, it is also disproved by a non-shortening, or extremely slight shortening, which is most conspicuous in the breeds that never fly. But the Neo-Lamarckian will probably spike his own guns rather than allow us to use them against him; and we must allow him to do so, if he chooses. The one thing clearly provable is the excessive dubiousness of a factor which is so strangely uncertain in its action, or is so weak as to be easily overruled in some of its effects by causes so trivial and obscure that their interference was never suspected.

We have seen that the theory of use-inheritance is unnecessary, since Natural Selection and other non-Lamarckian factors are evidently capable of effecting such changes as have been adduced. When we further consider the many difficulties and improbabilities involved in the hypothesis—the suspicious lack of satisfactory evidence—the evidence to the contrary which can be drawn even from an examination of the details of the very cases presented as proofs—the ease with which the effects of Natural Selection and panmixia are attributed to use or disuse—the certainty that mutilations are not inherited even when (as in the case of the hymen) they have been repeated for innumerable generations—and the extreme difficulty of accounting for transmission of acquired characters except by the overwhelmingly cumbrous and improbable method of pangenesis—we may well come to the conclusion that the inheritance of acquired characters cannot safely be admitted as a factor of evolution unless much better evidence is produced than has hitherto been forthcoming; and until such proof of the Lamarckian factor is obtained, we can have no right to rely upon a mere hypothesis which biologists are fast rejecting as a mere figment of the human imagination. And if the growing opinion of scientific men is correct, Mr. Spencer's psychology and many cherished but illusive expectations of continued progress by education alone will prove to be but castles built on sand, and will have to yield due supremacy to selective ideals and methods corresponding with the fundamental laws of evolution and of mental and moral progress—a position which by no means excludes the beneficent effects of education and parental influence, although it rejects the delusive and mischievous belief in the alleged inheritance of the changes thus produced.

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II.

Biological Theories.

IV.—SUPPOSED AUDITORY ORGANS.

THREE groups of phenomena appear to have been imperfectly distinguished in the minds of those who have ascribed auditory functions to the organs known as tentaculocysts in jelly-fishes, as otocysts in mollusca and some crustacea (*e.g.*, *Mysis*), as auditory sacs in lobsters, crayfishes, etc., and as auditory hairs or setæ in the same and other crustaceans.

The groups of phenomena are those associated with the transmission of sound-waves in air, of sound-waves in water, and of waves of other kinds in water. The nature of the confusion will be seen by a consideration of the following quotation from Sir John Lubbock's "Senses of Animals":—

> "Hensen took a *Mysis*, and fixed it in such a position that he could watch particular hairs with a microscope. He then sounded a scale: to most of the notes the hair remained entirely passive, but to some one it responded so violently and vibrated so rapidly as to become invisible." "*That these plumose hairs then really serve for hearing may be inferred . . . . . from the observed fact that they respond to sound-vibrations.*" "Hensen's observations have been repeated and verified by Helmholtz." (*Op. cit.*, pp. 93, 94).

The portions of the passage which I have omitted refer to the existence of nerves in relation with these hairs.

Two criticisms are at once suggested. In the first place, the inference is unsound. Because the human eyelashes "respond to sound-vibrations," it is not safe to infer that they "really serve for hearing," and it is not any more safe to infer from the "observed fact" that certain plumose hairs in Mysis "respond to sound-vibrations" that these serve for hearing.

In the second place, there appears to have been a misapprehension as to what constitutes a sound-wave in water.

If a sounding tuning-fork be pressed against the side of an aquarium, two kinds of waves are produced in the water, surface-waves and sound-waves. The surface-waves are visible to the naked eye and travel slowly across the surface, that is, sufficiently slowly to be followed by the eye. Each of these is a *movement* of the water,

partly up-and-down but partly in other directions. The details of this wave-movement will have to be considered in a future article, and it is only necessary here to recognise that the wave consists of an obvious movement; that its velocity is small enough to enable the progress of the wave to be watched; that it is transmitted only through a superficial layer of the water, the thickness of which varies with the intensity of the disturbance; and, finally, that its propagation depends upon gravitation and surface-tension.

The sound-waves agree in number with the surface-waves, and there the agreement practically ends. These waves are waves of compression rather than of movement; their rate of transmission is about a mile a second; their propagation depends entirely upon the elasticity of the water, that is, the property by virtue of which the water resists voluminal compression, and regains its original volume after compression; the movement of the water involved in the transmission of such waves, even when the sound is of explosive intensity, is far too minute to be seen; this minute movement is, moreover, strictly to and fro in the direction of propagation of the wave; the wave is transmitted not through a superficial layer only, but in all directions through the water.

If the aquarium be divided into two parts by a water-tight partition of wood, glass, iron, or other solid, the surface-waves will be stopped by this partition, while the sound-waves will pass unimpeded through it.

Sound-waves in air differ from those in water in the amount of movement of the particles transmitting the wave. Here, also, the wave is one of pressure rather than of movement, though movement ten thousand times as great as that in an aquatic sound-wave is involved.

Some idea of the minuteness of these movements may be obtained from the determination made by Lord Rayleigh and recorded in Ganot's "Physics." The note experimented upon was f^{IV} produced by an organ pipe. The intensity was such that the note was audible at a distance of more than half-a-mile. The movement of each air-particle was less than one two-hundred-and-fifty-millionth of an inch, *i.e.*, little more than one five-hundred-millionth of a wave-length.

Water is only one ten-thousandth part so compressible as air, and a like variation in pressure would, therefore, only involve in water a movement of each particle of about one five-billionth of a wave-length.

Whether Hensen's experiments were performed in a vessel sufficiently large for the accommodation of a series of waves each five billion times the length of the observed movements of the hairs or not, I cannot say,—the Atlantic Ocean would be a vessel of dimensions suitable for the experiment. What the notes were to which the hairs "responded" I do not know—suppose, however, the lowest

note which is audible to man to have been one of them—*i.e.*, a note produced by sixteen vibrations per second, or thereabouts. In water this note has a wave-length of half-a-furlong, *i.e.*, nearly four thousand inches. One five-billionth of this is one one-thousand-two-hundred-and-fifty-millionth of an inch ($\frac{1}{1250000000}$ inch). This can hardly be the length of the "violent" vibration which is said to have rendered the hairs invisible. Suppose the language used in describing the result to be not greatly exaggerated, we may safely say that a movement at least one million times greater than this was referred to. This is, however, the greatest movement producible in water by the passage of any audible note when the intensity is such as to render the note just audible at a distance of half-a-mile. The employment of notes of this low pitch, and of higher intensity than this, would be fraught with great danger, for the apparatus of the middle ear of man could hardly stand much more. To reach the intensity required for the production of the above result, a steam "devil" such as is used on large steamers would be required, and would have to be close to the ear of the observer who was watching the hairs under the microscope. The man who would risk putting his ear within a foot or two of such a source of sound when this was put into operation to the utmost limits of its powers would indeed be a bold man. I doubt, however, if the most powerful of these "whistles" ever made has ever been heard at a distance of thirty miles. Supposing $\frac{1}{1250}$ of an inch to be the extent of the violent movement rendering the hairs invisible, *i.e.*, a little over twice the diameter of a red blood-corpuscle, then the intensity of the sound required, supposing the note to be C_{IV} (=16 vibrations per second), would be such as to render it audible at a distance of 500 miles in air, and much further in water. Supposing it to be at such distance from the head of the observer as to have an intensity equal (in air) to that operating (in water) on the hairs, then the air in contact with his head would move with an amplitude of about eight inches, and with a force which no human skull could withstand for a moment. A microscope would be instantly destroyed by it. I doubt whether even any ordinary building could resist its destructive power, and do not for a moment believe it possible to construct an instrument capable of "sounding a scale" with such intensity as this.

We may, therefore, in spite of the authority of so eminent a physicist as Helmholtz, rest perfectly assured that the effects described have never been either produced or repeated or verified by either Hensen or anybody else. It is, however, not the record of the experimental results which is at fault, but the explanation of them. Vibrations in the water produced by the sound-waves in air, or by vibrations communicated from the source of sound through the table, microscope, etc., have been mistaken for sound-waves in water.

It is, perhaps, inaccurate to assume, as I have done, that the

intensity of a sound (whatever its pitch) is proportional to the range of pressure through which the transmitting medium passes during the transmission of the wave. Making allowance, however, for this error, the result would be the same. The intensity of wave, in water, necessary to produce a vibration of one-thousandth of an inch would in air produce a movement of about ten inches, and would involve changes of pressure which the human skull could certainly not resist, whether the walls of the building could or could not.

"Otocyst" is a name given to a sac, usually microscopic, lined with a sensory epithelium, usually, if not always, provided with sensory hairs. The sac encloses an "otolith," that is, a mass of some substance (usually calcareous) denser than the surrounding water or the tissues of the otocyst. The otolith may be replaced by several masses of some dense solid.

These, as the name "otocyst" signifies, are regarded, if not by all, at least by most zoologists, as auditory organs, and their action is said to depend upon the jolting of the hairs against the otolith or (which is worse!) *vice versâ*.

Perhaps what has been written above may convince zoologists that the production of any such jolting by an *aquatic* sound-wave is a physical impossibility. It is barely conceivable that any wave should involve pressures so great as to reduce the volume of any mass of substance to absolutely nothing, that is, to cause the matter of which it is composed to cease to exist. No pressure short of this would, however, bring the otolith into contact with the sensory hairs in question, even if they were, to begin with, at an infinitesimal distance. The fluid pressures we have to do with act equally in all directions. No movement, except in the direction of the wave-propagation, is produced: there can be no flowing outwards to the sides of even an infinitesimal amount of water which was between the tip of the hair and the otolith, for the pressure at the sides is equal to that between the hair-tip and the otolith.

Whatever the function may be of such organs as otocysts, tentaculocysts, auditory sacs, auditory hairs of marine crustacea, "ears" of fishes devoid of air-bladders, or, in fact, of any organ whatever, in any aquatic animal, that these organs, unless they be associated with a cavity containing a gas, should serve as auditory organs is simply a physical impossibility. An auditory organ would, moreover, be useless to such an animal as a jelly-fish, and the evolution of such organs in such animals could not be explained by Natural Selection. A real function of some of these organs will be treated of in a future article.

C. Herbert Hurst.

III.

The Fruit-Spike of Calamites:

A CHAPTER FROM THE HISTORY OF FOSSIL BOTANY.

FOR many years there has been much doubt and uncertainty among palæobotanists as to whether or not the fossil spike now known as *Calamostachys Binneyana*, Schimp., is entitled to be recognised as the fruit of *Calamites*. It was so regarded by Carruthers (1) in 1867 and by Schimper (2) in 1869, and Binney (3), who described it in 1868 as the fruit of *Calamodendron commune*, was practically of the same opinion, since the plant so named by him is one of the forms of *Calamites*. Williamson (4), on the other hand, has persistently maintained that its affinities are rather with the Lycopodiaceæ, and that the only spike entitled to rank as the fruit of *Calamites* is the one described by him in 1869, and more fully in 1888, and which is very different in many respects from *Calamostachys Binneyana*.

Recently the present writer has had an opportunity of studying a fine series of new preparations of the spike, belonging to Mr. W. Cash, of Halifax, and in a paper shortly to appear in the *Proceedings* of the Yorkshire Geological and Polytechnic Society, he claims to have fully established the fact that it is the spike of some form of *Calamites*. The time seems opportune, therefore, for a brief historical retrospect of the treatment accorded to *Calamostachys Binneyana* by those palæobotanists who have endeavoured to determine its affinities from a study of its structure.

As described by Carruthers in 1867, *Calamostachys Binneyana* consists of an axis, around which are placed in close array a number of whorled appendages. These appendages are of two kinds, whorls of sterile bracts alternating regularly with whorls of stalked structures carrying sporangia containing spores. The latter have peltate heads, and closely resemble the sporangiophores of *Equisetum*—so closely, indeed, that were the alternate whorls of sterile bracts suppressed or replaced by structures of the other type, the spike of *Calamostachys Binneyana* might well be taken for a spike of *Equisetum*.

The specimens at the service of Carruthers appear to have been imperfect and not well preserved, as the minute anatomy of the spike

is only partially given. Among other details, however, it is stated that the axis had "a bundle of fine scalariform tissue in its centre, forming about a third of its diameter, and generally appearing free from the surrounding cellular tissue, which is composed of somewhat elongated cells."

Throughout his description, Carruthers appears to have had no doubt whatever that he was dealing with the fruit of *Calamites*. In this he was probably guided by the great and undoubted similarity of the general characters of the spike to those of *Equisetum*, with which *Calamites* has always been held to have a close relationship. At any rate, he does not seem to have considered that the anatomy of the axis as he then understood it, *i.e.*, with scalariform tissue, and not parenchyma, in the centre, was an insuperable difficulty in the way of a determination supported by so many of the other characters of the spike.

Binney's description of *Calamostachys Binneyana* was published in 1868, and though his account is very unsatisfactory from the botanist's point of view, it implied the presence of a vascular bundle in the centre of the axis, as described by Carruthers. In spite of this, he accepted the view that the affinities of the spike were with *Calamites*, and does not seem to have considered it necessary to explain on this assumption the presence of the axile vascular strand.

In 1871 Williamson began that series of memoirs on "The Organisation of the Fossil Flora of the Coal-measures" which have done so much to increase our knowledge of Carboniferous plants. The first of these deals with *Calamites*, and incidentally the question of its relationship to *Calamostachys Binneyana* is referred to. It is pointed out (5) that in *Calamites* the centre of the stem is occupied by a purely cellular pith with a vascular zone at its periphery, while in *Calamostachys Binneyana* these conditions are reversed. Hence the inference is drawn that if the latter be the fruit-spike of the former, then in passing from one to the other the tissues must undergo a metamorphosis which is without parallel among living plants. In the fifth memoir (1874) the spike is described afresh, and many new details of structure are given with respect to it. Discussing its affinities in the light of the fuller knowledge, the author says (6):—

"After balancing these various facts and arguments, I am led to the conclusion that *Calamostachys Binneyana* has much closer affinities with *Asterophyllites* than with *Calamites*. With the latter it has no one feature in common. There is no solitary point in which the two plants resemble each other. The resemblance of the fertile sporangia of *Calamostachys* to those of *Equisetum* has been combined with the foregone conclusion that the *Calamites* were Equisetaceous plants, in leading to the belief that the two were parts of the same plant; but I cannot conceive of any conditions in which the stem of a Calamite could be prolonged into that of a *Calamostachys*. I have carefully investigated the relations which the fertile stems of the Equiseta bear to axes of their terminal fruit-spikes, and I find that their respective structures are typically identical. The transition from the stem to

the fruit-axis produces no structural changes save such as are of the most trivial kind; the general type remains unaltered and continuous. But to plant *Calamostachys Binneyana* upon the top of a Calamite would be as abnormal as to surmount the stem of an *Equisetum* with the strobilus of a Lycopod."

Thus, nearly twenty years ago, Williamson took up the position that, notwithstanding the close general similarity between this spike and that of *Equisetum*, the internal anatomy of the axis was so different from that of the stem of *Calamites* that its affinities could not be with the latter plant, but were rather with *Asterophyllites*. At that time he regarded the plant he described as *Asterophyllites* as a somewhat aberrant member of the Lycopodiaceæ (7), so that he virtually placed *Calamostachys Binneyana* in the same group.

In subsequent memoirs he has returned again and again to the affinities of the spike, but always with the object of strengthening the position taken up in the above extract. In the ninth memoir (1878) he expresses (8) surprise that Mr. Carruthers should still continue to believe in its Calamitean character, and points out that "both M. Grand' Eury and Dr. Dawson have fallen into the accidental error" of making him "regard the *Calamostachys Binneyana* as belonging to *Calamites*," whereas he has "most strongly opposed that idea." Two years later (1880) fresh specimens of the spike were described by him (9), with the result that they are said to have "fully confirmed the conclusions" arrived at in 1874, "that *Calamostachys Binneyana* had not the slightest relationship with the *Calamites*, but that it had strong affinities with *Asterophyllites* and *Sphenophyllum*." The following year (1881) is remarkable for the discovery of a new form of *Calamostachys*, which Williamson appears at first to have regarded as identical with *Calamostachys Binneyana*, but which he subsequently named *Cal. Casheana*, after its discoverer, Mr. W. Cash, of Halifax. After a detailed account of its structure, in which great stress is laid on the fact that it contains two kinds of spores, he says (10):—

"It is scarcely necessary to say that this discovery of macrospores and microspores in *Calamostachys Binneyana* supplies another link connecting this strobilus with the Lycopodiaceæ in the same measure that it separates the fruit from the Equisetaceæ. . . . This discovery strengthens my old conviction that the true affinities of this strobilus are with the Lycopodiaceæ."

Finally, in the fourteenth memoir, which appeared in 1888, we have his last pronouncement on the subject, which is as follows (11):—

"After a prolonged conflict the conclusions of those who have insisted upon the cryptogamic character alike of *Calamites* and *Calamodendron*, have met with an extensive, though not universal, acceptance. Meanwhile, both the opposing schools of palæontologists recognise the importance of discovering the fructification of these plants. Mr. Carruthers believed that he had found it in examples of *Calamostachys Binneyana*, and Mr. Binney arrived at a similar conclusion. I have always rejected these conclusions because of the conspicuous differences between the morphology of the Calamitean twig and that of

the axis of the *Calamostachys*. These differences appeared to me too great to make it possible for the one ever to have been a prolongation of the other."

Thus up to his latest utterances on the subject Williamson refused to recognise *Calamostachys Binneyana* as the fruit of *Calamites*, and justly insisted upon the difference between the anatomy of its axis and that of the stem of *Calamites* as being strong evidence against such a view. He even went further than this. So far back as 1874 he maintained (12) "that the only British strobilus of which the internal organisation has hitherto been described, that has any claims to be regarded as the fruit of *Calamites*," is that which he figured and described in the *Transactions* of the Literary and Philosophical Society of Manchester in 1869. Substantially the same statement is made in the memoir of 1888 already referred to, so that on both positive and negative grounds he refused to recognise any connection between *Calamostachys Binneyana* and *Calamites*.

Such was the state of opinion among palæobotanists when the new and much better-preserved specimens already referred to came into the writer's hands. Looked at from the botanist's point of view, it is obvious that the divergence of opinion is based upon the fact that there is a sort of inconsistency, if the expression may be allowed, between the external characters of the spike and the internal anatomy as hitherto understood. As already pointed out, the former have so close a resemblance to those of an *Equisetum* spike, that one naturally looks for the parent plant of *Calamostachys Binneyana* among those fossil forms most nearly allied to *Equisetum*, and by a short step we come to *Calamites*; but, on the other hand, the internal anatomy as described by Carruthers, Binney, and Williamson, is so irreconcilable with the anatomy of the stem of *Calamites* as to justify much that the last authority has written in opposing the Calamitean affinities of the spike.

An examination of the new material gave what the writer thinks is a complete solution of the difficulty thus presented, and at the same time indicated in what direction to look for the reconciliation of these diverse opinions.

In the first place, it soon became evident that the centre of the axis of *Calamostachys Binneyana* was not vascular, as had so frequently been stated, but was composed of cellular tissue which constituted a true pith. In 1891, when drawing up the first part of an Index to his Memoirs, Williamson introduced a brief footnote (13) in connection with *Calamostachys Binneyana*, which shows that he was beginning to have doubts on this point, and he admits that in the specimens described in 1874 "the elongated medullary cells" were "mistaken for tracheids." He says nothing, however, as to whether this parenchyma is a true pith, such as we find in *Calamites*, or merely a central parenchyma like that found in the stele or vascular cylinder of some *Lepidodendra*. On this point, however, the sections referred

to are very explicit, and leave no doubt that the former alternative is the true one.

In the next place, it became clear that the axis differed in a much more important point from the published descriptions of it, and that in a direction which had not been previously suspected. In fact, the specimens proved to demonstration that the cellular pith is surrounded by a ring of primary vascular bundles, which, in essentials, are of the true Calamitean type. The number of these is usually three, though there may be four or more, but, whatever be the number, each is characterised by the presence, at the apex of the xylem, of a carinal canal homologous with the canals found in the same position in the stems of *Calamites* and *Equisetum*, and which represent the tracheal initial strands of the primary vascular bundles. In the transverse sections, we find individual elements clinging to the sides of the canals, and projecting into their cavities, reminding us at once and forcibly of the appearances met with in *Equisetum*. So close is the resemblance, that one might from the transverse sections alone infer that these projecting elements are annular or spiral vessels, but for demonstrative proof recourse must be had to the longitudinal ones. Fortunately, some of these have been made in such favourable directions, and the tissues are so well preserved, that they leave nothing to be desired. They show us the carinal canals cut throughout their whole length, with the annular vessels still attached to the peripheral walls.

Hence it is obvious that the stele or central cylinder of the axis of *Calamostachys Binneyana* has a structure which is identical in essentials with that found in the stem of *Calamites*, and there is no such antagonism between the anatomy of the two as has hitherto been believed. Not only so, but when, as sometimes occurs, the formation of secondary xylem begins, it does so first at the carinal canals, as in *Calamites*, and subsequently in the intervening areas. In this way is brought about the triangular form of the pith, and the triquetrous arrangement of the vascular laminæ which surround it, and which Williamson compared with the secondary xylem of the plant he named *Asterophyllites*.

The conclusion, then, is, that the correspondence between the anatomy of the stem of *Calamites* and the axis of *Calamostachys Binneyana* is so close and definite as to leave no doubt whatever that the one is the fruit-spike of the other. Williamson's objections to this determination have all been based upon the supposed absence of such a correspondence, and now that it is shown to exist they naturally fall to the ground. The internal anatomy of the spike, then, points to the same affinities as the external characters, and together they supply all the evidence that is needed to establish the conclusion we have stated. To which of the forms of *Calamites* the spike is to be more particularly referred cannot at present be definitely stated, but there are some reasons for thinking that it must be sought

among those of the type of *Arthropitys*. This, however, is a matter of secondary importance compared with the fact that, after so many years of controversy and doubt, we have, at last, got clear and conclusive evidence that *Calamostachys Binneyana* is the fruit-spike of some form of *Calamites*.

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THOMAS HICK.

IV.

The Succession of Teeth in Mammals.

THE question as to the mode of origin of the successional teeth of Mammalia is one which has at various times received a considerable amount of attention from anatomists, and one which it may be of some interest to review in the light of recent researches by Dr. Kükenthal.[1]

In many of the Mammalia, as is well known, there are two sets of teeth, the first, or milk teeth, which are usually deciduous, and followed by a second more permanent set. In numerous reptiles and fish, on the other hand, we find a perpetual succession of teeth. Instead of the sets being limited to two, there is a constant replacement; as fast as one series of teeth wears away a new series appears to take its place. Moreover, in these reptiles and fish the replacement is also more complete than in Mammalia, for whereas in the latter we never find the whole series of teeth replaced, the true molars being never preceded or followed by others, in the fish and reptiles, when there is replacement of one there is replacement of all the teeth.

When the contrast between this diphyodont condition of Mammalia and the polyphyodont one of Reptilia is realised, the question most naturally arises, is the diphyodontism of the one a relic of the polyphyodontism of the other, or is it to be regarded as an advance from a monophyodont state, one in which a single set of teeth alone was possessed?

Two years ago much might have been said in favour of a primitive monophyodont condition by an appeal to the simplest Mammalia. In the first place, in the toothed whales, the Mammalia possessed of the most reptilian type of teeth and jaw, only one set of teeth was known. Again, in the Monotremes there is no replacement, and, indeed, the teeth which do exist are rarely used, being generally concealed beneath horny plates. Lastly, in the Marsupials only one tooth in each jaw, the posterior premolar, is ever replaced, and this seemed to give us the starting point for the more complete change occurring in higher forms.

Dr. Kükenthal's recent work has, however, invalidated two of

[1] *Ann. Mag. Nat. Hist.*, 1892.

these arguments, and in April of last year an authority as well qualified to speak on the subject as Mr. Oldfield Thomas, declared in favour of a diphyodont condition being the most primitive one in Mammalia—a condition, moreover, which was probably derived from the polyphyodontism of Reptilia.

The first argument drawn from the Odontocetes, the toothed whales, is shattered by the discovery of an embryonic series of teeth within those which later pierce the gum to persist as the teeth of the adult. In an advanced embryo of a dolphin (*Phocæna communis*), possessed of the twenty-five teeth in each mandible, rudiments of a second series were found internal to these. Kükenthal accordingly pronounces the teeth which are present throughout the life of these whales as representatives of the deciduous teeth of other Mammalia, while the uncut rudiments would correspond with the adult dentition of higher forms. These rudiments are, although small, perfectly recognisable as teeth, and are possessed of a cap of enamel and an inner core of pulp.

It was the Marsupials, however, that supplied the chief arguments to Sir W. H. Flower and Mr. Oldfield Thomas as to the secondary character of the diphyodont condition in Mammalia, and the most important part of Kükenthal's work on this subject is, therefore, the discovery of an almost complete series of successional teeth in some of the opossums.

In his classical paper of 1887, Mr. Oldfield Thomas,[2] after a considerable discussion of the teeth of Marsupials, arrived at the conclusion that originally all Mammalia were wholly monophyodont, the permanent teeth having ancestral priority over their ontogenetic predecessors of the deciduous series.

To account for the supposed secondary development of the deciduous dentition, it was assumed that a retardation of the permanent teeth would be beneficial by preventing overcrowding before the mouth was fully grown, while at the same time some provision of masticatory organs for a young animal with its teeth thus retarded would be essential. This advantageous retarding of the teeth, together with the necessity of finding some temporary grinding apparatus, was supposed to have resulted in the formation of a deciduous set of teeth which are of greater or less complexity, and retained for a longer or shorter time in different members of the Mammalia.

From the fact that only one tooth is replaced in the Marsupials, it was further supposed that the Mammalia had reached a fairly high degree of complexity before the necessity for deciduous teeth became urgent, since they could only have possessed one deciduous tooth when they had already gained all the complications of a generalised Metatherian condition. It was considered that at this juncture the

[2] *Philosophical Transactions* of the Royal Society, 1887.

Marsupials branched off from the main Mammalian stock and obviated their difficulties in other ways than by the development of a complete temporary dentition.

At the time of propounding this lucid and carefully elaborated theory, Mr. Oldfield Thomas pointed out that it would entirely collapse should a more complete replacement be discovered among any of the Marsupials, and he has been the first to acknowledge that this has now been done by Kükenthal.

Years ago Professor Huxley stated his belief that the deciduous dentition now found in Marsupials was but a relic of a fuller milk dentition, instancing an analogous loss in the case of the shrew, which has no deciduous teeth, while its near ally, the hedgehog, has a complete replacement. At the time, this belief seemed barely tenable, since in no Marsupial had any trace of a larger number of deciduous teeth been found, while even the one present in most was known to be absent in some forms (*Phascogale apicalis* and others). Moreover, fossil Marsupials, such as *Triconodon*, show the replacement of the posterior premolar, and of that tooth only. This, however, only affords one more instance of the danger of generalising from purely negative results, for Huxley's belief has now been amply justified by Kükenthal's discoveries, and diphyodontism is proved to have been the ancestral condition of at least the Didelphydæ.

It is, of course, possible that a complete cycle of development has been undergone, the milk dentition at first appearing as a provisional masticatory apparatus and afterwards persisting while their phylogenetic predecessors underwent abortion. There is, however, little or no foundation for such a suggestion; a far simpler explanation is to regard both series as representatives of two consecutive series of the Reptilian teeth.

In making this supposition, we must, moreover, grant some probability that the true molars were at one time subject to replacement, and this hypothesis also receives confirmation from the recent researches on the Marsupials.

Kükenthal appears to have been first led to work at the embryonic dentition of the Marsupials from the consideration of the synchronous development of the deciduous posterior premolar and the remaining permanent premolars. This suggested the idea that all the premolar teeth belonged to the same series, and Kükenthal has succeeded in proving that this is the case by the discovery of rudiments of an almost complete series of successional teeth.

These embryonic rudiments were found, not only for the incisors and canines, but also for the most anterior premolar, and for the first and second teeth of the true molar series. The only rudiments not found were, therefore, those of the second premolar and of the more posterior molar teeth. Further, these vestigial rudiments of all the successional teeth are identical in structure with the developing third premolar, which is afterwards cut, replacing its deciduous predecessor.

The only possible conclusion to be drawn is that in *Didelphys* (and probably in other Marsupials) the teeth of the permanent dentition, with the exception of the last premolar, represent the deciduous teeth of other forms, while rudiments of a second series are present in the embryo, but only cut the gum in the case of the third premolar; these rudiments must obviously be the remnants of a more complete replacement.

If the two series of teeth in the Mammalia are to be regarded as relics of the many series in the Reptilia, the Marsupials, inasmuch as they have almost lost one of these sets, have, in this respect at least, departed further from the common stock of Mammalia than have many of the true placental forms. There is, however, not unfrequently, considerable variation in the extent of replacement, even within the same groups of placental Mammals; for instance, among the Rodents, the Leporidæ have their incisors replaced, while the rat is entirely monophyodont.

A further point of interest lies in the fact that, whereas in the Marsupials it is the second series of teeth, or that which corresponds with the permanent series of most higher forms, which has become degenerate, in other groups it is the precursory ones that are suppressed. We see this suppression of the milk teeth in some of the Carnivora: in the dog, for example, the deciduous teeth are well developed, the bear has much smaller ones, while in the seal they are still more rudimentary, and absorbed before birth.

E. C. POLLARD.

V.

The Recapitulation Theory.

A REJOINDER.

IF the value of a theory is to be measured by the number and the extent of the papers written, and by the amount of work done under its inspiration, then the value of the Recapitulation Theory is indeed great: but there is always room for the doubt as to whether an equal amount of work would not have been done without that inspiration; whether, for instance, Hyatt, Buckman, Jackson, and Beecher would not have published researches as great and as brilliant as those referred to by Mr. Bather in his article on "The Recapitulation Theory in Palæontology" (p. 281), even if they had been inspired by something less fantastic than the theory in question.

In attacking this theory, however, I was fully conscious of the enormous amount of work done under its inspiration; and in attempting to destroy the theory, I was fully alive to the possible effect upon the work of the future. I look upon the loss of all the "recapitulation" literature with perfect equanimity—nay more, I look forward with hope for the time when men shall cease to occupy the pages of our scientific journals with controversies as to whether "the ancestor" of the vertebrates was an ascidian, polychæte, nemertine, leech, turbellarian, sea-anemone, sponge, spider, crustacean, Balanoglossus, echinoderm or other *recent* animal of the embryos of which the controversialists happen to have been cutting sections. I do not think that if the writings thus briefly catalogued had been written without the "inspiration" of the Recapitulation Theory, they would have been less valuable. The little I do know about the psychology of "dominant ideas" leads me, indeed, to believe that the investigations would have been carried out, not only more fully, but also with far less error of observation. Under the influence of a dominant idea, men see what does not exist, and are blind to what lies before them; and what is even worse, what little they do see of the things before them is seen distorted. Whether the dominant idea be a true idea or a false one, if dominant, it is almost certain to lead to profound error. When first the idea of evolution gained a hold over men's minds through the influence of Darwin's and Wallace's theory of Natural Selection, it suddenly developed into a *dominant* idea in the

minds of those who accepted it incautiously. It was not the theory which was wrong, but the mental attitude of those who accepted it. It was not accepted as a theory, but adopted as a creed. Fortunately, those who did so adopt it were comparatively few.

With the Recapitulation Theory it is otherwise. *All* who have accepted this, have adopted it *as a creed*. Few of them can even see that it is not an explanation of anything whatever. They *believe* that it explains the series of events constituting ontogeny. In reality, it is only a dogmatic assertion that this series of events is controlled by some mystical "dead hand of the past" in such way as to serve as a record of events long passed. If the dogmatic assertion as to the course of events constituting the ontogeny had proved to be true, then it would be difficult to imagine any more interesting problem than that involved in the *explanation* of so remarkable a phenomenon.

The truth of the assertion has been put to the test, not of direct comparison of the ontogeny with the phylogeny in a large number of cases, for that has hitherto proved impossible (*pace* Bather), but by comparison of the supposed "records" with each other, and by other indirect means. The mere fact that of all the conclusions drawn as to the origin of the vertebrates every one contradicts every other, is one disproof of the theory. The fact that von Baer's law is in exact accordance with observed facts in a large number of cases is another and a more definite disproof. The two views—von Baer's and the Recapitulation Theory—are irreconcilable. Von Baer's is applicable only to certain animals, but its applicability to these disproves "recapitulation" in these. These animals are such as pass through a considerable portion of their development as embryos, or fœtuses, before entering upon the struggle for existence amidst untoward circumstances. The remaining animals, that is, those which have to fight for a living from very early stages indeed, are, by virtue of that early struggle, exposed to the modifying influence of Natural Selection; and, hence, if any "record" had existed in the ontogeny of such species, it must of necessity be very soon obliterated by this action of Natural Selection. The obliteration need not be complete. Some traces of the ancestral ontogeny (*not* phylogeny) may remain, and, so far as these traces have been made out, they also are in conformity with von Baer's law, and, therefore, inconsistent with the theory of recapitulation.

It by no means follows that no ontogeny is such as to present even a startling resemblance to the phylogeny of the same species. Mr. Bather's too brief accounts of the truly remarkable correspondence between what is known to be the ontogeny and what is supposed to be the phylogeny of *Antedon*, will sufficiently impress my readers with this fact, and it is "greatly to be hoped" that he will shortly enrich the pages of NATURAL SCIENCE with a very much fuller account, both of all the stages of *Antedon* and of all its supposed ancestors, *with figures of every one of them*, that we may all more clearly

see what are "the plain facts of palæontology," to which it is suggested (p. 281) that somebody's eyes (? mine) are "persistently shut."

"It is not an occasional accidental parallelism between the ontogeny and the phylogeny which I deny, but the *causal relation* between the two." (I quote from my previous article, p. 198.) This appears to have been overlooked by Mr. Bather, and the whole of his article is consequently directed against a position which I have never held. I have stood perfectly still, and his explosive bullets have rushed past me with a violence suggestive of a shower of meteorites: his aim was excellent; not a single stray bullet touched me.

Mr. Bather admits that "von Baer's law is undoubtedly true" in cases of fraternal relationship, but holds that "there are many objections to supposing that it is equally true" in cases of filial relationship. I had quoted Darwin to show that in the case of the pigeons where the relationship between *C. livia* and the rest is of this kind (the only case I know of which has been tested) the law is fully applicable. May I therefore ask for, say, half-a-dozen of the best of the "many objections" referred to?

In defence of myself against what has now, unfortunately, reached the public eye, I feel bound to correct some of Mr. Bather's misapprehensions as to my position. To each reply I will prefix the number of the paragraph in his article where the misrepresentation occurs.

(3. Middle of p. 276.) The larval stem of *Antedon*, if it be, as I suggested, the modified equivalent of the stalk of the ancestral larva, need not be supposed to be anything other than what Mr. Bather describes, nor yet need any conclusions be drawn from it (as to the stem of its ancestors) which differ in the slightest from the conclusions he has drawn from palæontological work.

(3.) I have not ignored "acceleration of development." The antlers of stags were given as an example of it.

(5. Bottom of page.) Nothing I have said will bear the interpretation here put upon it. Each transient ontogenetic stage may be a modified equivalent of a corresponding ontogenetic stage in an ancestor, and may yet be either more complex or less complex, or may even be so completely changed that no resemblance is observable.

(5. P. 277.) I do not feel bound to suppose, and have never supposed, anything at all comparable with what is here suggested. I do unhesitatingly say that the ontogeny described in *Antedon* is not an epitome of the ancestral *history*. There is a truly remarkable parallelism between the ontogeny and the phylogeny as here set forth. It is not a history, inasmuch as the correspondence between the two series is not one which could safely be *assumed* to exist apart from palæontological evidence.

(7.) Let it be assumed that the evidence set forth by Darwin

proves conclusively that the domesticated breeds of pigeons are descended from a wild species in no way differing from the existing *C. livia*. Then variation (in my narrowed sense of the term) has so affected the adult structure, under the influence of long-continued artificial selection, as to produce barbs, runts, fantails, &c. Direct observation of late stages of development in each of these breeds has shown (Darwin) that these stages also have varied in the *same* direction, though to a smaller extent. In each of these breeds the newly-hatched nestling is *less* like the adult *C. livia* than is the newly-hatched nestling of the "wild parent species," *i.e.*, the *C. livia* of to-day. In order to produce a "record" of the descent from *C. livia*, *i.e.*, in order that the late stages of development should reproduce the *adult* characters of *C. livia*, those late characters would have had to vary in the direction of *greater* likeness to the adult *C. livia* during the same time as the adult characters were varying in the direction of *less* likeness. That is what I have called varying in "another" direction. I did not say "opposite" direction, for the simple reason that stags' antlers, &c., were in my mind at the time, and it was obvious that, in such exceptional cases, the two directions were nearly or quite parallel.

(8.) I have never either denied or hesitated to admit the occurrence of such phenomena as Mr. Bather records in the Ammonites; nor do I dispute the *facts* made known by Würtemberger, Waagen, Branco, Hyatt, Buckman, or anybody else. What I have denied I will deny afresh at the end of this article.

(9.) I find no fact whatever (not even an assertion!) in the table given on p. 279 which in any way runs counter to any view I have put forward.

(10.) I have paid full attention to the opinions of palæontologists so far as I have become aware of them, but I bow to no *opinion* based upon an argument which is inconclusive. The mere opinion of the whole zoological and embryological world appears to be opposed to mine. Would Mr. Bather wish me to bow to that opinion also?

(11.) What Mr. Bather calls my "main fallacy" is one into which I have never fallen! I have never believed that existing species are descended from other *existing* species, except under the influence of artificial selection (as in pigeons), or geographical or other isolation.

(11.) All seven parts of the quotation tell equally in my favour. Not one tells against me.

(12.) Whatever I may seem in Mr. Bather's eyes to suppose, I can assure him that I never supposed any reasonable person to hold the Recapitulation Theory in any form demanding such a "harlequinade" as he describes. It was not in any such form that I either described or attacked it.

(13.) The reason for introduction of *Culex*, etc., into my paper will be obvious to anyone who will read the paragraph which

mentions them. (P. 196.) It begins with the words "Another kind of variation is illustrated . . . ." By reading to the bottom of the same page it may be seen how I "made capital out of them."

(14.) "Another mistake" of mine (as Mr. Bather calls it) is like my "main fallacy"! Not only did I *not* refuse to admit more than one kind of variation, I actually insisted on the necessity of recognising *at least* two kinds. (P. 196.) I am fully aware that each "kind" includes many varieties.

(14.) The remainder of this paragraph is unintelligible to me. "The very same '*change in the constitution of successive generations* of a species leading to the production of a new species' *does take place in the life-history of a single generation.*" And again :—"All these well-known instances of variation (and I use the term precisely in Mr. Hurst's own sense) 'occur in a way utterly unlike the way in which it does actually occur,' *i.e.*, within Mr. Hurst's horizon." What can this mean?

(15.) By making myself acquainted with certain brilliant researches "inspired by the Recapitulation Theory" to which Mr. Bather directs my attention, I have no doubt whatever that I may learn much. That the brilliant researches should have been inspired by that theory will certainly *not* make me reconsider my condemnation. The whole science of chemistry has arisen out of brilliant researches inspired by the theory of the transmutability of the baser metals into gold. Shall we therefore accept that theory? Mr. Bather is hopelessly at fault in his assumption that my attack on the theory is due to my ignorance of the many brilliant discoveries which have been made under its inspiration.

The "amended" statement of the method of variation given on p. 277 errs in the precise way in which Mr. Bather says that my paper did. It refuses to admit more than one kind of variation! Moreover, it speaks of events being "*pushed back*" *in time* by other events which have not yet happened. This is, no doubt, figurative, but whether any such erroneous conception exists in Mr. Bather's mind or not, the acceptance of the statement in that form by recapitulationists would certainly produce in the minds of the "*profanum vulgus*" an impression that this was a case in which the effect is produced before the cause has arisen.

To prevent any further misrepresentation as to what I have denied and what I have not denied, I will conclude with a summary statement of my position:—

I do not deny that a rough parallelism exists in some cases between ontogeny and phylogeny. I do deny that the phylogeny can so control the ontogeny as to make the latter into a record of the former—even into an *imperfect* record of it.

I assert that those very characters of the adult which vary most in the adult are precisely the same as those which vary most in the late stages of development also.

I do not deny that there may be *exceptions* to this last statement.

I deny that any one of the theories as to the ancestry of the vertebrates based exclusively on embryological grounds is worth the paper it was written on.

I maintain that much otherwise good embryological work has lost much of its value through the bias produced in the minds of the writers by the dominancy of the idea of recapitulation.

Comparative anatomy gives some help: Palæontology may give even more: by their aid embryological facts may be explained: but the facts of palæontology can never be discovered by the study of embryology.

"Vestiges," and these only, can give any embryological clue to past history which could not be equally well made out from comparative anatomy. In this last I may be wrong.

I maintain that the applicability of von Baer's law to even a single case would be a definite and final disproof of the Recapitulation Theory even in its most reasonable forms.

Finally, I assert that the applicability of von Baer's law in a large number of cases is capable of being readily proved by anybody, by a direct appeal to Nature, *i.e.*, by the study of a few vertebrate embryos taken at random.

C. Herbert Hurst.

VI.

Climate and Floral Regions in Africa.

ONE of the strangest features of African botany is the extreme poverty of species within the tropics, accompanied by a wealth and superabundance of vegetation, *i.e.*, of individuals, which can be hardly realised without actual observation. On the other hand, outside the tropics, both the number of species is astonishing (sometimes there are more known from a given area than is the case anywhere else in the world), and again, the number of individual plants on a given area is exceedingly small as compared with the tropics, and never shows the same extraordinary luxuriance.

The reason for this difference may be partly appreciated by noticing some of the distinct climatic conditions which exist in temperate Africa; each different climate maintaining, of course, its own set of plants.

Travelling in imagination along North Africa, from Gibraltar to Egypt, we notice the following changes. First, in Morocco, Algiers, and part of Tunis (roughly speaking, on the Northern Mediterranean side of the Atlas chain and its continuations, which run out in the sea near Tunis) we find ourselves in a Mediterranean climate. It is a fairly dry country, but does not suffer from drought to any severe extent. On an Algerian hillside, one sees numerous wild olive trees, many bulbous plants, and small shrubby perennials, while in sheltered places many annuals occur, but there is an almost complete absence of thick sward and the dense growth which one notices, for instance, in England. There is nothing like the crowding and struggle for existence which goes on in temperate Europe; every plant seems, as a rule, able to spread out, and the tint of the landscape is that of the underlying soil, often red, with a sort of grey-green haze, due to the scanty vegetation; the number of species is, however, fairly large. An examination of the plants themselves shows that they are mainly Mediterranean forms with numerous endemic species or varieties, whose origin from European species or their ancestors is intelligible enough if we remember the years that have elapsed since Tunis was in free land communication with Sicily, and since the Straits of Gibraltar were formed.

In Tripoli and the Bay of Syrtis, we are, in the desert of Sahara, exposed to the most severe and protracted drought. The plants that exist are very isolated indeed, in most places a foot or more apart,

and there are no plants whatever over wide areas. In spring, a considerable number of small annuals start up after the rains, flower and fruit in an incredibly short time, and die down immediately the country dries again; the residents are stunted, very often thorny bushes, extremely small and only able to resist drought either by covering themselves with a woolly coat, or by hiding their transpiration pores in cunningly-concealed pits or grooves which are so many traps to prevent the egress of the precious water. Some are said to surround themselves with an atmosphere of their own by exudation of essential oil, &c., but this is theoretical. Many have no leaves, others only have leaves in the wet season, and the few that keep their leaves are succulents or have them most beautifully adapted to their requirements.

When we proceed further along the coast and reach the little limestone hillocks by the sea at Alexandria, we are again among a different set of plants. They are very near the Algerian forms, but not exactly the same; probably they came from Palestine and are part of the Syrian flora as distinguished from that of the desert. Nothing is more exquisite than the flora of these nummulitic limestone hills. There are masses of the crimson *Ranunculus asiaticus*, the delicate little white star-of-Bethlehem, many species of *Astragalus*, and generally a wealth of colour and a beauty of form which I have never seen elsewhere.

On entering the Nile delta there is again a sudden and distinct change. The deep, carefully-irrigated alluvium is covered with a dense mass of plants, that is, if they are allowed to grow, for the soil is very valuable, and these very special characteristics of soil and water have produced the endemic Egyptian plants, such as the *Trifolium alexandrinum*, or "berseem" clover, of which some seven crops can be raised in one year on the same ground. Travelling up the Nile, we find ourselves before Assouan out of the delta flora, and again, as at Tripoli, in the great desert whose plants in many cases extend from Beluchistan to the shore of the Atlantic between Morocco and Senegambia.

Thus, in North Africa there are four marked floras more different from one another than are the floras of England, and one might almost say, Tibet, and to these must be added that of the higher peaks of the Atlas Mountains, which is of an alpine character, that is to say, five different sets of plants in Extratropical North Africa.

In Extratropical South Africa, beginning at the North, we find along the coast an offshoot of the evergreen humid tropical forest which has crept down from the Zambesi, and has occupied Pondoland, the lower parts of Natal, and, in an extremely modified condition so far as species go, formed the Perie Forest of King William's Town, and the Knysna Forest of Cape Colony. All these species of the eastern littoral, finding themselves in temperate and not tropical conditions, have varied greatly, and being separated from each other,

the forests of lower Natal, the Perie, and the Knysna have each their own endemic forms. Each is, in fact, a climatic island, and varieties suited to very local conditions have free play.

Further inland the plateaux of Mashonaland, the Transvaal, and upper Natal are covered with grass, among which are perennials and bulbous plants, and in places isolated shrubs or small trees, chiefly acacias. It is believed that there must be a communication with Abyssinia by ridges or mountain summits, at least 5,000 ft. high, in the tropics, and that the plants have travelled down along these ridges and summits without experiencing any great climatic change on the way, but this is rather theoretical at present. At any rate, they have taken on different specific characters *en route* in most cases, and they are quite different from those of the coast below 2,000 ft. Following the plants of these grassy plains southwards, we can trace them, often in very modified forms, on isolated mountain summits (such as at Somerset East) as far as the top of Table Mountain at Cape Town; but within the Cape Colony these grassy mountain tops, which are usually flat and table-like, are also inhabited by a wealth of endemic Cape genera and species, which probably ascended the hills from the lower parts of the peninsula. Every isolated mountain top may be in this way a climatic island, with species of its own. Still these plateaux and tables are generally in a fairly moist climate, and under quite different conditions to either the arid deserts of the Karoo or the western parts of Cape Colony and the lower parts of the peninsula.

In the Karoo and Namaqualand similar conditions prevail to those of the Sahara, and we find there bulbous plants, succulents, thorny plants, and densely matted perennials, similar in structural type to those of the Sahara, but of utterly different origin. The Cruciferæ and Chenopods, which are predominant in the latter, are almost absent here, and their place is taken by Mesembryanthemums, Stapelias, Pelargoniums, and others, which are not found in the north, and make a very different flora. Here, again, we find the landscape the colour of the soil, and scarcely tinted by the vegetation.

As for the distinctive Cape flora (that is to say, the remainder after the Karoo-Namaqualand and mountain-flora has been subtracted), it is different to any of the others; the conditions are rather dry, and yet not desert ones; the rainfall occurs at a different season to that of the eastern slopes and the Transvaal, and the prevailing type of plant is again different. Most are shrubby little perennials like the heaths, and a very large number have heather-like leaves; there are many bulbs, and scarcely a single annual of any sort or kind. The landscape is not wholly free from the colour of the soil, but it is masked by the grey-green tints of these little shrubs, and sometimes it is bright and rich in colour, as when *Erica Plukenetii* is in bloom. It is never, however, so beautiful as, for instance, that of the top of Table Mountain in the right month, which simply beggars descrip-

tion, and the conditions vary so greatly that in this peninsula there are probably more species than exist anywhere in the world on a similar area.

This makes, therefore, in Southern Extratropical Africa, four distinct floras, all, probably, closely united genealogically, but all specifically and very often generically distinct.

Turning now to Tropical Africa, a glance at the map and the exercise of the imagination will show that the enormous valleys of the Congo and Niger, up to from 3,000 to 5,000 feet, have been excavated out of a continuous tableland, and are in free communication with each other by the littoral of the west coast. Speaking generally, the whole of this enormous region is one continuous, dense, evergreen forest, and similar humid conditions prevail throughout its whole area. It, therefore, forms another flora comparable with the others and probably not nearly so much richer as might be supposed. The higher lands above 3,000 feet at 10° N. lat., and above 5,000–7,000 feet at the Equator, have another flora of a more or less similar type to that of Abyssinia, and the truly alpine flora of the Cameroons, for instance, seems almost specifically the same. In this region one can never see the landscape, as one walks usually through hedges of trees and shrubs 30 to 70 feet in height, or when the forest has been cleared, through tall grass which gives a realistic idea of the presumable view of a rabbit in a field of corn.

Hence, to show the relative abundance of floral regions, we have in North Extratropical Africa the South European flora in Morocco and Algiers, the Sahara desert flora, the Syrian flora at Alexandria, and the Egyptian or Delta flora, while in Southern Extratropical Africa there are temperate forest floras in Natal and the Cape, a grassy plateaux flora in the Transvaal and Cape Colony, the Karoo desert flora, and that of the Cape proper; that is to say, eight different sets of climatic conditions. In Tropical Africa there are the evergreen forest and the plateaux flora, that is to say, only two, of which the second is practically the same as that of the Transvaal, or is, at any rate, so close as to be scarcely reckoned as distinct. If one takes in the alpine flora of the highest summits of the Atlas, Abyssinia and Kilima-njaro, we get a total of eleven different floras.

The manner in which these have so far been studied reveals the difficulties of systematic botany. Boissier's "Flora Orientalis" mixes up part of the Algerian, part of the Sahara and part of the Syrian and Egyptian floras. Oliver's "Flora of Tropical Africa" includes the tropical forest, the Abyssinian and Transvaal grassy plateaux, and the alpine plants in part, and is about half done. Harvey and Sonder's "Flora Capensis" mixes inextricably the grassy plains of the Transvaal, the Karoo, and the peninsula, and is not half finished. Engler's "Hochgebirgeflora," containing the high mountain plants, is the only attempt to work on natural lines, and is also pre-eminently distinguished from the last two by having reached a conclusion.

G. F. Scott Elliot.

VII.

The Moas of New Zealand.

IN a paper in vol. xxiv. of the *Transactions of the New Zealand Institute*, p. 93, Mr. F. W. Hutton has discussed at great length the classification of the Moas of New Zealand. "My work," he says, "is founded on the measurement of the leg-bones of individual birds belonging to sixteen different species, and from these I have inferred with considerable certainty the proportion of the leg-bones in the other species." As to the genera, he says, "the crania of the different Moas are quite sufficient to indicate the existence of several genera. . . . After reducing the species to order, I find that they fall into seven well-defined genera founded on the crania, but generally accompanied by characters derived from the pelvis, the sternum, the absence or presence of a scapulo-coracoid, and the robustness of the leg-bones" (*loc. cit.*, p. 100).

In his paper Mr. Hutton gives a table in which the *Ratios in the Genera* are obtained, *as regards the long bones*, by dividing the length by the girth; and *for the skull* by dividing "the breadth at the squamosals" by "the height at the basitemporal," called ratio of breadth to height, and "the length from the supra-occipitals to the nasals," by "the breadth at the squamosals," to obtain his ratio of length to breadth. On examining this table we find that the maximum ratio of length to girth of the metatarsus in *Dinornis* is 3·6, and the minimum 2·5; while the maximum and minimum ratios in *Tylopteryx*, *i.e.*, 3·3 and 2·7, *both* lie within the maximum and minimum assigned to *Dinornis*. The same is the case with their tibiæ and the femora, so that as regards their long bones, those placed in *Tylopteryx* could all have gone into the genus *Dinornis*, for there is no reason why they should be assigned rather to one skull than to another. If we take the ratios of the crania (assigned to a great extent, therefore, arbitrarily to these limb-bones), the maximum and minimum ratios of breadth to height and of length to breadth in *Dinornis* fall completely within the ratios of *Tylopteryx*, and those of the sternum and pelvis of *Tylopteryx* fall within the ratios of *Dinornis*.

The ratios of the maxima and minima of both the metatarsus and the tibia of *Palapteryx* (the next genus further removed than *Tylopteryx* from *Dinornis*), fall also within those of the latter genus, so that as regards the metatarsi and tibiæ of *Dinornis*, *Tylopteryx* and

Palapteryx, there is no apparent reason for separating them. Take the next genus, *Anomalopteryx*, the maximum ratios of both its tibiæ and femora fall between the maximum and minimum of the genus preceding it, while the maximum ratios of the metatarsus, tibia, and femur of *Cela*—the genus following in succession—fit between the maximum and minimum of those in *Anomalopteryx*, while the ratios obtained from the metatarsus, tibia, and femur, in the next genus still, *Mesopteryx*, fit also between the highest and lowest ratios in *Cela*. It is evident that these ratios are valueless for differentiating the various bones into their proper genera *by measurement* alone, as is done in this paper. Indeed, if Mr. Hutton has correctly assigned to the genus *Euryapterx* three types of skull, why may not many of the varieties or types of cranium of Moa which we know not have belonged to one genus, even species, just as the cranium of *Gallus* or *Columba* does? It is also only in a few cases known certainly to what crania the numerous separated beaks belong, most of them having been only *fitted* together.

Turning to the question of species, the metatarsus of *Dinornis maximus* is separated from that of *D. excelsus* by extreme difference of ·5 inch in length and 1·25 in girth, and by a mean difference in the two species of ·5 inch in length and of ·7 in girth; while associated together as *D. validus*, we have metatarsi varying as much as 1 inch in length and ·8 inch in girth. The difference between the minimum tibia of *D. maximus* and the minimum tibia of *D. excelsus*, is 1·5 inch in length and 1 inch in girth, while we have tibiæ differing by 1·5 inch in length and in girth of 1·2 inch assigned to the one species, *D. validus*. Again, the maximum metatarsus assigned to *D. giganteus* lies midway between the greatest and least metatarsus assigned to *D. validus*; the minimum tibia assigned to the former is intermediate between the largest and smallest of the latter; while the maximum femur of *D. giganteus* is identical with the minimum femur of *D. validus*. The greatest difference in the metatarsi between the largest *D. validus* and the smallest *D. giganteus* is only ·5 of an inch in length, and 1·8 in girth; in the tibiæ 2 inches in length and 1·2 in girth; in the femora 1 inch in length and 1·4 girth. If we compare *D. robustus* with *D. firmus*, we find,

	In *D. robustus*.		In *D. firmus*.		A mean difference of	
	Length.	Girth.	Length.	Girth.	Length.	Girth.
Metatarsus ..	17·2–15·7	6·7–5·2	17·0–13·75	5·6–4·75	·08	·78
Tibia	32·7–30·0	6·8–6·2	33·0–30·0	6·5–5·3	·35	·60
Femur	15·5–14·4	8·1–7·2	15·25–14·5	7·5–6·0	·07	·90

Take another instance, and compare *D. ingens* with *D. firmus*:—

	D. ingens.		*D. firmus*.		Difference.	
	Length.	Girth.	Length.	Girth.	Length.	Girth.
Metatarsus ..	15·25	4·75	15·75	4·75	·50	·00
Tibia	29·50	5·30	30·0	5·30	·50	·00
Femur	14·25	7·25	15·25	7·50	1·0	·25

Is there any real reason why these metatarsi, tibiæ, and femora

should not all be assigned to the same species? Mr. Hutton has in his paper placed bones that vary by only ·5 of an inch in length in *different* species, while in the case of *D. validus* above, we have metatarsi differing by 1·5 inch in length and 1·2 in girth united in the same species. Why also should metatarsi whose dimensions lie between

12·2–13·0 ins. in length and 4·0–4·75 in girth be described as *D. torosus*.

those between

13·0–14·25 ,, 4·25–4·9 ,, ,, *D. gracilis*.

and those of

12·0 ,, 5·0 ,, ,, *D. struthioides*.

and why should femora of

11·50 ,, 5·5 ,, ,, *D torosus*.

and those of

11·25 ,, 5·5 ,, ,, *D. struthioides*.

We find in this lengthy paper also the following:—

Metatarsi measuring 10·4 inches in length and 4·0 inches in girth = *dromioides*
and ,, ,, 10·6 ,, ,, 4·0 ,, = *plenus*
giving a difference of 0·2 in length and ·0 in girth.

Tibiæ measuring 21·0 inches in length, and 4·0 inches in girth = *dromioides*
and ,, ,, 21·5 ,, ,, 4·6 ,, = *plenus*
giving a difference of ·5 in length and ·6 in girth.

and Femora measuring 9·6 inches in length and 4·0 inches in girth = *dromioides*
,, ,, 10·0 ,, ,, 4·0 ,, *plenus*
Difference of ·4 in length and ·0 in girth.

How unsatisfactory this method of classification is, may be gathered from Mr. Hutton's observations under *Cela curta*, Owen, where we read: "I regret that I cannot maintain *D. oweni* (described by Von Haast) as a separate species. It is only a small individual of *C. curtus* . . . Sir J. V. Haast says that the femur is shorter and the metatarsus more slender in *D. curtus* than in *D. oweni*; but my measurements do not bear this out, and the supposed anatomical differences between them are only individual variations, which may be found in almost every species of Moa. If *oweni* is to be separated from *curtus*, then for the sake of uniformity most of the species should be split into two, for they show quite as wide a range of variation." Now of *D. oweni*, the *metatarsi* are ·6 of an inch in length by ·5 inch in girth, and the *tibiæ* 1·65 inch in length by ·35 inch in girth less than *C. curta*; but the metatarsi and tibiæ of *P. dromioides* and *P. plenus*, which differ by a much less fraction from each other, are placed under different species. Nevertheless, it has been found necessary to unite Haast's species, *D. oweni*, though separated by greater differences, to *Cela curta*. It is very difficult to comprehend on what grounds these unions and separations are based, and impossible to assign unknown bones to their proper genera and species solely by this method of differentiation.

As for the identification of *Anomalopteryx antiqua*, it is founded on the photographed pieces of a metatarsus which Mr. Hutton had no opportunity of examining, and on fragments of two tibiæ, which

were, as I was informed by the mine-driver who excavated them, obtained from tunnels far apart, and many feet further under the lava, at a different part of the quarry from where the metatarsal fragments were obtained. These fragments are quite insufficient to determine with anything like the accuracy employed throughout his paper by Mr. Hutton the length of the tibia, which is, notwithstanding, entered (without a mark of interrogation as if certainly ascertained) as 12 inches. The chances are almost infinite against the two pieces being parts of the same bone. In speaking of these two fragments of tibiæ (which I examined carefully in the matrix, but found too imperfect to dare to identify), I say that they "were, undoubtedly, portions . . . of one of the greater forms." I should have expressed myself more accurately if I had said they were "undoubtedly not one of the smaller forms." As regards the metatarsal fragments which Mr. Hutton has assigned to this species, apparently for the reason that they were found in the same quarry, I have carefully compared them with the collection in the British Museum, and they agree most closely in form, though larger in size, with those of *D. curtus*. They belonged, however, *most certainly* to a species which was *not* "smaller than any which lived subsequently in the south island," as Mr. Hutton affirms. It cannot, therefore, yet be argued from them that the earliest known Dinornithidæ were smaller than those that succeeded them.

The differentiation of Moa bones into genera and species solely by the differences in their length and girth, I hold to be quite unreliable and misleading. Their form and outlines, I venture to think, are the only characteristics by which they can be accurately separated from each other. Before leaving New Zealand, I had already commenced to protract on paper the prominent features of the principal groups by referring them to rectangular co-ordinates which were drawn through the corresponding points of all the bones. By this method, I found that their points of agreement and difference, quite apart from size, could be strikingly exhibited. Professor T. J. Parker has recently contributed to the Zoological Society in a paper to be published in its *Transactions*, a classification of the Dinornithidæ, solely based on the crania of these birds; and as he hopes, I believe, to supplement it by a paper on the remaining bones of the skeleton, I trust he may give the suggestion I have made here a trial. The method will, at all events, I think, be found to afford an invariable standard of reference in describing the different bones. His forthcoming paper has, it is satisfactory to know, been collated with Mr. Lydekker's classification in the British Museum Catalogue of *Fossil Birds*; while, on the other hand, it is greatly to be regretted by all workers on this most difficult subject that Mr. Hutton did not defer the publication of this valuable paper, in which has been brought together almost all the known information on the Moa, till he had found time to compare his nomenclature with

that of Mr. Lydekker's catalogue,—a volume which had already reached the Colony before the reading of his paper, in which the classification is based on the type material in the British Museum, instead of adding to the already almost hopelessly involved synonymy of the Dinornithidæ, as his paper before the New Zealand Institute unfortunately does.

As to the age of the oldest fossil Moa bones found in New Zealand, Mr. Hutton, I find, dissents from my opinion as to the geological horizon in which the remains were found, which he is, of course, entitled to do. In stating, however, that "this conclusion was not arrived at by a re-examination of the sections at Mount Horrible and the Pareora, Mr. Forbes merely went to a quarry near Timaru, and, with 'little doubt,' identified a 'rough red shingle,' which he did not even see *in situ*, with the gravels of the alluvial fans of the Canterbury Plains," Mr. Hutton has affirmed what is not only quite incorrect, but what could not possibly be within his knowledge. He could not know how often I examined the geological structure of the region, or with what care, or what I have seen or not seen *in situ*. The shingle below the clay (or so-called loess) underlying the lava-bed, by which the age of the section is determined, I practically *did* see *in situ* on my first visit, and having subsequently re-examined the district, I have assured myself of the correctness of my previous observations and opinion. I shall, however, most willingly admit that I am mistaken as to the age of the strata in which these *Dinornis* remains have been found as being other than "newer Pliocene or even Pleistocene," so soon as the officers of the New Zealand Geological Survey—who are really the only competent referees in the case—shall have assigned to these gravels a different age. Notwithstanding Mr. Hutton's doubt as to the correct identification of the avian remains in the same bed in association with those of the Moa, I can, without hesitation, re-affirm that the bone I determined as *Apteryx australis*, and figured in the *Transactions* of the New Zealand Institute (vol. xxiii., pl. xxxvi., p. 368), really belonged to that bird. The bone, of which the drawing (for which I am responsible) is unfortunately not so good as a better draughtsman might have made, was, as I there pointed out, somewhat distorted by heat, but otherwise its identity was unmistakable. I am glad to find that Mr. Hutton admits "if it had been correct that bones of a living species of Kiwi occurred with them [the Moa-bones], it would have been strong evidence in favour of the bed being younger than Miocene." His observation that the figure in the plate illustrating my paper more resembles the femur of *Aptornis* surprises me, for the bones in the two birds are very dissimilar in form and strikingly different in size. The bone figured by me is represented of the natural size, and even in so poor a figure, the delineation of its internal structure recalls at once the section of an *Apteryx* femur.

Mr. Hutton, in the same paper, discusses the question, How long

ago did the Moa became extinct? My exploration of the Sumner Cave proved conclusively that the Moa and the Maori were contemporaneous (*Trans. N. Z. I.*, vol. xxiii., p. 373). This cave had been shut up by an extensive landslip, but for how long a period it was impossible to discover any evidence. The carving on the implements left on its floor was unmistakably done by Maoris. Along with the remains of Moas and other birds round the last fire-place of the inhabitants, I found the shells of Moas' eggs, with the shell membrane still intact, showing that the shells had not been part of any utensil, and their presence in the cave at all seems to leave little doubt that they were brought in as eggs for the purposes of food.

Now it is held by some authorities on Maori history and tradition that, because the Moa is rarely mentioned in their poetry or proverbs, this knowledge "dates from . . . . almost prehistoric times, long before the beginning of the genealogical descent of the tribes, which, as we know, extended back for more than twenty-five generations" (Colenso, *T. N. Z. S.*, vol. xii., p. 63). On the other hand, the Maoris now or recently living, especially in the North Island, have numerous traditions about the Moa, its colour, feathers, food, manner of life, fighting, and about their mode of capturing it. Mr. Hutton says, "So far as the North Island is concerned, I am compelled to believe that the Moas were exterminated many [400–500] years ago, because I feel sure, if it were not so, we should find as many allusions to it in Maori tales and poetry as we do to all the other birds, beasts, and fishes that were of interest to the natives." The date of the extinction of the Moa, I feel, however, cannot be reliably determined by reference to the native traditions or proverbs, for in Monck's Cave at Sumner, associated with the remains of the Moa, I discovered, for the first time, evidence of the former existence in New Zealand of a species of swan, somewhat exceeding the Black Swan of Australia (*Chenopis atrata*) in size.

Shortly afterwards, I received from various correspondents (chiefly from Mr. Hamilton, now of the Otago University), and from all parts of both islands, abundance of evidence that this bird had been at one time widely distributed, and had been used in great numbers by the Maoris for food down to comparatively recent times, for their remains were found in middens, precluding the possibility of their being of vast antiquity. In the Chatham Islands, also, I found, in abundance, bones of what I take to be the same species of swan, in ancient kitchen middens of the Morioris, in association with the remains of *Aphanapteryx*, and from localities there, as well as from the state of their preservation, which also precludes the idea of any great age. Yet though so abundantly used as food by these peoples, and especially by the Maoris, who have handed down, with great fidelity and exactitude, the names of most of their food animals, with their mode of capture, their traditions are entirely silent about this conspicuous bird, and, till two or

three years ago, no Maori scholar had even a suspicion that the swan was known to the natives before its quite recent re-introduction by Europeans, and if this omission from their traditions has taken place with one important bird, why not with another? In this case, the negative evidence relied on by Mr. Hutton is far from being convincing.

The enormous number of bones found by the early settlers scattered over the surface of the ground on both islands, which have now nearly disappeared, is evidence which seems to me to prove that the Moa existed down to quite recent times. This disappearance of the bones is attributed by Mr. Hutton to "the constant burning of the scrub by *Europeans* [for he evidently thinks (p. 151) that there were no fires before there were "human inhabitants to light the fires"], in which case the surface bones do not prove the late existence of the Moa in the districts where they were found." In the time (according to Mr. Hutton 400–500 years) since the Moa disappeared, there must have been all over the country fires ignited hundreds of times by lightning, and by stones rolling down hill-sides striking sparks from other stones and setting the parched grass, which is as inflammable as tinder after every nor'wester, on fire, which would travel farthest and burn most furiously just in those dry regions of Otago, where, Mr. Hutton believes, "bones, skin and ligament, once dried, and protected from the sun might easily be preserved for centuries." Then, also, the numerous little heaps of gizzard-stones lying on the surface of the ground (evidently voided by the birds), can, surely, scarcely have remained so little disturbed, even where protected by vegetation, for four or five centuries, in the midst of the denudation that goes on in New Zealand, by rain, frost and winds; for the gales that blow from the north-west in the summer can carry pebbles, as I have myself seen, larger than most of the Moas' gizzard-stones. It is very singular that Polack (whose book was published in 1838) should have been told by the natives that large birds (which he considered to be struthious) were then still living in the South Island, and this, it must be remembered, was before there was any suspicion of the existence of the Moa, and before the first bones had been discovered; it is not improbable, therefore, that the Moa may have been living on the South Island down even to the time that Captain Cook visited New Zealand.

Much, it is evident, still remains to be done before it can be said that the life-history of the Moas has been fully elucidated or their classification satisfactorily established.

HENRY O. FORBES.

SOME NEW BOOKS.

TYPES OF ANIMAL LIFE. By St. George Mivart. Small 8vo. Pp. viii. and 374. Illustrated. London: Osgood, McIlvaine & Co., 1893. Price 6s.

AUTHORS sometimes fail to do themselves justice by not selecting a sufficiently comprehensive title for their productions, but this omission certainly cannot be charged against Professor Mivart in regard to the present work. When we first opened the volume before us we not unnaturally expected to find a work somewhat on the lines of the late Professor Rolleston's well-known "Forms of Animal Life," in which typical representatives of the leading groups of animals of all kinds would be treated in the author's well-known style. Our surprise was accordingly great when we found that "Types of Animal Life" really meant, at the most, "Types of Vertebrate Life," and chiefly "Types of Mammalian Life." As a matter of fact, the work is exclusively restricted to those groups of Vertebrates possessing limbs differentiated into the full number of segments, and consists mainly of essays on the leading groups of Mammals; certain chapters being devoted to Birds, Reptiles, and Amphibians—apparently with the intention of pointing out the essential differences of the members of these groups from Mammals. As a whole, the work may be regarded as a companion to the author's recently-published "Elements of Ornithology," with a chapter on Birds which, in our opinion, might have been advantageously omitted.

The author commences his subject with a well-written dissertation on monkeys, as the representatives of the highest order of Mammals; and in the next chapter takes the opossum as his text for a sermon on the lower members of the same class. The next three chapters are devoted to the representatives of the lower Vertebrates coming within the scope of the work, namely, Birds, Reptiles, and Amphibians; while in the sixth chapter the author reverts to Mammals, taking the Carolina bat as a typical example of the Chiroptera. Then follows a chapter headed the American bison, in which the Ungulates are dealt with; while another, under the title of the racoon, introduces us to the Carnivores; and a third, designated the sloth, treats of the Edentates. In the tenth chapter, the title sea-lion appropriately designates a dissertation on the seals in general; while of the two remaining chapters, one is devoted to whales and sirenians (called by the author "mermaids") and the second treats of such Mammals as are not mentioned in the foregoing sections. The last chapter, by the way, is entitled "the other beasts," which leads us to remark that, in our opinion, it is decidedly a pity the author has thought fit throughout the work to follow the lead of the late Professor Parker in employing the objectionable term "beasts" in place of the now familiar mammals.

The work is fully illustrated and pleasantly written, and is admirably adapted for such readers as wish to gain a general idea of

the leading types of mammalian life, and their relations to birds, reptiles, and amphibians, without being encumbered with descriptions or systematic arrangement. The author has, moreover, exercised a wise discretion in avoiding, so far as possible, the use of technical names, which renders the book far more pleasant reading than several popular works we could name, and it is almost needless to mention that he is in general a master of style. We venture, however, to suggest that the less frequent use of the word "nevertheless" would save some weariness to the reader, and that when it is used the punctuation ought always to be the same (see pp. 150, 151).

The mention of punctuation reminds us that Professor Mivart stands in sad need of a proof-reader, not only in the present, but in some of his other works. For instance, on page 60 we are informed that the marsupial mole hails from America, while on page 182 we are told that tame gayals exist in certain parts of Spain! Again, on page 349 Soumerat stands for Sonnerat, while, if we mistake not, on page 292 Seaman does duty for Scammon. Moreover, it would have been well if the author had made up his mind on what plan he intended to transliterate Greek names, and we should not then have had *Chirotes* in one place (p. 146) and *Cheiromeles* in another (p. 165).

There are, however, other and more serious errors in the book which cannot be laid to the charge of the long-suffering printer, and for some of which the author will find it difficult to plead any adequate excuse.

One of the most glaring of these blunders occurs on p. 3, where the author states that the American monkeys, as compared with their Old World allies, "have developed an additional wisdom-tooth." And again, on page 32 we read that "the monkeys hitherto noticed, whether they have two or three wisdom-teeth on either side of each jaw, all agree with us in having three grinders, which have milk predecessors, and are technically known as premolars. But the marmosets alone have only two such on either side of either jaw." Now, in the first place, it is totally inadmissible to speak of all the molars as "wisdom-teeth," that name applying solely to the last of the series. Next, with the exception of the marmosets, all monkeys and apes, like ourselves, have three molars (wisdom-teeth); while those of the Old World have two premolars. The New World monkeys differ in developing a third premolar; while the marmosets are distinguished from these by the loss of the last molar, a true "wisdom-tooth," and thus have three premolars and two molars, and not, as the author states, two of the former and three of the latter teeth. Such a hopeless muddle as Professor Mivart has made of a simple subject would be hard to equal; and it is doubly to be deplored in a work intended to instruct the weaker brethren. It is, indeed, instruction (!).

We have already mentioned two slips in regard to distribution which may probably be attributed to careless proof-reading; but what are we to say with regard to the statement on page 184 that "the goats are exclusively confined to Southern Europe and Northern and Central Asia"? Has the author never heard of *Capra siniatica* of Egypt and the Sinaitic Peninsula, of *C. walie* of Abyssinia, or of the Nilgiri wild goat? Then, again, the statement on page 320, that the common rorqual is the largest mammal, is manifestly incorrect, the creature which has the honour of occupying this position being the blue whale (*Balænoptera sibbaldi*).

Other statements show want of reading on the part of the author.

For instance, we find it mentioned on page 9 that the apes obtained during Hanno's "Periplus" were probably gorillas. Now it has been conclusively shown that the place where Hanno touched was Sherboro Island, in north latitude 7° 30", or 4° 60" north of the range of the gorilla! If the creatures then obtained were anthropoids at all, it is, therefore, evident that they must have been chimpanzees. So far, however, as we are aware, no one has disproved the conclusion reached by Mr. Winwood Reade, that the so-called "gorillas" were really dog-faced baboons.

Similarly we notice on page 352 a repetition of the old legend as to the harmonious relations existing between the prairie-dogs and the burrowing owls (*Speotito*), whereas Dr. Elliot Coues, and after him Captain Bendire, have expressly stated that the owls certainly prey on the young, and probably sometimes on the adults, of the "dogs." We believe, moreover, that the idea of flying frogs (p. 103) has been proved to be a myth. Then, again, we are told that the European beaver never makes dams, although it did so in the 13th century. Has the author, we may ask, any reason to discredit Meyernick's account of the dam-building beavers near Magdeburg in 1829?

In another passage (p. 116) the author observes that when "we descend to the Lias and Trias and Carboniferous rocks we come upon a great variety of" Labyrinthodonts. Now, although one of these creatures occurs in the Lower Jurassic of Russia, we are unacquainted with any Liassic representatives of the order, so that it is, in any case, incorrect to speak of Labyrinthodonts as occurring "in great variety" when we reach that formation. Later on (p. 373) the author does injustice to himself in stating that chevrotains—without any reference to the fact of the generic distinction of the African and Asiatic forms—are common to the Ethiopian and Indian regions; while, when giving hunting-leopards as distinctive of the latter region alone, he is incorrect.

Most of the illustrations appear to have been specially executed for the work, but we regret that we cannot congratulate the artist on the result of his efforts, none of the figures being good from an artistic point of view, while some—especially those of the seals—are positively bad. It would, moreover, have been better had the artist paid a little more attention to the relative sizes of the animals he depicts. For instance, it is decidedly misleading to find the figure of "the smallest anteater," on p. 256, far larger than that of "the great anteater," two pages back; and a similar remark will apply to the proportions of the koala, on p. 49, compared with those of the thylacine, on p. 47.

While commending the book as a readable and instructive one to those desirous of gaining some knowledge of vertebrate zoology, apart from technical details, we cannot but regret the presence of the errors alluded to above, which, we fear, will be accepted as gospel when coming from the pen of a zoologist of the high standing of Professor Mivart. R. L.

A History and Description of the Modern Dogs (Sporting Division) of Great Britain and Ireland. By R. B. Lee. 8vo. Pp. xiv. and 584. Illustrated. London: Horace Cox, 1893.

Till the appearance of Darwin's classic works on this subject, the study of the various groups of domestic animals, and the extraordinary modifications which have been produced in them by careful breeding,

were far too much neglected by zoologists; but at the present day these narrow views have been, to a great xtent, abrogated, although there are still some naturalists who affect to despise the subject. To those, however, who, like ourselves, are of opinion that a full a cquaintance with domestic animals is essential to a right understanding of evolution, the appearance of the present volume will be welcome as showing to what extent our modern dogs are still being modified under the selective care of the breeder. The work—which treats only of the breeds more or less closely connected with sport—is, of course, written for the breeder and sportsman rather than the naturalist, but the latter will be able to select the portions of special interest to himself and skip the remainder.

An especial feature of the book is the excellence of the illustrations, which are in the form of plates by one of the photo-engraving processes, and are far superior to those in any other English work of similar size. As a rule, no particular dog has had his portrait taken for the group he represents, but a "generalised" dog is in most cases depicted, and in this the author has been decidedly well-advised.

Mr. Lee's remarks on the scenting power of the bloodhound—in the course of which various old stories are disposed of—will be found of special interest, while as an instance of remarkable modifications produced of late years, the reader may refer to the account of the modern English breed of the dachshund. Following the practice of English shows, the author considers the great Dane as inseparable from the German boarhound; but if he will turn to the dogs depicted under these names in the third edition of Brehm's "Thierleben," he will find a considerable difference between them; and this is surely a subject in which the views of foreigners ought to receive a large amount of weight. We are glad to see that the author endorses the view that one of the so-called Irish wolfhounds was a boarhound.

The work ought long to remain the authority on the subject of British sporting dogs, and we hope that it may ere long be followed by a companion volume from the same pen on the other groups.

R. L.

THE FOOD OF PLANTS: An Introduction to Agricultural Chemistry. By A. P. Laurie, M.A., B.Sc. Pott 8vo. Pp. ix. and 77. With 15 Illustrations. London: Macmillan & Co., 1893. Price 1s.

A NEAT little primer, "written as an experimental introduction to Agricultural Chemistry for beginners," and embodying a number of simple experiments by which the principles of plant nourishment are put before the student.

The idea of the book is an excellent one, theory and practice being happily combined, but occasionally the author's statements are somewhat crude or even misleading, while a sentence ending with a preposition and the use of "and which" are not to be commended.

In Chapter I., the relation between the plant and water is well and simply illustrated, but it is wrong to speak of the young root as "covered with little branch roots or *hairs*," branch roots are one thing and hairs another, and to make the two synonymous is to inculcate error for some one else to eradicate. Again, we question the wisdom of teaching that the soil holds "the water just as a sponge does"; when one of the points to be impressed on the embryo

agriculturist is that the interstices of the soil must be filled with air, and not saturated with water like a sponge.

Chapter II. deals with food obtained by the plant from soil, and is excellent as far as it goes, the student being taught that soluble substances only can pass into the roots, and that there are certain substances in the soil, soluble in water, which are absorbed by the roots for food, but no reference is made to the greatly increased power of solution afforded by the acid reaction of the root-hairs, or the carbonic acid gas dissolved in rain-water.

The nature of the soil, the "substances of a leaf," the composition of the air, and the absorption of carbonaceous food therefrom, are well explained, though surely in the heading on p. 40, "The Seed obtains Food from the Air," we should read Seedling for Seed. In the seventh and last chapter, on the source of the nitrogen required by plants, the author is a little too crude and brief in his explanation of nitrification and the tubercles on the roots of leguminous plants, "full of minute creatures, to which the name of bacteria have (*sic*) been given." The two appendices "Notes on the Experiments," and "On the use of the balance," contain useful hints.

Mr. Laurie should have got a botanical friend to look over his proofs, and thus have avoided the serious errors which mar what might have been a capital little book.

PRÉCIS DE TÉRATOLOGIE, ANOMALIES ET MONSTRUOSITÉS CHEZ L'HOMME ET CHEZ LES ANIMAUX. Par L. Guinard, Chef des Travaux de Physiologie a l'Ecole vétérinaire de Lyon. Précedé d'une préface par M. le Dr. Camille Dareste. Pp. 512. With 272 figures. Paris: J. B. Baillière et Fils, 1893. Price 8 francs.

AFTER the great work of Is. Geoffroy-Saint-Hilaire, published from 1832–1837, for many years there was little done in Teratology. A few isolated papers contained descriptions of unusual monsters, but the attention of naturalists was so absorbed by the new light thrown on the domain of normal Nature that her freaks and sports were almost disregarded. Then, in quite recent years, came Camille Dareste's series of studies on the artificial production of monsters, and a number of other studies of which Mr. J. A. Thomson gave an interesting *résumé* in the last number of NATURAL SCIENCE.[1] The more recent part of the study of monsters concerns itself with the conditions of their production, and has been called by Dareste Teratogeny, as distinguished from Teratology, the record and classification of the types. The present volume is limited designedly to Teratology, and is meant to be a compact and convenient guide to the known types of abnormality; for, if it be, at the first glance, a paradox to write of types of abnormality, or to discuss the laws of disorder, a very brief study of M. Guinard's volume will convince that monsters are not the frolic of chance, but an inverted or tip-tilted regularity. Some are caused by mere mechanical pressure or torsion, and cause and effect tread on each other's heels as closely as the pressure of a boot and the growth of a corn; in others unusual condition, raising or lowering the temperature beyond the eurythermal limits, alteration of the natural position, conditions of strain (as on a centifrugal machine) tend to produce abnormalities, although the nexus of cause and effect is invisible; but, however produced, the productions fall readily into categories. M. Guinard gives, in a series of chapters, accounts of simple anomalies, classified according to the organs or

[1] NATURAL SCIENCE, vol. ii., pp. 294–306.

tissues chiefly affected; of complex anomalies, like hermaphroditism, apparent or real; and of monsters single or double. The illustrations are very useful, and as appropriately horrible as the subject demands. There is an index; full table of contents; and glossary of terms.

Altogether it is a most useful book, and is to be commended to all anatomists and embryologists. P. C. M.

THE EMBRYOLOGY AND METAMORPHOSIS OF THE MACROURA. By W. K. Brooks, Professor of Animal Morphology in the Johns Hopkins University, and F. H. Herrick, Professor of Biology in Adelbert College. National Academy of Sciences, vol. v., Fourth Memoir. Pp. 325-576, with 57 Plates.

IN this beautiful memoir the authors have made a notable contribution to knowledge of the development of the Macroura. Animals which, like the Crustacea, pass through a series of successive moults during their larval life, naturally exhibit a series of gradations leading towards the adult condition. Fritz Müller and Claus showed that in this group a very large number of orders, families, genera, and species display the relation between ontogeny and phylogeny. At the same time, Claus pointed out that an equally large number of adaptive larvæ are to be found, and in the introduction to this memoir Dr. Brooks strongly insists on the necessity of careful and exhaustive study in each case before the significance of a larval history can be understood. The survival of larvæ living free in water depends on their adaptation to their present environment, and so recapitulation of ancestral stages in the present metamorphosis must depend to a large extent on the persistency of those external conditions to which the larvæ were originally adapted.

The chief forms studied in this memoir are *Stenopus hispidus*, *Alpheus*, and *Gonodactylus chiragra*. In addition to anatomical and embryological detail, a number of side issues of general interest turn up. Thus one would expect (says Dr. Brooks) that the least specialised species are the most widely diffused. But the larvæ of *Stenopus hispidus*, although *Stenopus* is one of the most highly specialised of the Crustacea, are most widely distributed. Specimens from the Indian Ocean and the South Pacific agree with those from the West Indies down to the most minute marking. "It is well protected from enemies by a thorny armour of hooked spines which cover all the upper surface of its body and limbs, and, as all the hooks point forward, the attempt of an enemy to swallow a *Stenopus* must be difficult and painful."

Many of the species of *Alpheus* live as parasites in the canal of sponges, and individuals of the same species living in different species of sponges may differ greatly in colour and habits.

In *Alpheus* and in *Stenopus*, after impregnation takes place, the nucleus divides and a syncytium of eight nuclei is found. In *Alpheus* there is a slight invagination and a modified gastrula is found. But in all the accounts of development given readers will be struck by the want of distinctness in the separation and formation of the germinal layers. It certainly seems as if the familiar successive origins of the germinal layers are present only in a very vague manner in this group.

The account of the larvæ of *Gonodactylus chiragra* is specially valuable, as so little is known of the Stomatopoda. In the present work, Dr. Brooks is able to confirm most of the results he reached in the "Challenger" monograph.

CONTRIBUTIONS TO THE ANATOMY OF THE ANTHROPOID APES. By Frank E. Beddard, M.A., Prosector to the Zoological Society of London. *Trans. Zool. Soc.*, vol. xiii., part v., 1893 (pp. 177–218, plates xx.–xxviii.).

THERE will be few people who failed to hear at least of the existence of the celebrated chimpanzee "Sally"; her reputation, like that of some others, higher, perhaps, in the zoological scale, was made not entirely by her own unaided merits, but was largely due to the assistance of the Press. Her lamented death gave rise to obituary notices more extensive than those commonly devoted to archbishops. Human greatness, however, is apt to be but transitory; and apparently also the same is the case with anthropoid apes; but the waning reputation of "Sally" will, perhaps, be renewed by the above-quoted memoir upon her anatomy. "Sally" belonged to a species of Chimpanzee which was originally described by M. du Chaillu as *Troglodytes calvus*; the species was not by any means universally accepted at the time of its description; but the present paper sets at rest any doubts which may be still entertained upon the matter. The question of the species of Chimpanzees is one which is not yet decided, nor is it yet ripe for settlement; the material in the way of skulls and skeletons available in the museums of Europe is not sufficient. A great many different names have been at various times applied to supposed species; but at the present moment no more than two can be regarded as having been definitely proved; those are, of course, *Troglodytes niger* and the species which forms the subject of Mr. Beddard's memoir. This ape was remarkable not only for being the only individual of her kind ever exhibited at the Regent's Park menagerie, but also for her intelligence and long life. From the condition of the teeth it appears that she was about eleven years old; at any rate she lived in the Gardens for no less a period than eight years, which is, for an anthropoid, quite phenomenal. "Sally" finally yielded to a complication of diseases which rendered it impossible to give any account of the viscera; this is to be regretted, as the chances of studying the visceral anatomy of this species are not likely to be frequent. The absence of any account of the internal structure of the animal is, however, made up for by an account of the muscular anatomy and of the brain. The latter organ is well figured in several aspects; as might be expected, it does not show any great differences from that of the common Chimpanzee; nor is the muscular anatomy particularly characteristic; such points of difference as exist are dwelt upon by the author. The reader may be interested to hear that the brain is now to be seen in the Oxford University Museum; the skin has been stuffed and is in the Museum of the Hon. Walter Rothschild at Tring. A great feature of the paper, as is, indeed, usually the case with the excellent publications of the Zoological Society, is the illustrations; we do not intend to imply by this that the text in any way falls short of the illustrations; but it certainly is the case that the plates with which the publications of this Society are adorned are of their kind unsurpassed. We have in the present paper figures life-size of the full face, of the head seen from above (evidently to show its dolichocephalic character), of the hands and feet, and a whole plate and part of another is devoted to the brain. The second part of Mr. Beddard's paper deals with another anthropoid, who was familiarly known by the name "George"; this animal was thought to be a representative of the smaller Orang, *Simia morio*; Mr. Beddard, however, is not convinced of the correctness of this view, which was partly based upon the supposition (erroneous, as it now turns out) that the ape was elderly;

"George" appeared on an examination of the teeth to be after all a mere baby. A beautiful coloured lithograph of the head occupies one plate, and the hands and feet are also illustrated. It is, perhaps, unnecessary to say that the memoir is entirely technical in character, though the first few pages, occupied by some discussion of the species of Chimpanzees, will be found to be of somewhat more general interest.

THE BIRDS OF DERBYSHIRE. By F. B. Whitlock, with Notes by A. S. Hutchinson. 8vo. Pp. vi. and 235. Illustrated. London and Derby: Bemrose & Sons, 1893.

THE fact that no complete history of the bird-life of such an important and interesting county as Derby has hitherto appeared, affords ample justification for the issue of the little volume before us, in which the subject appears to be as fully treated as materials permit. The author calls attention, however, to the lack of local observers in the wilder districts of the country; and it may be hoped that the result of his labours will be to stir up other ornithologists to complete our knowledge of the subject. To show how necessary is local observation, it may be mentioned that, according to the author, till quite recently the merlin was considered to be only a casual winter visitor to the county, whereas it actually breeds on the high peak. It is to be regretted, however, that the efforts of the pestilent gamekeeper have, within the last year or two, almost, if not entirely, exterminated this falcon from its Derbyshire haunts. Of the five plates illustrating the work, four are views of scenery, while the fifth represents a remarkable variety of the corncrake.

Although the work is, of course, to a great extent of local interest, it contains many observations bearing on the subject of British ornithology in general, and must therefore be of value to all students of that science. R. L.

FOREST TITHES AND OTHER STUDIES FROM NATURE. By "A Son of the Marshes." 12mo. Pp. 208. London: Smith, Elder & Co., 1893.

THE tendency of modern science to become a mere record of "section-cutting" and of the dry details of comparative anatomy is so marked, that we have sometimes feared whether the old-fashioned field-naturalist was not in as much danger of extermination as many of the animals in which he took such a delight. Happily, however, the observer who writes under the *nom de plume* of "A Son of the Marshes" shows that the field-naturalist still exists among us in his best form; and his observations on the varied types of animal life that may still be seen within easy access of London affords us another example of the truth of the old story of "Eyes and no Eyes." Most of the essays in the little volume before us have already seen the light as separate articles, either in the *Times*, the *Cornhill*, or *Blackwood*, where many of our readers have, doubtless, ere this, perused them with pleasure. All who have done so, we think we may safely assert, will have still greater gratification in seeing them in their present guise, as it would have been a thousand pities had such delightful reading remained buried in the pages of a magazine or the columns of a newspaper. Whether in describing the otter, as he slinks alone the river-bank, with which his coat harmonises so closely in colour as to render him invisible to unpractised eyes, or in recording the movements of the

heron watching for trout at some broken weir, the author is equally at home; and we can thoroughly recommend his book to all lovers of Nature as a living picture of many British animals in their native haunts. R. L.

THE RECRUDESCENCE OF LEPROSY AND ITS CAUSATION. By William Tebb. 8vo. Pp. 412. London: Swan Sonnenschein, 1893. Price 6s.

THIS is a book written to show that vaccination is accountable for the spread of leprosy. We go so far with the author in entirely condemning the system of arm-to-arm vaccination, for it is definitely certain that though the health of the child may be satisfactory, in 99 cases out of 100 there is no means of ascertaining the condition of its progenitors. The author has collected together much information concerning leprosy that is valuable to the professional as well as interesting to the general reader, and concludes by observing that, as the disease is "practically incurable, it behoves all interested in the public well-being to do their best to prevent its diffusion," which he considers is largely due to the practice of vaccination.

EVOLUTION AND MAN'S PLACE IN NATURE. By Henry Calderwood, LL.D., F.R.S.E., Professor of Moral Philosophy, University of Edinburgh. Pp. 349. London: Macmillan & Co., 1893. Price 7s. 6d.

IN the minds of many some of the value of a book on Man's Place in Nature, written by the holder of an endowed chair of Moral Philosophy in a Scotch University, will be discounted from the outset. The Professor must hold a brief for his client, and his client is Man as a Moral Agent. The interest of the book or the lectures for such readers resolves itself into a curious contemplation of the byeways by which the author shall arrive at the known goal. The Scotch Professor of Moral Philosophy, at whose feet this reviewer had the duty of sitting, cleared a Stanley path through the forest of Science, raucously scaring the poor pigmies, shooting down the detestable niggers, hacking through the forest, marching straight with blundering and boisterous declamation on his inspired mission. Quite other is Professor Calderwood. On his way to his conclusions he dallies with variation and environment, embryology and evolution, very agreeably passes the time of day with Huxley and Hæckel, Eimer and Weismann, Helmholtz and Darwin, and says what very intelligent fellows (in a moderate and non-moral way) they all are. But when the real business comes on (in the last chapter) the naturalists rather fade away, and are replaced by Aristotle and Plato, Butler and Green, Shakespeare and Holy Writ, and Professor Ray Lankester; for Professor Ray Lankester has written on "Degeneration"—a branch of the theory of Evolution held in high honour among the dogmatic.

Professor Calderwood accepts the principle of evolution so far as man's body is concerned. With the proviso that life itself is inexplicable on mechanical or chemical grounds, and with the usual quotation from Huxley against abiogenesis, readily enough he concedes that there is an enormous preponderance of evidence in favour of the organism of man being of the same kind, and descended in the same way, as other organisms; but in addition to this organic life, he claims for man "rational life" unexplained by science and giving to man a dominance and position in nature totally unex-

plained by his physical nature. To arrive at this, he goes over a large field, where it would be impossible to follow him in detail, pointing out a multitude of distinctions—as, for instance, that man is a producer, animals only consumers, animals are modified by environment, man can modify environment, animals have sensibility, man rationality, and so forth. All these are points of discussion that, however Professor Calderwood trick them out in new settings, have been debated abundantly; and it is no unfair thing to say that one knows beforehand how the facts will appear to different men. Professor Calderwood and his kind exaggerate the mechanical rigidity of "instinct," dilate on the breaking down of animals in unaccustomed circumstances, and dwell on the glories of Shakespeare and Newton. The Darwinian and his kind dwell on the gradations by which "instinct" seems to pass into deliberated adaptation of means to ends, lavish adulation on the intelligence of the dog, and grovel before the animal stupidity of the savage.

Professor Calderwood, and with him many others, still fear what Carlyle called "the monkey damnification of mankind." They have lost the old fears now that they have lost the battle over the evolution of the body: they redouble them on the question of reason. To this reviewer it seems inexplicable that as all men know we have (to use the popular terms) reason, conscience, power of making moral judgments, and the apparent choice between them, it should matter whether these came suddenly or gradually; and it appears extraordinary that when all the processes of the world (including evolution) are a "sovereign wonder of superhuman fixedness of law," any writers should be so concerned to establish jerkiness in the law. Thus, Professor Calderwood with considerable pride, leads up to an argument for the existence of deity by coming to the conclusion that he has shown the incompetence of science to explain life, mind, and reason. Then he goes on: "There is a Power operating continually in Nature, which does not come within range of the observation possible for scientific modes and appliances, yet to which science is ever indirectly bearing witness. This Power has manifested itself at the most impressive periods in the world's history, first at the appearance of Organic Life, again on the appearance of Mind, and again on the advent of Rational Life." A smoothly-running process, an organic growth of the world of matter and life, a growth on which the fruits of life, of mind, and of soul appear in their due season, seems to Professor Calderwood less compatible with theology than an interrupted and repeatedly tinkered Nature.

This book is devoid of scientific value, and no doubt was not intended to have any; and while it cannot conceivably help to support the faith of anyone who has not in his mind a thought-tight partition between faith and reason, it may be a consolation to many good stupid people sorely tried in their faith.

THE YEAR BOOK OF SCIENCE. Edited for 1892 by Professor T. G. Bonney, D.Sc., LL.D., F.R.S. 8vo. Pp. 519. London: Cassell & Co., 1893. Price 7s. 6d.

IN this second volume of the Year Book of Science the unevenness of the different sections is still very noticeable, and the book might be greatly improved by stricter editing. We observe that the authors of the different parts take various views as to the objects of

this annual. One section or chapter may be all that it ought to be, and may give a good outline of the year's advance in some branch of science; but the next section is treated quite differently, and leaves us in entire ignorance of the advances made, except in some small field specially cultivated by the author.

As examples of good outlines we may mention the Mineralogy and Petrology, by G. T. Prior; Stratigraphical Geology, by H. B. Woodward; Palæontology (Vertebrate), by R. Lydekker; Palæontology (Invertebrate), by J. W. Gregory. Then comes a section on Palæobotany, by T. Hick, in which the whole of the prolific literature published during the year on fossil plants is ignored, except a few species from the Coal-measures, and a single Alga from the Permian. To this author the whole Tertiary and Secondary flora is non-existent!

The sections on Physical Geology and Geography, by H. G. Seeley; and Anthropology, by H. G. Seeley, are very imperfect. Biology (Animal), divided into several sections, and treated of by C. H. Fowler, C. S. Sherrington, R. I. Pocock, and R. Lydekker is much better.

Biology (Botanical), is divided between W. B. Hemsley, who takes Systematic and Geographical Botany; G. Massee, who writes Morphology and Biology of Plants; D. H. Scott, who treats of the Minute Anatomy of Plants; and F. E. Weiss, who abstracts the literature on the Physiology of Plants. But in some unaccountable way all these botanical writers have forgotten to mention Lubbock's large monograph on Seedlings! Each probably thought that it would be in another section, and so it was missed altogether.

M. Martinus Nijhoff, of the Hague, has just issued a "Catalogue de Livres sur les Possessions Néerlandaises aux Indes Orientales et Occidentales sur l'Empire Indo-Britannique, les Possessions Espagnoles, Françaises, Portugaises, la Chine et le Japon, l'Australie." The catalogue is arranged under headings, and will be of assistance to those interested in these countries. Pp. 280.

The Administrator's *Annual Report of British New Guinea from 1st July, 1891, to 30th June, 1892*, has just been issued as a Blue-book by the Colony of Queensland. The affairs of the Possession are reviewed under the separate heads of Legislation, Administration of Justice, Administrative Visits of Inspection, Government Property and Works, Establishments, Meteorology, Trade, Mission Work, Lands, Prisons, Finance, Native Dialects, Scientific Contributions, and Reports by Officers. It contains 113 pages of text and 9 maps. No one can read this official document without being impressed with the extraordinary amount of information it contains of the highest scientific value. This young colony is more favoured than most in having an officer in charge who is not only a highly efficient Administrator, keenly alive to all the best interests of the natives, but a man possessed of an unusually wide range of attainments. Under the head of Administrative Visits of Inspection, the dispatches contain an amount of geographical, geological, zoological, anthropological, and linguistic data, collected chiefly by the Administrator himself, of unusually high value. The Report contains also sub-reports by officers specially commissioned for the purpose, *i.e.*, by Mr. Moreton on his unsuccessful Expedition from Philipps Harbour towards

Mount Suckling; on Meteorology, by Mr. Clement Wragge, Government Meteorologist of Queensland, and a long and valuable report on the Geology of the Possession by Mr. Gibb Maitland, of the Queensland Geological Survey.

Meteorological observations are taken at four stations in the Possession, at the capital, at Mekeo, and at the headquarters of the Eastern and Western districts. "Unfortunately, complete records can hardly be procured, except at Port Moresby, owing to the fact that officers at other stations are often necessarily absent from their headquarters on other duties." At Port Moresby the mean atmospheric pressure for the year 1891, at 9 a.m., was 29·919 inches. The grand mean temperature was 83·0 F.—the highest shade (2nd February) 96·2°, the lowest (27th August) 72°. The rainfall for the year was 72 inches, of which 53 inches fell in the first five months of the year. It rained on 95 days. At Samarai (in China Straits), from April, 1891, to March, 1892, 126·5 inches fell on 174 days. During the three months of July, August, and September a fall of 59 inches was recorded there, while only 15 inches fell at Port Moresby. Dufaure Island is the meeting place of the Port Moresby and Samarai climates. Sir W. Macgregor contributes, as an appendix, a vocabulary of the Kiriwina dialect in the Trobriand Islands, with observations on its grammar, and the Rev. W. Bromilow one on the aboriginal vocabulary of Dobu.

In concluding his Report, which, we repeat, is of great scientific value, the Administrator remarks: "The results of native administration appear, on the whole, fairly satisfactory for the year. In not a few districts natives are settling down to more systematic work in preparing exports for the trader. . . . In some of the districts where native murders were fearfully common two or three years ago, this crime has completely disappeared, or become very rare. No doubt what in these days may be called 'much time' is required to acquaint the Papuan tribes with the working of the Government, and the impartial and beneficial action of law. But they possess an aptitude for such tuition which cannot but appear to be extraordinary to any person acquainted with the history of civilisation among the present existing cultured races of our globe. If the Papuans are allowed anything like reasonable time—a time which compared to that required for such a purpose by other races would be a very short period—there can be no doubt that they will become a very considerable, if not an important, unit in the Australasian dominions of the Queen." These observations strikingly confirm the opinion of the natives formed by Mr. H. O. Forbes during his intercourse with them, even before Government supervision had begun to take effect, and expressed in *Blackwood's Magazine* for July, 1892.

The English Catalogue of Books for 1892 has just appeared (Sampson Low, five shillings). We are glad to see that the plan of arrangement adopted last year is adhered to; there is no system more convenient for a reference book than the purely alphabetical. We would suggest, however, one improvement, which is that all the miscellaneous collection at present under "Transactions" should fall into their proper alphabetical position in the next issue. "Transactions" is clearly wrong, "Serials" or "Academies" if you will, but not "Transactions."

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

MR. JOHN STORRIE has resigned the Curatorship of the Cardiff Museum.

DR. JORDAN has arrived at Tring from Hildesheim, and will take charge of the Entomological collections belonging to the Hon. Walter Rothschild.

MR. GEORGE MASSEE has been appointed to succeed Dr. M. C. Cooke in the Kew Herbarium. The second volume of his *British Fungus Flora*, dealing with Agaricineæ, has appeared.

MR. F. A. BATHER, of the British Museum of Natural History, has obtained special leave of absence on account of a weakness of his eyesight. Mr. Bather will spend the greater part of four months on the sea and will visit Teneriffe, Cape Town, Hobart Town, Sydney, Japan, San Francisco, Burlington, and New York. He left on March 30.

MR. A. SMITH WOODWARD is on his way back from the Lebanon, where he had gone to study the important deposits of fossil fish of Cretaceous age.

THE funds of the McGill University have been increased in the sum of 160,000 dols., the gift of Mr. Molson and Mr. Donald Smith.

THE syllabus of work for the "Edinburgh Summer Meeting" for the seventh Session, July 31—August 26, 1893, has just been issued. The business of Section C (Natural Science) consists of ten lectures on Comparative Psychology, by Professor Lloyd Morgan; Hygiene, ten lectures by Dr. Louis Irvine; Biology, twenty lectures by J. Arthur Thomsom and Norman Wyld; Practical Botany (including Field Work), twenty meetings, Robert Turnbull; Field Geology, ten excursions directed by Norman Wyld; Practical Zoology (at Granton Marine Station), twenty meetings, J. Arthur Thomson; Regional Survey of Edinburgh and neighbourhood, twelve lectures.

THERE will also be delivered twenty lectures on Contemporary Social Evolution by Professor Geddes, and many other courses on various branches of Education, such as History, Social Science, Music, Elocution, &c. Particulars can be obtained by application to J. Arthur Thomson, M.A., University Hall, Edinburgh.

We are glad to observe that a suitable inscription has been recently placed on the pedestal of the bust of Sir Richard Owen, which has a place of honour in the Pavilion of the Palæontological Gallery at the Natural History Museum. This bust is a plaster cast of the Hamo Thornycroft marble (1880), preserved at Sheen Lodge, and was presented to the Museum by the family about four years ago. The inscription reads as follows:—

SIR RICHARD OWEN,
K.C.B., M.D., D.C.L., LL.D., F.R.S., &c., &c.
Superintendent of the
Natural History Departments
of the British Museum,
from 1856—1883.

Born, 20th July, 1804. Died, 18th Dec., 1892.

"Accesserunt ossa ad ossa, unumquodque ad juncturam suam."—
Ezech., xxxvii., 7.

We are informed by Dr. Henry Woodward that the appropriate quotation from the Vulgate was selected by the Rev. Professor Bonney.

The fourth annual meeting of the Museums' Association, which is to be held in London under the Presidency of Sir W. H. Flower, K.C.B., will commence on Monday, July 3, and probably extend over four or five days. The mornings will be devoted to the reading and discussion of papers, but the afternoons will be given up to the inspection of various metropolitan museums, under competent leadership.

A Geological Society has recently been organised in Washington for the presentation and discussion of topics of interest to geologists. The constitution and standing rules were subscribed to by 109 founders at the first public meeting, March 8, 1893. Its members are of two classes, active and corresponding. The annual subscriptions of the first are 2 dols., and of the second 1 dol. Meetings will be held on the second, and generally also on the fourth Wednesday of each month, from October to May inclusive. The journals and bulletins of the various societies appear to furnish sufficient opportunity for the publication of papers read before the Society, so that for the present the Society will not undertake to publish the papers presented. It will probably issue one bulletin each year containing the address of the retiring President, and such other matter as the Council directs. The officers, elected February 25, 1893, are C. D. Walcott (President), S. F. Emmons and W. H. Holmes (Vice-Presidents), A. Hague (Treasurer), Whitman Cross and J. S. Diller (Secretaries); the Council being G. F. Becker, T. M. Chatard, G. H. Eldridge, G. K. Gilbert, G. P. Merrill. We should like to have seen the organisation of local excursions among their objects, for field observation is an essential to the increase and diffusion of geological knowledge.

The collection of Lepidoptera (chiefly Micro-Lepidoptera) formed by the late H. T. Stainton has been presented by his widow to the Natural History Museum. The presentation is the more valuable as it includes the original drawings and papers illustrative of the specimens.

The Zoological Society of London has just issued its Annual Report of Accounts for 1892. We are glad to note a slight improvement in gate-money (£272) over 1891, which points to an increased appreciation in the efforts made by the Society to render the Gardens interesting to the public. It is curious to see a falling-off in the elephant rides of £3 19s. 4d., but still the comfortable amount of

£606 was received from this source during the year. There is a considerable improvement in the admission fees, which points to an encouraging influx of new Fellows. Turning to the payments, we find that out of an expenditure of £25,968 no less than £17,618 was expended on the Gardens, an amount which speaks eloquently for the excellent standard maintained. Of this amount, £843 was spent on animals and their transport, £3,974 on food, £3,468 on salaries and pensions, £765 on horticulture and garden-work, £2,628 on works and buildings, and £3,466 on menagerie expenses. We have commented on the expense of the *Zoological Record* on page 324.

Mr. Edward Best, who has been on the staff of the Geological Survey for 38 years, has now retired. His place as acting secretary is taken by Mr. W. Topley, and Mr. A. C. G. Cameron has been promoted to the rank of geologist.

At the annual meeting of the Norfolk and Norwich Naturalists' Society, held at the Norwich Museum on March 28, Mr. Thomas Southwell was elected president. The address of the outgoing president, Mr. H. B. Woodward, was devoted to various local topics, principally geological.

The work of remounting the great series of microscopic preparations made by the late Professor de Bary, and acquired by the British Museum, is nearly completed. The slides illustrating the comparative anatomy of the vascular plants, and those dealing with the structure and life-history of the lower forms, have been successfully restored, and the Fungi, which have been left to the last, are three-parts done. The value to students of Botany of the presence of this great collection in this country can only be indicated, since it will always remain as the authentic basis of the great botanist's work.

Professor Herdman sends us the "Sixth Annual Report of the Liverpool Marine Biology Committee." The laboratory at Port Erin appears to be a flourishing institution, and a considerable amount of work has already been turned out. The Committee have secured the services of Mr. Henry Vanstone, of the Royal College of Science, South Kensington, as "resident Curator," and important additions have been made during the winter to the laboratory. A great number of students appear, from what we have heard, to be availing themselves of the opportunities of working there.

We regret to learn that Mr. T. D. A. Cockerell has found the climate of Kingston, Jamaica, too trying for his health, and has been obliged to leave the Museum there and return to his old home near the Rocky Mountains. Mr. Cockerell was doing some good work in economic entomology, and his departure will be a great disappointment to him and a distinct loss to the Kingston Museum. His address will be Las Cruces, New Mexico, U.S.A.

Mr. E. J. Bles has been appointed Director of the Laboratory at the Plymouth Marine Biological Station.

We hear that Professor Albert Gaudry has resigned the position of Professor of Palæontology at the Jardin des Plantes.

OBITUARY.

ALPHONSE LOUIS PIERRE PYRAMUS DE CANDOLLE.

BORN OCTOBER 28, 1806. DIED APRIL 4, 1893.

THE great Swiss botanist died at Geneva, in the old house in the Cour St. Pierre, which, built by one of two brothers who took refuge in Geneva at the Reformation, had been the home of the family for many generations.

He was born in Paris a few days after his father had beeh rejected on his second presentation to the Institute in favour of another botanist, much his inferior, Palissot de Beauvois, and in his autobiography published by his son in 1862 ("Mémoirs et Souvenirs"), Auguste speaks of the consolation he derived from the happy event. Two years after, Auguste went to Montpellier, where he succeeded Broussonet as Professor of Botany, but in 1816, owing to political troubles then rife in France, he resolved to return to his native town, at the University of which a chair in natural history was founded expressly for him. This he held till 1834, when he resigned, his place being taken by his son, Alphonse, the subject of the present memoir.

The name of De Candolle would always be an honoured one among botanists, even if Auguste had not had in Alphonse a son to carry on his great work and strike out new lines of his own, while, now the son is gone, Casimir, the grandson, a well-known botanist, lives to perpetuate its fame in the third generation.

Every botanist who has any knowledge of systematic botany, every gardener who lays any claim to scientific knowledge, knows and has proved the grand *Prodromus Systematis Naturalis Regni Vegetabilis* which, begun in 1824 by Auguste and finished in 1873 by Alphonse, will be a lasting memorial of their genius and perseverance. There was grit in these old botanists.

Nowadays when a man scores a paper in a few weeks or months, or perhaps the best part of a year, one looks back with admiration at Auguste de Candolle, no longer young, starting, in 1816, to monograph all the Orders of the vegetable kingdom according to the then but little known natural method. When, after five years of hard work, only eleven Natural Orders were complete, including, nevertheless, two of the most important—Ranunculaceæ and Cruciferæ—he

realised that the work was too vast, and started on the *Prodromus*, evidently, from its name, intended as an abridged form of the larger work. After the first few volumes, however, the *Prodromus* became itself a series of complete monographs, thus ultimately realising Auguste's original ideal. The first volume appeared in 1824, and the name of the father alone appears on the title-page of this and the six that follow. In his historical account in the seventeenth and last volume, Alphonse says his father was almost entirely responsible for the matter of the first seven volumes, at which he worked unceasingly for twenty years, from 1822 till his death in 1841. From the third volume onwards, however, he was assisted by his son, who, after his father's death, carried on the work for thirty-two years more, adding ten volumes. Though he received a great deal of help from other botanists, of whom he gives a list, with the number of pages due to each, he was solely responsible for the editing, and also worked up twenty-six of the monographs.

Folks may wonder, he says, why, with so many helpers, the work took so long. But monographers find their difficulties increased with the number of species, specimens, characters, and synonyms. Moreover, rarely are herbaria and books to be had at one and the same time without some hindrance or delay, libraries being often separated from the plants, or wanting in the more recent works. Here a most devoted botanist lacks either the books or plants, while there the botanist is himself wanting in zeal, method, or time for work. There are also authors who, having undertaken a monograph, do not forward it in due time, or even give it up altogether. This last failing in particular caused loss of time, and necessitated frequent division of the volumes into sections and transposition of the Orders, one (Artocarpeæ) having, at the last moment, to be abandoned, and, finally, the conclusion of the work, at the end of the dicotyledons, "ne tertiam botanicorum generationem occideret!" In the "Monographiæ Phanerogamarum," however, the number of missing Orders is gradually being reduced, seven volumes having been issued at the time of his death, while others are in active preparation.

The *Prodromus* includes 214 natural orders, 5,134 genera, and 58,975 species; 15 per cent. of the genera and 25 per cent. of the species are new. Of the 13,194 pages, the de Candolles (including Casimir, who supplied 260) wrote 5,950 pages, various curators of the Candollean Herbarium 1,475, and other botanists, among whom are three Englishmen—Bentham, Sir J. Hooker, and A. H. Weddell —5,769.

But the *Prodromus* was not the sole outcome of all these years. Until 1850 Alphonse occupied the Chair, instituted for his father, at the University of his native town, but in this year resigned it, in consequence of the state of political affairs in Geneva. In 1835 he had published an "Introduction to the Study of Botany," a book of two volumes, and in 1844 produced a third edition of his father's

"Théorie Elémentaire." In 1855 appeared his "Géographie botanique raisonnée," published in two volumes, and containing, in 1,300 pages, "an exposition of the principal facts and laws concerning the geographical distribution of plants at the present day." "In its aggregation of facts and results, and in their skilful marshalling," says a reviewer in the current issue of the *Gardener's Chronicle*, "it is comparable only with the works of Darwin, some of whose views were indeed something more than foreshadowed, by the great Swiss botanist." In 1867 he laid before the International Congress of Botanists at Paris his "Lois de la nomenclature botanique," which still forms the basis of modern botanical nomenclature. In 1880 appeared "La Phytographie," or the art of describing plants, a work of the utmost value to every systematist, and concluding with an invaluable list of the principal herbaria. His "Histoire des Sciences et des Savants depuis deux siècles" (1873) is a charming book, and quite intelligible to the general reader, while his "Origine des Plantes Cultivées," a translation of which is published in the "International Science Series," is also well-known.

These do not represent nearly all of the stately and courteous Professor's contributions to botanical science, but this is but a short biographical notice, not a bibliography, and no place can be found for his numerous papers dealing with very various aspects of botany.

De Candolle made several memorable visits to this country. When about two-and-twenty he came over to London, and leaving the metropolis in May went down to the West of England. Thence he worked up through Wales and the English Lakes to Glasgow, where he met Sir William Hooker, then Professor of Botany at the University. From Glasgow he went to Skye, and having done the island on foot, crossed to Inverness, and so back to London, where he arrived again in the autumn. Certainly a very creditable tour, considering the means of communication more than sixty years ago.

In 1866 he presided over the London Botanical Congress held at South Kensington, and in his address emphasised the great importance of an intimate relation between botany and horticulture. In May, 1850, he was elected a foreign member of the Linnean Society, of which he was at his death the senior foreign member.

In 1889 the Society conferred on him its gold medal, the highest honour it has to give. The President, when announcing the sad news at the meeting on the evening of April 6, mentioned that his son Casimir, who has done good botanical work, had been selected by the Council for nomination as a foreign member.

De Candolle loved the English and English ways, dining in English fashion and speaking English largely at home, and, in common with the rest of his family, was by no means friendly disposed to the political powers of his native city.

He was well and vigorous till within six months of his death,

when he began to feel old. His wife died some years ago, but he leaves two sons, to one of whom, Casimir, we have already referred as a well-known botanist.

WE regret to announce the death of the Rev. T. Wolle, pastor of the Moravian Church at Bethlehem, Penn. He was the author of books on the *Fresh-water Algæ of the United States* and on the *Desmids of the United States*. His work has value as that of a systematic pioneer and recorder of forms—a labour to which he brought great zeal and enthusiasm.

PROFESSOR Giuseppe Antonio Pasquale, who died at Naples on February 14, was Professor of Botany at the University, and Director of the Botanic Gardens. He was the joint author of a "Flora medica della Provincia di Napoli," published in 1841, and a "Compendio di Botanica," which reached a third edition in 1890. He also wrote an account of the "Flora Vesuviana," and other systematic and morphological papers, and compiled a "Catalogo del real orto botanico di Napoli."

CORRESPONDENCE.

I HAVE to thank Dr. Hurst for bringing to my notice his interesting suggestion as to how inoculation may be supposed to confer immunity from disease. The idea that the attenuating process produces, by selection, a new strain of microbe having a different physiological action, seems worthy of careful examination. Such a gradual production of a more slowly-increasing variety well explains the diminishing intensity of the disease produced by successive inoculations. As Dr. Hurst points out, the diapedesis of the leucocytes is soonest able to overcome such feebly-increasing organisms. There is, however, to my mind, this difficulty. Attenuating processes consist generally in growing under *unfavourable* conditions, and I cannot see why such conditions should favour the slowly-increasing rather than the rapidly-increasing varieties of the microbe. Indeed, it seems to me probable that, other things being equal, unfavourable conditions generally would rather select the more quickly-increasing. Quick breeding is such an obvious advantage under adverse circumstances, as affording, in each generation, more numerous chances of survival. There remains, of course, the suggestion that the selection may "act in some other way."

Supposing, then, we have a virus attenuated in the manner suggested by Dr. Hurst, how can immunity conferred by inoculation with the same be explained? We may take it as an established fact that the local inflammation attending inoculation is accompanied by diapedesis of the leucocytes. These leucocytes, in their abnormal state of activity consequent on inoculation,—and, perhaps, as Dr. Hurst suggests, in increased numbers,—and, after their victory over the feebler army of microbes, might be ready to attack and subdue the more formidable foe. This would explain momentary immunity, but I cannot see how such a state of

abnormal activity—which is inflammation—can be supposed to continue for months or years. Nor does it seem to me probable that an increased rate of production of the leucocytes within the body could be rendered permanent by the temporary action of the attenuated virus; for it is to be remembered that the immunity is supposed to extend over certain prolonged intervals—nine months in splenic fever, according to Pasteur, and seven years for the small-pox, according to the medical faculty; and if protective inoculation merely consists in the increase in the strength and activity of the standing army of leucocytes, then *one* inoculation should suffice for *all diseases*; he who is protected by inoculation with the cholera virus should be proof against small-pox, and the virus of scarlet fever should protect against typhus. Moreover, might not such a state of leucocytal activity be produced by other means, without the introduction of microbes or poison from a diseased animal—an idea which, to say the least, is lacking in poetry? That such were the case is, indeed, a "consummation devoutly to be wished" and prayed for. If Dr. Hurst can prove his proposition he will have the thanks of a long-suffering public, threatened with the nightmare anticipations of a life spent in being inoculated—a veritable flying from "the ills we have to others that we know not of."

Finally, I must remind Dr. Hurst that the inoculation liquids of many experimenters contain no microbes. Dr. Koch's consumption vaccine, I believe, contained none, nor does that of Dr. Kraftkine.

G. W. BULMAN.

"TWINS AND TRIPLETS" IN *Doris*.

MR. J. ARTHUR THOMSON refers to the frequency of "twins and triplets" in *Doris*, meaning thereby two or more embryos developing within a single egg, and suggests that it may be due to "the fact that the ribbons [of eggs] are battered to and fro by the surge."

I have observed the same phenomenon, however, in eggs laid by a *Doris* in an aquarium, and not exposed to any movement at all.

April 15, 1893. C. HERBERT HURST.

TO CORRESPONDENTS.

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

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ERRATA.

P. 254, line 21, for "Caprettæ" read "Caprellæ."
P. 255, line 13 from bottom, for "Hasting" read "Harting."
P. 259, the block was lent by Dr. Sclater.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

Fog and Vegetation.

AMONG the most interesting of last month's reports is that issued in the *Journal of the Royal Horticultural Society* (part 1, vol. xvi.) by Professor F. W. Oliver, who publishes the second instalment of his observations on the effects of urban fog on plants cultivated under glass. The effect is twofold; in the first place, we have to consider the serious loss of light; in the second, the poisonous substances present in the air.

The general tone of plants, especially of those fond of sunlight, must be considerably lowered by the often long persistent, dull winter weather, with its frequently-recurring fogs. The assimilation of carbonic acid is interfered with, while the transpiration of water vapour from the leaves is almost at a standstill. The roots are not affected, and continue to absorb water from the soil, especially in the case of stove plants, where they are kept by the warmth in a state of marked activity. Hence the cells of the plant become unduly distended with water. Finally, owing to faulty circulation in the intercellular passages, access of oxygen from without is impeded, while the supply normally received from the decomposition of carbonic acid in assimilation is almost entirely cut off; respiration is checked, and substances tend to accumulate in the cells from want of complete oxidation. Thus the whole leaf-mechanism is out of gear. In this enfeebled state the plant is exposed to the attacks of sulphurous acid, hydrocarbons, and other noisome fog constituents, and the result is lamentable, often, indeed, fatal.

Professor Oliver distinguishes two classes of injury, produced by distinct causes. First, cases in which the leaves show local discolourations, particularly at the tips and margins, while the unaffected parts remain fully functional, and the leaf does not fall; secondly,

cases in which the leaves fall, showing either a complete brown or yellow discolouration, or only a partial one limited to the apex, margins, or base, or restricted to minute specks, or irregular patches scattered over the surface. Sometimes the leaf falls uninjured; according to Mr. Watson, during some recent fogs bushels of healthy-looking leaves were gathered up almost every morning in the Palm House at Kew.

The local blotchings of the first class are presumably due to the action of an acid on the upper surface of the leaf. The layer of dirt deposited by the fog contains an appreciable amount of sulphuric acid, the product of oxidation of sulphurous acid. The frequent wetting of the leaves brings this into solution and the drops having a tendency to collect at the tips and margins will leave there on evaporation a deposit of acid. This process is continually repeated until sufficient acid accumulates to corrode the surface.

The changes in the leaf which cause its rapid fall, with or without a colour change, are, the author thinks, largely due to an attack on its delicate unprotected internal tissues. The fog effects an entry through the stomates into the system of intercellular spaces, where its poisonous ingredients come in immediate contact with the moist, delicate, and unprotected membranes of the living cells, which offer but little resistance, and the protoplasm is directly attacked. Whether the leaf succumbs or not depends on the inherent constitution of the protoplasm. The process was carefully followed in leaves of Rhododendrons and others, and the action was found to begin in the lower layers of the spongy tissue next the stomates. Thence it spread to the upper parts of the leaf and the epidermis. In the case of thin uncuticularised leaves when the epidermis is very soft, the noxious vapours may enter directly through the outer layer, as well as by the intercellular spaces. It is not clear to what component of the fog this second class of injuries is due. Owing to the very general absence in the injured leaves of acid products of the green colouring matter, sulphurous acid cannot be the sole agent, and Professor Oliver thinks that some complex organic bases like pyridine may play an important part. As regards remedial measures, there seems but little to be done, except, as Lindley long ago suggested, to keep the temperature as low as is compatible with the life of the plant, with sufficient humidity to avoid desiccation. If the houses are otherwise tolerably air-tight, a contrivance for filtering the incoming air through boxes containing trays of charcoal sticks may be used to mitigate the evil.

A Technical Education Conference.

On April 20 and 21 the Senate House at Cambridge was the scene of a Conference on the relation of Universities to the County Councils in regard to technical education.

The meeting was a representative one. Delegates from the Board of Agriculture, the Agricultural Society, the Oxford and London Universities Extensions, and numerous County Councils met a number of the lecturers of the various societies and others interested in the subject under the auspices of the Cambridge University Extension Board, with the Vice-Chancellor, Dr. Peile, as chairman.

At the first session the "needs of rural districts" and "local organisation" evoked some spirited discussion; a certain difference of opinion was manifest as to the former, while, as regards the latter, a point of vital importance, many of the lecturers had to report a serious apathy on the part of local authorities and persons of influence. Co-operation between neighbouring counties and between County and Town Councils was also the subject of a paper.

At the second session, Professor Liveing spoke on the sequence of subjects of instruction; the meeting then passed to the consideration of the Cambridge and Counties Agricultural Education Scheme. This will ensure a course of two years' instruction in subjects bearing upon Agriculture, including Chemistry—Elementary and Agricultural; Botany—Elementary and Agricultural; Physiology; Geology; Economic Entomology; Book-keeping, Mensuration and Surveying, and Agricultural Engineering. The course will occupy about half of each year, so that those intending to become farmers will have the other half in which to study the practice of farming. Certain Professors and Teachers in the University will admit to their lectures, and to practical instruction in their laboratories, students who, being over seventeen years of age, shall give satisfactory evidence of a sufficient previous education to enable them to profit by such instruction. Several County Councils have voted money for the support of the scheme, while a grant of £100 has been made by the Board of Agriculture. The same County Councils have also offered scholarships to assist promising young men desiring to take the course. Finally, it is hoped that the University will shortly sanction an examination in connection with the course and will grant a diploma to successful candidates.

We wish the scheme a hearty success, and hope that those whose future is to be devoted to farming will take advantage of means which will enable them to become intelligent farmers at a very small cost, and thus go a long way to solve the vexed questions of making agriculture more profitable.

Gigantic Australian Marsupials.

At the last meeting of the Zoological Society of London (May 16), Professor Alfred Newton communicated a letter and drawing from Professor Stirling, of Adelaide, referring to the discovery of a great accumulation of skeletons of *Diprotodon* and other extinct marsupials in South Australia. The find has already been briefly

recorded by the publication of a telegram in the *Times* of April 21, and, judging from the letter, it seems likely to complete our knowledge of the skeleton not only of *Diprotodon*, but also of other extinct Australian animals. The bones are said to occur in a salt marsh in a wonderful state of preservation; but the country is now so hot and arid that the difficulties of digging and transport are very great.

Diprotodon, it may be explained, was a wombat-shaped animal about as large as a rhinoceros—the largest marsupial hitherto discovered—and, with the exception of its feet, the skeleton is now almost completely known, thanks to the explorations of Dr. Bennett and others and the researches of Sir Richard Owen. The history of the gradual discovery of the animal is one of some interest. The name *Diprotodon* was first given by Sir Richard Owen in 1838 to the anterior end of a lower jaw obtained by Sir Thomas Mitchell in the Wellington Caves, New South Wales. Five years later, a drawing of part of a jaw with teeth reached England from the same source, and this Sir Richard Owen believed to represent a kind of *Dinotherium*, indicating for the first time the occurrence of primitive elephants in Australia. In the same year, a portion of a molar tooth, associated with the shaft of a femur and other fragmentary bones, was also received, and the same anatomist wrote: "The fossils, which my friend has now transmitted, incontestably establish the former existence of a huge proboscidian Pachyderm in the Australian continent, referable to either the genus *Mastodon* or *Dinotherium*." Only a year later, however, these early surmises proved to be incorrect, and within a short time Sir Richard Owen was able not merely to describe most features in the osteology of *Diprotodon australis*, but also to distinguish another allied genus, *Nototherium*. The feet alone remained unknown, and parts of these were described in the *Philosophical Transactions* of the Royal Society in 1886 as the toes of that fabulous monster, the "Great Horned Lizard of Australia" (*Megalania prisca*, Owen). Complete skeletons, such as Dr. Stirling leads us to expect, will no doubt help much in the determination of minor points and classificatory matters; but the researches of Owen and later authors leave little to be learned about the main features.

THE EARLIEST MONKEYS.

IN a recent paper on the Eocene Mammals of North America, Messrs. Osborn and Wortman announce their belief that the European *Adapis* and certain allied American forms instead of being, as generally supposed, Lemuroids, are really monkeys. The ground for this appears to be that they have normal lower canine teeth, instead of having the first premolar modified to serve this function. We cannot, however, see that this is a valid reason for their separation from the Lemurs, the earlier forms of which, in our view, were probably ancestors of the Monkeys. This, however, we suppose, is nowadays heresy.

The Ancestors of the Cat.

The above-named authors in the same communication put forth the hypothesis that the cats, instead of being directly related with the other Carnivores of the present day, are independently descended from the Creodont genus *Palæonictis*. In view of the close similarity between the Cats and the Civets (the latter being clearly related to the Dogs), this is "parallelism" with a vengeance. Does it never strike the promoters of such extreme views that by proving (!) the want of relationship between apparently closely allied animals they may be cutting away the ground from beneath the foot of the evolutionist, since, if resemblances are, so to speak, fortuitous, why should not we revert to the doctrine of separate creations? That parallelism does exist in Nature, we are fully prepared to admit, but it is not omnipresent. However, when once the ball is set rolling in a certain direction, everyone thinks it necessary to give it another push.

Aërial Roots of Orchids.

The velamen, or characteristic external covering of the aërial roots of epiphytic orchids, forms the subject of a note by P. Groom in the last issue of the *Annals of Botany*. As terrestrial orchids are almost invariably devoid of a velamen, it is usually assumed that the aërial orchids acquired theirs subsequently to the adoption of an epiphytic mode of life, but it is suggested that the few exceptions may indicate the existence of the covering in a previous terrestrial mode of life.

With a view to getting some light on the date of origin of the velamen, the author has made observations on *Grammatophyllum speciosum* and species of *Bromheadia*, both at Singapore. The former is one of the few orchids which grow naturally both as terrestrial and epiphytic plants, and can be found growing in the jungle when it has happened to fall off a tree. When a terrestrial plant, the velamen is not only retained but even strongly developed in the soil, while the upwardly-growing aërial roots characteristic of the epiphytic habit, which Schimper has shown to be respiratory organs, are still developed.

Bromheadia alticola lives in the full blaze of the sun on high tree tops, and has a peculiar two-layered velamen, while in *B. palustris*, which is terrestrial, the velamen is fundamentally similar, but lasts only for a short time, peeling off and disintegrating. This points to the conclusion that this covering is essentially adapted to epiphytic plants, and was evolved subsequently to their assumption of an aërial mode of life.

The two sets of observations seem to give contradictory results. In the first, the velamen being more highly developed in the subterranean; in the second, in the aërial roots; but the explanation is found in the fact that the function of the structure varies. In *Grammatophyllum* it is essentially an absorbent organ, which not only

persists, but is more highly developed, while the root becomes subterranean; whereas in *Bromheadia* it is mainly protective, preventing loss of water, while the absorptive function is carried on by root-hairs borne on the lower surface, which, in the terrestrial species, are reduced to strongly cuticularised papillæ.

Hence, in the aërial branches of the subterranean roots of *Grammatophyllum* the velamen dwindles, while in the subterranean roots of *Bromheadia* it peels off so as not to interfere with the process of absorption.

Science and Dictionaries.

The explanation of the sins of omission and commission in scientific words in dictionaries would task the acutest mind, and it is very dismal to notice that Dr. Murray's monumental English dictionary is not conspicuously better than its less ambitious predecessors. In an idle hour we dipped into the "A's" and brought up the following:—

Sins of omission—

Alecithal: Amphiblastula: Analogy (in its scientific opposition to "homology"): Asteroidea: Anamniota: Anthropomorpha: Acœlous or Acœla.

Sins of commission—

Ascidian—A group of animals belonging to the Tunicate Mollusca: considered by evolutionists to constitute a link in the development of the Vertebrata.

Anthozoa—Another name for the zoophites called Actinozoa.

The Imperial Institute.

Fellows of the chartered scientific societies have received so many pressing invitations to become Fellows of the Imperial Institute that they have long been anxiously awaiting a definite statement of the privileges and work in which they might share. Since the opening by the Queen, on May 10, their wish has at last been gratified by the appearance of an advertisement, which is strongly suggestive of that of the Crystal Palace and the Earl's Court show, and seems to explain that the Institute is to be another summer resort for Londoners. A music and dancing licence has been obtained, and on four days a week the public are to be admitted to the entertainments at popular prices. "Tea, coffee, and other refreshments will be served from the garden kiosks. . . . Seats and tables may be reserved in the public dining-rooms by making application to the dining-room superintendent. . . . The full band of the Royal Artillery will play daily in the kiosk of the West Garden. . . . Entertainments of vocal and instrumental music will occasionally be given in the Great Hall and the Indian Pavilion." Fellows and their friends have the sole enjoyment of these privileges on Wednesdays and Fridays.

Cholera.

The May number of the *New Review* has a sensible and readable article by Dr. Robson Roose on the propagation and prevention of cholera. Numerous cases are cited in illustration of the intimate relation between the water supply and cholera outbreaks, and hence the supreme importance of the purity of water used for drinking. The author does well in pointing out that dirty filters are worse than none at all; unless it is from time to time cleansed or renewed, fairly good water may actually take up impurities from the filter. If charcoal be the agent it should be boiled occasionally, say once a month, and then dried in the sun or an oven. Spongy iron filters are recommended for general use as being cheap and easily renewed. The fact that water is cool and sparkling does not imply purity. An outbreak in Golden Square in 1854 was traced to a well, the water from which was much liked for having these characteristics, but on examination was found to be contaminated by leakage and filtration from a cesspool. The general rules for prevention of the epidemic are those of ordinary hygiene, cleanliness in all things, moderation, and care in diet and exercise. The fact that more than five-and-twenty years have passed since cholera gained a footing in this country, though it has from time to time reached our ports, may fairly be attributed to the improved sanitary conditions which now obtain in all our large towns.

A study of the range of the Molluscan genus *Placostylus* has lately led Mr. C. Hedley to generalise in reference to the ancient geography of the region of New Caledonia, the Solomon Isles, New Hebrides, and Fiji Isles, and their connection with New Zealand (*Proc. Linnean Soc. N. S. Wales*, ser. 2, vol. vii., 1893, pp. 335–339). He thinks that these islands form part of a shattered continent, never connected with, or populated from, Australia, but rather deriving their fauna from Papua *viâ* New Britain. The presence of genera common to Australia and New Zealand is explicable on the ground that they migrated, not from the one territory to the other, but each from a common source, New Guinea. New Zealand and New Caledonia seem to have been early separated from the northern archipelagoes and to have ceased to receive overland immigrants therefrom. Finally, the Fijis appear to have remained to a later date in communication with the Solomons, though severed from that group before the latter had acquired from Papua much of its present fauna.

The problem of stocking a pond with fish receives an unexpectedly bold solution in our contemporary *Illustrated Scientific Facts* (April 15); it is there said that, as the bottom of many ponds consists of soil which was once the bottom of the sea, it probably abounds in spawn deposited, say, by the fishes of the Old Red

Sandstone; the vivifying influence of the water hatches out the spawn, and hence the appearance of fish. The paragraph is gravely worded, and there is not the least indication that it is "writ sarcastic."

In the *Trans. Entom. Soc. Lond.*, 1893, pp. 191–9, Dr. D. Sharp describes the very remarkable eggs of a Reduviid bug from the Amazon valley. The eggs were closely arranged on a leaf, and a wasp, supposed to have been killed by the mother bug to furnish food for her young, was entangled in the mass by the wings. Each egg is cylindrical, the upper part containing a conical, crown-shaped structure, composed of a system of network and tubes believed to afford entrance to spermatozoa. This cone is pushed upwards by the young bug in emerging, and the upper part of the egg-capsule, which has kept it in place, is ruptured. Numerous parasitic Hymenoptera were bred from the two outer rows of eggs, the cones of which had not been lifted. It seems that the habit of laying the eggs in a serried mass secures the safety of the majority within by the sacrifice of these outermost rows, beyond which the ovipositor of the ichneumon-fly cannot penetrate.

An interesting paper on the Pupæ of Moths, by Dr. T. A. Chapman, appears in the latest number of the *Trans. Entom. Soc. Lond.* (1893, pp. 97–119). He distinguishes two principal types:—the obtected pupa, which has a hard, even surface, with the skin of the appendages closely attached to that of the body, and with the fifth and sixth abdominal segments alone capable of movement; and the incomplete pupa in which the appendages are more or less free, maxillary palpi are present, and three, four, or five abdominal segments can be moved. The obtected pupa, also, has no power of progression, but the incomplete generally emerges from its cocoon before transformation into the moth. The division of the moths by means of these pupal characters roughly corresponds with the old division into Macro- and Micro-lepidoptera, but the Pyraloids, having obtected pupæ, are classed with the higher moths, while the Zygænidæ, Sesiidæ, Cossidæ, Hepialidæ, and Limacodidæ are placed with the Tineids, Tortricids, and Micropterygids. Most of these changes have been suggested by naturalists working mainly by the neuration of the wings; the confirmation afforded by this distinct line of research will hasten the general acceptance of the new views.

At the meeting of the Entomological Society of London on March 8, Dr. Sharp read a paper on Stridulating Ants. He said that examination revealed the existence in ants of the most perfect stridulating or sound-producing organs yet discovered in insects. We have only received the abstract of this paper, and must wait fuller details before commenting on it.

Several contributions have lately been made to knowledge of the distribution of the mollusca. Mr. A. Everett has made extensive collections of land-shells in Borneo and the Philippine Islands, and these have been described by Mr. E. A. Smith (*Journ. Linn. Soc. Zool.*, vol. xxiv., 1893, pp. 341–352, pl. xxv., and *Ann. Mag. Nat. Hist.* [6], vol. xi., 1893, pp. 347–353, pl. xviii.). Mr. A. Abercrombie has collected 320 species of mollusca on the coast of Bombay, and 25 are determined to be new by Mr. Cosmo Melvill (*Mem. and Proc. Manchester Lit. and Phil. Soc.*, vol. vii., 1893, pp. 17–66, pl. i.). A list of 205 species from the Seychelles is also given by P. Dautzenberg (*Bull. Soc. Zool. France*, vol. xviii., 1893, pp. 78–84).

The *American Naturalist* for April contains an article, by Professor E. D. Cope, on the Genealogy of Man. Professor Cope agrees with M. Topinard in believing that the Hominidæ descended directly from the lemurs, without the intervention of the Simiidæ. A special feature of this article is a plate showing the peculiar character of the grinding faces in the teeth of the Palæolithic men of Spy.

The vexed question of Man and the Glacial Period is discussed over twenty pages of the *American Geologist* for March. Messrs. Shaler, Wright, Leverett, Upham, Claypole, Winchell, Hitchcock, and Putnam contribute to the discussion, which has apparently been induced by the publication of Professor Wright's book, noticed in this Journal for February. There are also some papers on Glacial deposits; and those on Pleistocene Geology, read at the Ottawa meeting of the Geological Society of America, held in December, 1892, also appear in print.

One result of the recent excursion of the Geologists' Association to Norfolk (to which we called attention in our April issue) was the finding in the Norwich Crag, at Bramerton, of a portion of an antler of *Cervus sedgwicki* ?, Falconer. The specimen was obtained by Mr. R. W. Hinton, and identified by Mr. E. T. Newton. Although named with a query, it belongs to a form hitherto not recognised out of the Cromer Forest Bed.

The subject of chlorophyll in animals has just been discussed again in a lengthy article by Dr. E. L. Bouvier (*Bull. Soc. Philom. Paris* [8], vol. v., 1893, pp. 72–149). He admits that the green colouring matter is sometimes diffuse in Infusoria, but he considers that in the large majority of cases there is distinct proof that it occurs solely in symbiotic algæ of the family Palmellaceæ. He points out that these green specks are frequently found free in the water, and in that case they multiply by zoospores like the isolated algæ of certain lichens. He has also observed the inoculation of organisms by these free chlorophyll-bearing cells.

A RECENT number of the *Farmers' Bulletin* (no. 10), issued by the United States Department of Agriculture, describes the Russian Thistle, *Salsola kali*, the Prickly Glasswort of our sandy sea-shores, as one of the worst weeds ever introduced into American wheat fields. One year's damage in Dakota alone is estimated at 2,000,000 dollars. The bulletin reports that it takes complete possession of the soil, while its spiny nature makes it objectionable to horses and other animals.

IN *Le Naturaliste* of March 15 Henri Coupin argues that zygomorphy of the flower, that is, symmetry in relation to a single plane, is primarily for the purpose of protecting the pollen from rain, while adaptation for pollination by insects is only a secondary consideration.

THE "Dictionnaire Pratique d'Horticulture" is an improved translation of Nicholson's well-known Dictionary of Horticulture. It is edited by M. S. Mottet and is, says the *Gardeners' Chronicle* of May 6, "indispensable to all who read French, as it is more complete than the original edition, and contains several additional features." It is issued in parts by Octave Doin of Paris.

THE *Journal of Botany* has been showing a tendency towards Cryptogamic Botany during recent years. In the April number, there is a paper on Fresh-water Algæ, one on Marine Algæ, one on a Moss, one on Hepaticæ, and a long obituary notice of a Cryptogamic Botanist. The editor probably means no more by this than that Cryptogamists (even though one fewer) are getting too many for him.

MR. HERBERT SPENCER has issued a reprint of his articles on "The Inadequacy of Natural Selection," followed by that on "Professor Weismann's Theories," appearing in the *Contemporary Review* for February, March, and May, 1893. The pamphlet is published by Messrs. Williams and Norgate.

PROFESSOR LOGAN LOBLEY has in the press "The Parishes of Surrey: their Surface Features, Geological Structure, and Natural Resources." The book will deal with the 148 parishes in the county, which will be taken in alphabetical order. It is proposed to treat the subject in as practical a way as possible, and to render the volume a thoroughly useful compilation.

We have had the opportunity of seeing a specimen copy of the first part of Mr. Lydekker's "Royal Natural History," and are glad to be able to report on the splendid style in which this important work is got up, the illustrations looking far better when set up among English type than they do in Brehm's ponderous tomes among German letterpress. If this part be not in the hands of our readers by the time they peruse these lines, we believe it will be issued in the course of the current month.

Mr. J. W. Gregory has decided, with characteristic energy, to push on through the Mount Kenia country to Lake Barengo, and proposes to return by the unfrequented Sabaki route. The district is fairly healthy, there are many geological and zoological problems awaiting solution, and we trust that he will be successful in doing good work; he may thus save the reputation of the Villiers Expedition, so seriously injured by its originator. Sir Gerald Portal's cautiously worded reference to Lieut. Villiers was quoted in our April number.

The Government of India has decided to dispense with the services of natives as Geological Surveyors. The reason for this decision is stated to be, that habits of observation and practical enquiry are not sufficiently developed in the Hindoos by the present system of education. We also understand that the Government has issued further orders restricting the work of the Surveyors to questions of economic interest.

We have received the *Bulletin of the Geological Institution of the University of Upsala*, vol. i., no. 1, edited by Professor Hj. Sjögren. This is a new serial, and is intended only for papers worked out at the Geological Institution of the University, or based on material belonging to the collections of the Museum. At present, it is proposed to issue a yearly number, which will contain a report of the meetings of the geological section of the Upsala Students' Association of Science, in addition to the original articles. Subjects may be treated in French, German, or English. The present number opens with "Contributions to Swedish Mineralogy," by the editor, in which Axinite, Hedyphane, Humite, Chondrodite, Clinohumite, Longbanite, Szabite, and Adelite are treated, the last three being recently-discovered forms. C. Winran writes "Ueber das Silurgebiet des Bottnischen Meeres"; O. Nordenskjöld on "Der Hälleflinten der nordöstlischen Smålands"; and J. G. Andersson on "The Occurrence of the Paradoxides ölandicus-zone in Nerike." The *Bulletin* is illustrated with five plates of minerals.

I.

Flowers in the Guiana Forest.

THE "spicy breezes" of the tropics are proverbial, and many travellers have written on the beautiful flowers and their perfumes. According to the popular notion, these odours are perceptible at considerable distances from the coasts, and greet the weary mariner almost before he sights land. This, however, if not altogether untrue, is highly exaggerated. When there is a land breeze, it more generally brings with it a heavy stifling odour redolent of the mangrove swamp and decaying vegetation, no doubt recalling memories of the forest, but rarely pleasant, and, sometimes, even disagreeable. On the rivers, where a heavy mist hangs at night, perfumes are sometimes wafted to considerable distances, but rarely can they be detected at sea, probably because there is always some motion in the air which disperses the vapour.

The ordinary visitor, with exaggerated notions of the beauty of tropical flowers, expects to see them here, there, and everywhere, and is naturally disappointed when he finds so many trees and plants with, what he considers, no blossoms at all. Coming from meadows dotted with moon-daisies or golden with buttercups, he sees only a few inconspicuous weeds, with here and there a yellow rattle-bush (*Crotalaria*), or the blue *Ruellia tuberosa*. In the gardens, however, he can hardly fail to be struck by the size and beauty of the numerous species of *Hibiscus*, *Convolvulus*, and *Bignonia*, but these are not wild flowers.

The true native plants are neither to be found in field nor garden in the cultivated districts. To see them he must paddle along the rivers and creeks, walk on the sand-reefs, and explore the savannahs; but, even here, he will be disappointed if he expects to see anything like a continuous stretch of colour such as gratifies the eye on an English down or mountain slope. The flowers in the tropics do not grow in great masses, like heather, furze, or broom, but one species here and another there, rarely in anything like a clump, or even scattered in any considerable number. Then they do not all open at the same time, but some are earlier and some later, even in the same species. Again, few remain open for any lengthened period; some can only be seen by the early riser, as they close soon after sunrise,

while others unfold with the dawn and droop and wither in an hour or two.

Wallace, in his "Tropical Nature," heads a section, "Comparative Scarcity of Flowers," and states that "conspicuous masses of showy flowers are so rare, that weeks and months may be passed without observing a single flowering plant worthy of special admiration." This is such a sweeping assertion, and so contrary to the facts, that, notwithstanding the author's status as an authority on the tropics, we can hardly do other than try to refute it.

In the dark arches of the forest scarcely a flower is to be seen; but even here we often come upon a litter of golden or crimson petals which have fallen from above, so that, although we cannot see that the tree is flowering overhead, we know it must be a magnificent spectacle if we could only rise above it. Along the banks of the rivers and creeks there are always flowers, sometimes few, sometimes many, but at no time entirely wanting. Most of these are borne on creeping and scrambling vines, some are particularly showy, and even an ordinary observer must admit that many of them are worthy of special admiration. Again, there is the sand-reef where the low bushes, at certain seasons, are decked with flowers, which can be easily seen, and are by no means rare.

One of the reasons why so few flowers are noticed is because they are not looked for at the right time. It cannot be expected that forest trees will be in flower all the year round, although they may blossom twice instead of once in twelve months. In Guiana the flowering seasons are in February and March, and July and August. During these periods masses of showy flowers are common, although many of them are so far above our heads, or so hidden by the forest canopy, that we can only get an imperfect glimpse of them.

The expanse of green foliage is so great as almost to overpower all but the most gaudy flowers. Whites and all dull colours are obliterated, and even yellows combine with the sunlit green in such a manner as to be invisible at a distance, unless, as in the case of the etabally (*Vochysia*), the whole tree is covered with flowers. The large waxy-white flowers of the clusias and yellow and cream Malvaceæ, are quite invisible, while the great spikes of Mimoseæ can hardly be seen from the ground unless specially looked for. Rarely does a tree become bare at the time of flowering, and even when this takes place the individual is almost lost in the great crowd. The foliage is so dense that we can only compare the expanse to a roof. When Wallace states that from some elevated point you often gaze down upon an unbroken expanse without a single patch of bright colour, he speaks the truth, but it does not follow that there are no showy flowers there. Take away the background of foliage, if this were possible, and many a brilliant group of flowers would appear. Even in an English orchard the difference between those trees which flower before and those with the leaves is enormous.

A great many forest-trees have greenish-white or green flowers. These are, of course, very inconspicuous at all times, but they by no means eclipse those with showy flowers. Rarely do even two or three of a species come together, so that there will be always some of the more conspicuous even in a small area. When we mention that the predominant natural order is the Leguminosæ, it will be seen at once that there can be no scarcity of flowers. This family is a host in itself, the various forms of Papilionaceæ, Cæsalpineæ, and Mimoseæ all combining to make an interesting and showy collection. Then there are the monkey pots (Lecythideæ), all of which have large and highly-coloured blossoms; but the most conspicuous tree is, undoubtedly, the etabally (*Vochysia*), which, during the season, is a canopy of gold, the result of flocks of sulphur-coloured butterflies.

It is unnecessary to give a list of the different families which make up the forest flora. They are so numerous and varied, so different from anything in the European woods, as to be most striking to the ordinary observer, and a continual source of interest to the botanist. It is, however, their beauty of foliage which compels attention at first; the shapes and sizes of their leaves, their luxuriance, and their continual struggle to occupy every little patch of sunlight to the exclusion of all others, which prevents the forest from ever becoming dull or monotonous.

Book illustrations rarely give any adequate representation of forest scenery. If they are not conventional they are generally almost caricatures. Artists have attempted to picture some of the most striking peculiarities, but even they only observe the superficial. Appun's illustrations in "Unter den Tropen" (Jena, 1871) are among the best, and as nearly true to life as a draughtsman can make them; but in the whole series there is hardly a flower, this proving that the artist was impressed by the foliage almost to the exclusion of everything else.

In paddling up a creek the canopy of forest-trees is so far overhead as to be out of the line of sight. Every bend brings into view a new scene. Clumps of graceful palms, masses of gigantic creepers, jungles of tree-ferns intermingled with the contrasted foliage of marantas and heliconias, and great aroids which climb almost every tree or perch on their branches, all combine to leave an impression of rampant vegetation which eclipses the most gaudy assemblage of flowers.

But, with all this, flowers are not wanting. Great bunches of yellow bignonias, dipladenias, allamandas, scarlet noranteas, combretums and cacoucias, with spikes which glow like fire in the sunlight, a hundred species, of various shades, from rosy crimson to purple; and last, but by no means least, the tubular Cinchonaceæ and other white flowering plants, the clusters of which often show up quite prominently against the dark background of foliage. Some of those with lurid flowers are by no means inconspicuous. The

species of *Mucuna*, which hang their flowers on long strings, one of our commonest timber trees; the wallaba, with similar blossoms; the *Marcgravia*, with its curious pitchers; and several bushes and trees with their flowers sessile on stems and branches, all go to add to the interest of forest scenery.

As if this were not enough, many of the shrubs and lower-growing plants have coloured bracts. Almost but not quite hidden by their handsome foliage, the heliconias glow with a fire-like brilliancy, and some of the marantas, although not so conspicuous, still show up well among their great leaves. A species of *Cephaëlis* has large scarlet bracts, and other plants of the same family come to the front in a similar way. Tillandsias, which are epiphytal, and have inconspicuous flowers, often shine up on the edge of the forest owing to their scarlet bracts.

Colour is by no means wanting in the leaves. The great family of Melastomaceæ, so characteristic of the American tropics, stands out most prominently as an example of beauty in form, venation, and colour. Many of the species have a crimson glow over their whole surface which marks them out from their dark-green neighbours. Their flowers, although not generally showy, are always elegant, and their dark purple fruit very pretty indeed. The young seedlings of forest trees have always two or three of their upper leaves glowing with those delicate shades which are so conspicuous at the change of seasons, and low-growing marantas are often striped with rosy or white lines. Large patches of ferns and selaginellas also cover the ground on the edge of the forest, and with their lighter tints contrast with the dark greens and browns.

Only in the dense forest, where perpetual twilight reigns, are the tree-trunks quite bare. Along the river banks, borders of the savannahs, and sand-reefs, every stem is more or less clothed with creepers. Some, as *Vanilla* and *Marcgravia*, climb like ivy, their leaves lying almost flat, others festoon the tree with garlands, while the immense heart- and arrow-shaped leaves of the giant aroids congregate at the top, almost covering their host. Other great plants of the last-mentioned family sit on the upper branches and throw down long aërial roots, the size of thick twine, which branch into masses of fibres as they reach the water. Mosses are not so plentiful as in temperate climates: they are replaced by patches of ferns, some of which are particularly delicate. Species of *Trichomanes* often extend up the trunks for several feet, where they are replaced by *Polypodium piloselloides*, and higher yet by *Peperomia nummularifolia*. These form cushions on which small epiphytal orchids, ferns, tillandsias, and gesnerias find congenial habitats, all combining to decorate the otherwise bare stems and branches.

To return to the flowers. Away from the rivers and open spaces, they are almost invisible, and even when seen many of them are inconspicuous; but, although hidden from sight, they make their

presence known by their perfumes. They may be of that greenish white which is hardly distinguishable from the background of foliage, and yet be able to attract myriads of nocturnal insects. Pass by at any time in the day and not the faintest trace of perfume lingers upon them, but paddle along at night and you wonder whence comes the overpowering fragrance.

Diurnal flowers and insects are comparatively scarce when contrasted with those of the night. The great tubular blossoms which glow in the sunlight are visited by humming-birds, butterflies, and bees, but the hum and whirr of insect life, so characteristic of night in the forest, is almost wanting. Where a few butterflies may be seen fluttering lazily from flower to flower at noon, after sunset moths are attracted in numbers by the great white blossoms, or the fragrance of thousands of smaller and less conspicuous flowers.

How difficult it is to trace these perfumes. Sometimes it is quite impossible. Who recognises the odour of the mora[1] flowers, or knows anything of the numerous scents that are wafted across the narrow rivers after nightfall? As certainly as we see a particular kind of whitish flower, we can confidently state that it is odoriferous at night, but hardly a single species is known in this way. In passing along the creek, a perfume is suddenly wafted across, but where it comes from is a problem not easily solved. Even that common orchid, *Epidendrum nocturnum*, which derives its specific name from the fact that it distils its perfume after nightfall, is rarely known by this character. We have seen cases where these orchids have been kept for years without their owners knowing anything of their perfume, and when it was discovered, this came about by the accident of bringing a plant into the house for an evening. Other orchids are quite as peculiar in this respect; a most delicious odour may be perceived at one particular time, and not be appreciable at any other.

Some flowers open for an hour or two, and then, whether fertilised or not, droop and wither; others are able to try again perhaps at the same time next morning or evening. Those which do not fall may still have their particular time for fertilisation, when it appears as if every effort is strained to attract the particular insect whose agency is so urgently required. It is surprising to find that so many are successful in these efforts. Even when their odour is not appreciable to us, the bees find them out. They may be hidden away under a tangle of bush-ropes, and not a single insect of the species required be visible in the neighbourhood, but somehow or other, as the flowers open, the bees or moths appear in considerable numbers.

Flowers must be peculiarly sensitive at these times. Some, as

[1] A gigantic timber tree, belonging to the family Leguminosæ, and forming extensive forests in British Guiana. It grows from 130 to 150 feet high, and produces a very tough and close-grained wood, which is imported into this country in considerable quantities for ship-building, even rivalling oak in its non-liability to splinter. The bark is astringent, and useful for tanning.—ED.

we know, have their temperature raised several degrees, and, probably, all are more or less heated for the short period when they are prepared for fertilisation. Several species of *Hibiscus* go through a series of changes of colour from morning to evening; the *Victoria Regia* opens the first night as a waxy white flower, and the second with rosy petals. Several orchids also change colour very quickly, especially after their work is accomplished. In a few hours the plump waxy petals become limp, the markings spread and get diffused over the lip, and the whole flower becomes unsightly. Where there are male and female flowers the latter often lasts much longer, and may even be perceptible for months at the apex of the seed-vessel.

What an important crisis in the life of the plant is its flowering time. All its energies appear to be directed to this one end and aim. We can hardly look upon it as merely vegetating, but as an individual straining to accomplish the greatest work of its lifetime. There are so many little things to be considered that, whatever our ideas of Natural Selection and the chance survival of the fittest may be, we are bound to think of them as sentient beings. When a man fixes an hour for performing certain work, and that is the most suitable and perhaps the only possible time for accomplishing it, he does little more than the orchid, which, apparently, knowing that its friend the insect will be on the wing from midnight to dawn, opens its flowers and circulates its perfumed invitations through the neighbourhood only during those few hours.

We say, perhaps, that these actions are instinctive, but what is instinct? Is it not the accumulated experience of all past ages? Every individual, whether animal or plant, may be considered as a link in a long chain of beings, the origin of which goes back to some past age when the common ancestor was nothing more than a simple cell. During this long period, in which so many generations have passed away, what a wealth of experience must have been gained. Is this lost at the death of the individual? On the contrary, do we not see that every organism has the stamp of its parentage, and we can hardly be wrong in saying that not the slightest impression has been made on any individual that is not perpetuated in its offspring. These impressions may remain latent for generations, but nevertheless be ready to come to the front at any time according to circumstances.

Suppose we compare these to a composite photograph and call them physical memories, is it not necessary that there should be some power of selection? Is everything done mechanically as reflex action? Can we not conceive that there may also be physical reason, which works unconsciously, but at the same time always towards certain well-defined ends? When we go to sleep at night with the intention of waking at a certain hour in the morning, and do so perhaps to the minute, physical memory and physical reason

has perhaps kept the record for us and done it well. That something of the same kind is at work in every plant and animal is so obvious that it can hardly be disputed.

It is utterly impossible to conceive of such curious and wonderful contrivances as are found in the orchids, especially *Coryanthes* and *Catasetum*, originating without a purpose, and being perpetuated only by the chances of Natural Selection. Given physical memory and physical reason, we see at once that the difficulty is reduced to almost nothing. For ages environment has been at work moulding every part of the plant as it were, until it has become what we see it to-day; but it cannot remain on the same level; it must either advance or go backwards. Here individuality comes in. Why does every plant and animal differ from every other? With the same inherited characters, and grown under circumstances apparently identical, two seeds from one capsule produce trees differing more or less from each other. Natural Selection may account for a great deal, but it can hardly explain the origin of variation. Taking into account, however, the fact that flowers are extremely sensitive at the period of fertilisation, is it not possible that impressions may be made on the plastic germ-cells which affect the new individual? The seeds in one capsule may not be all impregnated at exactly the same time, and circumstances may occur in an hour, or even a few minutes, which affect the one individual, then at the most critical stage of its existence, to the exclusion of others in different stages.

Many insects and plants die soon after impregnation, the males almost immediately, and the females as soon as the germs of their offspring are sufficiently mature. It follows, therefore, that the end and aim of their whole life is to bring forth new individuals. Plants can be increased by budding and other than sexual connection, but it is doubtful whether the portions so divided are equally strong with those raised from seed. They are not individuals to the same degree as if raised from two parents with different experiences. Then, again, they have not passed through that critical period when the germ-cell was highly susceptible to every external influence.

Looking upon the trees of the forest as individuals, how interesting they appear. Sometimes we may fancy them conscious, and, instead of feeling lonely in their midst, rather think that we are in good company. No longer vegetating, they live, strive to do their duty, look out for "number one," and aim at perfection. The great scrambling creepers seem to know how to get ahead at the expense of others, growing longer or shorter according to circumstances, sometimes turning to raise themselves to great heights, or at others forming dense bushes. One species has succeeded by some peculiar contrivance, and its neighbour by means entirely different. They twine round or adhere to the tree-trunks as long as it suits their convenience, and then stretch forth on every side, or perhaps hang in long streamers almost to the ground. The mangrove extends its long

slender arms downward, and branches into fingers as it nears the water, feeling its way, as it were, to the most congenial places. Near it is the courida, which anchors itself in the mud in a different manner and has given up using props. But there at intervals up the trunk are large excrescences covered with aborted aerial roots which seem to indicate similar possibilities in this tree also.

Other examples are continually forcing themselves on the attention of the naturalist, all going to prove that plants are something more than creatures of circumstance. After making every possible allowance for the influence of light, heat, and moisture, there still remains the apparent selective power, which only seems to differ in degree from that possessed by animals. Without the power of locomotion, they yet extend their roots to great distances, feeling around, as it were, for congenial food, and if they fail in one direction try several others. Then their branches are condensed at the apex, or spread out from the base upwards, according to the situation, the same species, in different places, appearing quite distinct in shape and size.

But where do we find such examples of apparent selection of means to particular ends as in the orchid family? When certain species provide quarters for a garrison of carnivorous ants, it is easy to say that the insects found the bunch of aerial roots suitable for a habitation, and that the plant did nothing. But when we see that only a few orchids have developed contrivances to this end, and the great advantage they derive from them, we cannot but look upon them as designed for the purpose. In some cases they are so well placed as not only to defy every larva and cockroach, but even man himself. In our own experience, we have had to pass on and leave a fine plant, because we could not risk the bites of such a host as swarmed out at the least touch.

Some flowers, as we know, secrete nectar for the express purpose of feeding the insects they attract by their colours and perfumes. Others attract, but provide nothing, yet the blundering insect still fertilises it in his search for what is not there. When boys play similar tricks, we call them practical jokers. Has the plant done this either accidentally or mechanically? Some go so far as to provide traps by which insects are caught and utilised; are these contrivances also due to chance? In the *Coryanthes* we have a marvellous collection of means to a particular end, *i.e.*, the fertilisation of its flower. By means of its perfume and colour it attracts bees, only to give them a bath from which they can escape by doing its work. If a man does something of a similar kind we consider him rather clever, although, perhaps, not very honest. It is now generally conceded that animals are reasonable beings; to go a step farther, and allow the same to plants, is almost as necessary. It does not follow that because a dumb man cannot explain his thoughts that, therefore, he has none. When the strong man bears pain without flinching, is it

because he feels less than the baby who cries out at the top of his voice? It is a matter of common observation that plants show their likes and dislikes, suffer and languish when sick, brighten up and look robust in congenial positions, and have, to a certain extent, most of the faculties common to man and the lower animals. True, they cannot see nor hear, but their sense of feeling is very acute, and we may conclude that even taste is not wanting.

JAMES RODWAY.

II.

Biological Theories.

V.—SUGGESTIONS AS TO THE TRUE FUNCTIONS OF "TENTACULOCYSTS," "OTOCYSTS," AND "AUDITORY SACS."

SUCH a jelly-fish as Aurelia has eight minute finger-like bodies lodged in little pouches in the edge of the umbrella or swimming-bell. The tip of each is weighted by a calcareous deposit within it, and the stalk is hollow, the cavity of the gastro-vascular system extending into it. To the whole organ, that is, to the diminutive tentacle and its pouch together, the name "tentaculocyst" is applied, and the function performed by the eight such organs is a subject which I propose to consider first.

The large nerve-supply has led to the belief that the tentaculocyst is a sense-organ, and it is not infrequently described as a combined visual and auditory organ. That it is not auditory, I have already shown. That it is sensitive to light is a reasonable conclusion from the presence of pigment, but that does not explain its structure. A simple pigment spot without pouch or tentacle would serve that purpose quite as well. There is, moreover, a wide difference between an organ sensitive to light and a visual organ, for "sensitive to light" does not necessarily mean giving rise to a sensation when light falls upon it. It may be that light produces a stimulus when it falls on a pigmented patch, but unless that stimulus gives rise to a sensation of some kind or other, the organ should not be called visual. A reflex muscular contraction may follow upon the stimulation by means of light, but that by no means shows that sensation has intervened. Little as we know of the psychology of these lowly-organised animals, we are justified in doubting, if not in denying, the possibility of consciousness and, therefore, of sensation. It is nearly impossible to believe that consciousness, and judgment, and intelligent action upon the basis of knowledge received by means of sense-organs is possible in an animal with a nervous system so lowly-organised as that of a jelly-fish, especially of a Scyphomedusa, and an explanation of the function of these organs which is free from any such assumptions as these would, therefore, seem to be called for.

If, however, these were auditory organs, as has been supposed by

many, the fact that there were eight of them would be difficult to explain, though, of course, it might be suggested that this was necessary to enable the animal to determine the direction in which the sound reached it; but, even then, the limited powers of locomotion would seem to render such a sense useless, and hence the evolution of the organs could not be explained by Natural Selection, nor yet, since the animal has no vocal organs, by sexual selection.

Temperature may, perhaps, affect them, but if so surely it would affect all alike, and eight would not, therefore, be more useful than one or two; nor is their structure explicable on the hypothesis of sensitiveness to temperature.

It may be well, therefore, to enquire what are the conditions under which the animal lives, and what are the dangers to which it is exposed.

The danger from living enemies may be left out of account, for the abundant supply of thread-cells and the very small percentage of matter useful for food which they contain make them almost useless as food for other animals, and, even if it were not so, they have no means of escaping from any animal which might pursue them.

These animals are pelagic, that is, they live in the open ocean, near its surface. The tissues of the body are so delicate that it is difficult to lift one from the water without tearing it to pieces. The dangers of the storm-tossed surface of the ocean for such an animal are, in part at least, obvious, but it is less obvious how they are avoided, especially if the animals be devoid of judgment and even of consciousness. The view I propose to offer is, that the tentaculocysts, without giving rise to sensation, serve to automatically steer the animal in such way as to keep it out of the way of its chief dangers and, at the same time, in the region where its food most abounds. How this is effected can only be made clear when we know something about the nature of the disturbance which we call a wave.

It is not easy to know very much about a wave without also knowing more about certain branches of higher mathematics than is usual among zoologists. Parenthetically, I may, as a biologist, admit that the Senior Wrangler who defined a biologist as "a man who cannot solve a mathematical problem," came sufficiently near the truth to make the epigram a distinctly unpleasant one!

Those biologists who are undismayed by formidable formulæ, and who have not as yet studied wave-movements of water from the mathematical point of view, will find the greater part of the problem treated mathematically by Lamb (1) and Basset (2); but I will assume that the Senior Wrangler was right, and that the biologist is a man of observation rather than of mathematical investigation, and I will, therefore, appeal rather to his power of observing than to his mathematical faculty.

It is unfortunate that at times in our dredging excursions, when the waves are largest and best fitted for study, those of us who lean

over the side of the boat are usually not in the best possible condition for making scientific observations. The end of a pier has, however, some advantages over a boat under way; but in a boat at anchor is the place where the observations can best be made. The best time for the purpose is when the waves are large, and especially when their crests are sharp-edged and not rounded, and when there is no wind and no breakers or "white horses." Most of the points, however, may be made out from the pier-head, though in that case the path of the particle or of the mass is not a circle, as in deep water, but an ellipse with the long axis horizontal.

A *small* floating body almost or completely submerged is not carried along with the waves, but moves when on the crest of a wave more slowly than the wave, but in the same direction. So soon as it has fallen behind the crest into the trough it moves in the opposite direction as if, having slipped off one crest, it were hurrying to climb up to the summit of the next one. In the open sea the extent of this to-and-fro movement is equal to that of the up-and-down movement, so that the body—provided it be *very* small—constantly travels in a circular orbit, the diameter of which is equal to the height of the crests above the troughs. If there be no wind it will hardly progress at all, but will return always to almost the same place. The very slight progress of particles near the surface may be neglected, and the path of each particle may be regarded as a circle.

Let us now suppose the water divided into a series of layers, which, if the water were at rest, would each have a uniform thickness of, say, one inch. The passage of waves would not cause the water of one layer to mix with that of others; but since the water of the superficial layer in a trough is moving in the opposite direction to that in the crest behind it, it follows that the length of the portion between these two points, measured in the direction in which the waves are travelling, is getting less, and this portion of the layer must therefore be getting thicker, while the portion on the back of the wave is getting thinner. The total amount of the elevation of the crest is the net result, the sum, of the thickenings of all the successive layers of water, from the surface down to the bottom. The depression in the trough is similarly the net result of the thinning of all the layers below this region.

Another mode of observation is even more instructive. A *small* loose tangle of zoophytes, or of fine sea-weed, or a very loose ball of tow, thrown into the water, is seen to rise and fall and to move to and fro; but though each part of it describes a circle, the mass does not rotate, nor yet do its parts move all together. One part of the mass is at the top of its circular orbit before another, and hence the mass changes in form. The changes of form, or deformations, may be most easily described if we suppose the mass to be such that in still water it would be spherical, that is, a sphere of water rendered visible by the suspension within it of loose tow or seaweed, or, better still,

mud-particles. When the water is disturbed by the passage of waves, the sphere becomes an ellipsoid, but it *does not mix* with the surrounding water. The longest axis of the ellipsoid is vertical when the mass is under (or within) the crest of a wave, and its shortest axis is horizontal and parallel to the direction in which the waves are travelling —this direction we will call longitudinal. The transverse axis of the ellipsoid, that is, the axis parallel to the crest of the wave, remains unchanged throughout, and therefore equal to the diameter of the spherical mass we started with. As this remains unaltered, we may leave it out from further consideration.

As the mass we are considering falls into the trough behind, that is, as the wave recedes from it, it comes to be elongated in a series of new directions. At first sight, the mass appears to rotate in the direction in which each of its parts revolves, so that the upper end of the long axis comes to be inclined forwards till when the bottom of the trough is reached this long axis is horizontal and longitudinal. The mass, however, does not really rotate : the point which was uppermost when the mass was at the crest of the wave is uppermost still when in the trough. The *form* of the ellipsoid rotates, while its substance does not. The long axis is vertical every time the mass is at the crest of a wave. In passing from one wave-crest to the next the *form* (not the substance) rotates through *half a revolution*. During the same period each particle performs a *complete circular* journey. The relative movement of any two particles in the mass is one of oscillation. Take, for instance, the uppermost and the lowermost particles of the spherical mass with which we started. When the water is still, one is vertically above the other. During the movement the upper one (A) moves in a circle, and the lower one (B) moves in a smaller circle. A is therefore above B when both are under the crest, and again when both are under the bottom of the trough. At every moment A and B are moving in the same direction, but A always faster than B. At the crest both are moving forwards, A therefore comes to be slightly in front of B, but as both turn to move backwards, A comes to be above B again in the trough and then comes to be behind it. If we take two other particles a similar result is reached, but with a difference. If the two particles are not in the same vertical transverse plane when at rest, then they will not at any moment be moving in the same direction. Suppose C and D be particles at the two ends of the "longitudinal" diameter of the sphere in still water, C being in front, D behind. Then as the waves advance D will always move somewhat before C, so that when the middle of the mass is under the crest D will already have begun to move downwards, while C is still moving upwards. At this stage C and D will also be nearer together than at any other. In the trough these will be further apart than at any other stage, and while both are moving backwards D will already be moving backwards and upwards, while C is still moving backwards and downwards.

The diameter of the circular orbit of a particle at the surface is equal to the height of the crests above the troughs, as has already been seen. The point which is of most importance to the jelly-fish, however, is the amount of movement and of deformation at various depths below the surface. This has been investigated by mathematicians, who express the result as follows:—Calling the diameter of the circular path of any particle the "extent" of its movement, and writing a for the extent of movement of a superficial particle, that is, for the height of the crests above the troughs, and λ for the distance between one crest and the next, then a particle which in the undisturbed water is at a distance h below the surface will in the disturbed water move to the extent expressed in the formula $ae^{-\frac{2\pi h}{\lambda}}$

At a quarter of a wave-length from the surface $4h = \lambda$, and the extent becomes $ae^{-\frac{\pi}{2}} = \frac{a}{5}$ or one-fifth of extent of movement at the surface. At half a wave-length below the surface $2h = \lambda$, and the extent, therefore, $ae^{-\pi}$ = one twenty-third of a. At one wave-length the extent is $ae^{-2\pi} = \frac{1}{535}$ of a. (The calculations are only rough approximations.)

From this it is obvious that, however violent the movement may be at the surface, there is always a zone to be found not far from the surface where the jelly-fish may swim and feed in safety, but above which it would be exposed to danger from the violence of the movements of the water. This zone we will call the "zone of safety," but it appears probable that the name "zone of maximum food-supply" would be equally appropriate. Its depth below the surface varies with the violence of the movements at the surface. In still water it is at the surface.

I have now to show reason for the belief that the tentaculocysts, without giving rise to any sensation, can automatically steer the jelly-fish in such way as to keep it close to this zone and to bring it back to this zone whenever it is forcibly carried away from it (as by currents), and that this will happen, whatever the distance of this zone below the surface may be.

Before considering how the tentaculocysts can serve this important function, I would once more remind my readers that the only waves we have been considering are waves—not breakers or "white horses"—in the deep ocean, and not in shallow water.

Suppose the jelly-fish to be some distance below this zone of safety and in water in which there is no appreciable disturbance, it will then, by virtue of the greater specific gravity of its oral portion, float with the mouth downward. Its rhythmic contraction will, under these circumstances, by increasing the mutual pressure between the water in the sub-umbrellar space and the inner surface of the umbrella, cause the animal to move upwards till it reaches a zone

where the movement of the water is great enough to tilt the animal into an oblique position.

In what zone this will happen depends upon conditions too complex for consideration here, but the direction in which the tilting will occur is a matter worth considering, and one which presents no great difficulty.

Each layer of water becomes thinner when under a trough of the surface-wave, and thicker when under a crest. The ratio of the velocities of two layers is constant so long as the surface disturbance remains unchanged. Hence it is obvious that as the jelly-fish extends through more layers when under a trough (where the layers are thinnest) than when under a crest, the difference in the rates of movement of the uppermost and lowermost of the layers occupied by the jelly-fish is greatest when under the trough, that is, when the movement is opposite in direction to that of the waves. Hence the alternate tilts in opposite directions will not be equal, and the total effect will be that the animal is turned with its aboral (or "upper") surface "up-storm," that is, towards the direction from which the waves are coming.

The rhythmic contractions continuing, the animal will swim upwards and in a direction opposite to that of the waves. As it does so it comes into new zones, each moving more rapidly than the previous ones, till at length a zone is reached where the changes in velocity are so great that the slight elasticity of the stalk of the tentaculocyst is no longer sufficient to keep the weighted tip from lagging so far behind the pouch as to touch its walls. At the moment when it touches, a stimulus is produced, which causes a more energetic contraction of the bell, especially in the adjacent portion of the bell. The tentaculocysts first so affected will, of course, be those exposed to most rapid changes of velocity, that is, those in the edge which is turned upwards. The vigorous contraction of this portion of the bell in advance of the contraction of the rest will turn the animal, with its aboral face, first, directly towards the point of the compass from which the waves proceed, and then gradually downwards, the swimming movements taking the animal to a greater depth, and in the direction opposite to that in which the waves travel.

When a deeper zone is reached, where the movement is too slight to bring the lithite, or weighted end of the tentacular portion of the tentaculocyst, into contact with the sides of the pouch, the greater specific gravity of the oral portion of the animal again turns the aboral surface somewhat upwards, and the animal again approaches the surface.

The animal is thus automatically steered so as to be kept always in the uppermost zone of safety—a zone of maximum food-supply; and this explanation involves no assumption as to consciousness, or sensation, or judgment on the part of the jelly-fish.

A pelagic mollusc, such as *Pterotrachea*, has a pair of "otocysts"

in close relation with a large nerve ganglion, or rather pair of nerve ganglia, with which the eyes and tentacles are also connected. The organisation of the mollusc is, moreover, incomparably higher than that of the jelly-fish, and it is not very rash to suppose that in this case the shaking of the animal by the movement of the water, and the consequent collision of the walls of the "otocyst" with the lithite ("otolith") gives rise to a sensation. Whether it does so or not is, however, of little moment. The ganglia are connected (indirectly) with muscles, and the stimulus produced by the collision of lithite and otocyst-wall may safely be assumed to lead to muscular movements, either reflex or voluntary—movement which will take the animal to safer zones of water.

A shaking of the "auditory sac" of a crustacean would, by jolting the "auditory" hairs against the lithites—mere sand particles—produce a stimulus. The shaking might be produced in either of two ways: it might arise from a tremor of the ground upon which, or in which, the animal is resting, or from a disturbance of the water about the flagella of the antennule. Of the advantage of being thus warned of the approach of an animal which either may serve as food for the crustacean, or might feed upon it if it were taken unawares, there need be no doubt.

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C. HERBERT HURST.

A CORRECTION :—

In the previous article in this series (p. 352, 19 lines from the bottom), for "500 miles" *read* "500,000 miles."

III.

Naegeli's Experiments on Living Cells.

A SHORT notice of Carl von Nägeli's paper, "Über oligodynamische Erscheinungen in lebenden zellen," concerning the fatal effect of "nominally" pure water on living cells, appeared in NATURAL SCIENCE (May, p. 333). The following is an account of the experiments by which Nägeli arrived at his conclusions. The record of this work, which was carried on in his laboratory at Munich, was found by Schwendener among his papers after his death. Professor Cramer, of Zürich, went carefully over the ground a second time and obtained the same results as his old master.

The research, we are told by Schwendener, dates from the year 1880. Dr. O. Löw and Dr. Bokorny had recently published a paper on the reduction of silver salts by living protoplasm. While verifying for himself the results arrived at by these gentlemen, Nägeli was led to turn his attention from the effect of living cells upon silver salts to the reverse action of the silver salts on living cells.

The *Spirogyra* experimented on is a very common fresh-water Alga; it is composed of long cylindrical filaments, divided up at regular short intervals into cells, with spiral parietal bands of chlorophyll. The spirals may be single or double, and they, as well as the cells, vary in size in the different species. These differences affected, to a considerable extent, the sensitiveness of the plant under examination; the more compact the spirals the greater the resisting power of the cells. It was also found that resistance to any hurtful influence was at its maximum towards the close of the day, when there was a great accumulation of assimilation products in the chlorophyll bands. In the early morning, when these were withdrawn and dispersed through the cells, the plant was much more sensitive.

The alkaline solution of the silver salt used by Nägeli in his experiment was the one prepared by Dr. Löw (containing 1 $AgNO_3$, 1 NH_3, 3·6 K_2O to 100,000 volumes of water), which killed the plant by poisoning it; the cells lost their turgidity, the protoplasmic layer shrunk away from the walls, the chlorophyll bands changed colour and gradually became disintegrated. With further dilution the cells still died, but in a totally different manner; the turgidity of the cells and the protoplasmic layer remained unaffected, the chlorophyll bands alone seemed to be sensitive, they shrunk together with the

nucleus into a round ball. To this latter result Nägeli gave at first the name *Isagität*, but later he changed it to *Oligodynamic*.

As to the effect of solutions generally on *Spirogyra* and other cells, Nägeli enumerates three ways in which they may prove fatal—physically, by inducing plasmolysis, for instance; chemically, by poisoning the plant; and lastly, oligodynamically. The last-mentioned resulted when the chemical solution was so diluted that the reaction could not be one of simple poisoning, but must be due to some other influence.

Nägeli pursued the research still on the same lines; he put the filaments into Dr. Löw's solution, diluted quadrillion-fold, till, finally, he concluded that no calculable trace of the poisonous silver nitrate was present; the result was equally satisfactory from an oligodynamic point of view, the cells died often in less than four minutes. He now turned his attention to the water used in the solution, which was distilled in the ordinary way, and the conclusion arrived at was a startling one, namely, that pure water is a dangerous and hurtful fluid. After boiling, the evil effects diminished, they did not quite disappear.

Another series of experiments was tried with mercuric chloride—a still stronger poison. This was diluted down to septillion-fold, and the plants died as before. Professor Cramer has calculated that in order to dilute 1 milligramme of mercuric chloride to this extent, we should require more water than there is on our planet. A globe of water would be necessary thirteen million geographical miles in diameter, which would reach almost from the sun to Venus, and the molecules in such a solution would be separated from each other by a distance equal to two-thirds of the earth's diameter. He concluded, as we do, that the solution would be harmless, so far as mercuric chloride was concerned. This experiment proved conclusively that the evil was due to something other than chemical poison.

Attention was next turned to the glass vessels used, to see if the effect was produced by mechanical action. The glasses were tested in various ways, by movement, by covering them, etc., but no difference was visible in the behaviour of the solution; the *Spirogyra* died, and yet not always; in four out of this series of experiments the *Spirogyra* remained uninjured in the diluted solution. Again, boiling was tried, and in most cases the water was restored—we can hardly say to purity, but to harmlessness, or, as Nägeli terms it, it was rendered neutral.

Meanwhile, control experiments with pure distilled water had been carried on quite successfully in glasses full of *Spirogyra*. A culture was now tried with the same water and only a few filaments; as Nägeli expected, they died, if anything, still more quickly than in the diluted chemical solution, and water from the tap acted frequently exactly like distilled water.

A further research was now entered on to test the water, and as distilled water was, on the whole, more fatal than tap-water, he

fancied the poison might be a gas absorbed in the water and passing over in distillation. Carbonic acid, ammonia, and ozone were ruled out of the question, as any water in which Algæ live necessarily contains these gases in greater or less quantity, and such water, from spring or river, pool or pond, was found to be harmless. Moreover, when these gases were added to the cultures no effect followed. There remained nitrous acid to be suspected, and the more so that Munich water was said to contain a good deal of this acid which would not be eliminated by distillation. A solution was made containing potassium nitrite and sulphuric acid, which would liberate free acid in the distilled water. The same results followed as before, namely, chemical poisoning in the stronger solution, and the oligodynamic effect when much diluted. Griess's test was applied to estimate the quantity of nitrous acid in the ordinary water and in distilled water, but only once was there any reaction, and that so slight that no possible harm could be done by the amount present.

Very remarkable results in rendering water oligodynamic were obtained by treating it with various bodies considered practically insoluble in ordinary water. These were copper, silver, lead, iron, tin, and quicksilver. A whole series of experiments was tried with well-cleaned coins. Thus glass vessels, containing 100 or 500 cubic centimetres of water, were arranged with one, two, four, and eight gold coins. In these glasses were placed equal quantities of *Spirogyra* filaments. The plants in the glasses with the largest number of coins died first, the others succumbed more slowly, while the same species of *Spirogyra* lived in a healthy condition for weeks in ordinary spring water.

Again, the effect was tried with neutral water in metallic vessels of silver and platinum; the *Spirogyra* filaments died as before, and if copper coins were dropped in a glass containing the filaments the effect was so far localised that those nearest the coins were attacked first.

Nägeli had thus found an easy method of rendering water oligodynamic. A step further, and he made the still more remarkable discovery that he could again render the solutions neutral by the addition of quite insoluble bodies, trying first sulphur, soot, or graphite, then manganese, starch, flour, cellulose (filter-paper, cotton, linen, or wood), silk, wool, stearin, paraffin, etc., and the more of these bodies he put in, the quicker did the water recover its neutrality. He found that Algæ themselves would act in the same way if he put in plenty of them; they, too, had power to render the water neutral. This enabled him to account for much that had been hitherto inexplicable in the control experiments, where no proportion had been observed between the relative quantities of Algæ and water. This was put definitely to the proof, and it was found that the more water used, and the fewer the filaments, so much the quicker did oligodynamic reaction take place. Colloid substances, such as gum,

dextrin, albumen, glue, were also successfully employed as neutralising agents; while sugar and salt, which dissolved in the water, were almost wholly ineffective.

Results were for a time much confused by the persistence of after-effects in the vessels. The glasses in which coins had been immersed retained, to some degree, oligodynamic power, even after careful cleansing, and the effect continued, all unknown to the experimenters, during three or four subsequent cultures.

Nägeli had now arrived at what seemed to be a deadlock; that almost insoluble substances should exert so fatal a power, and that quite insoluble substances should be effective in counteracting it, was very mysterious.

Would electricity, or some such imponderable agent, he asked, account for the facts? And so there were started new experiments to put this theory to the test.

The first bore upon Temperature; but here again he was baffled, as neither excessive heat nor sudden change accounted for the hidden force.

The effect of Light was questioned, and that also gave no response.

There remained Electricity. Had it been generated in the water by the metals employed, and did *Spirogyra* cells act as an electroscope, exceeding anything hitherto known in sensitiveness? This theory also was found untenable. Vessels of water were separately charged with positive and negative electricity, and the plants were put in. They remained unaffected, and equally so when a strong induction current of electricity was passed through a tube in which filaments had been placed. The plants were further exposed to the direct action of electricity from a battery; this caused the cells to swell, and induced other changes, but it in nowise resembled the action of oligodynamics.

The only conclusion left was that here was a new power not to be accounted for by any present theories; either a new agent was at work, or the old agents had developed new properties.

The problem was attacked again, to see if the metals above-mentioned acted in solution or in mass. Gold and platinum are quite insoluble; they had already been tested, but the gold coins used were alloyed with copper. Pure gold was prepared from gold chloride, and the platinum was treated with hydrochloric acid; they were then found to be harmless, and also it was now proved that washing with acid would neutralise oligodynamic glasses. Thus it was, in all probability, some barely soluble substance, such as copper, that had to be dealt with.

Oligodynamic water was analysed, and was found to contain lead, zinc, copper, and iron. Some copper coins were left for a stated time in water till it became strongly oligodynamic. On analysis, the solution yielded one volume of copper to seventy-seven million volumes of water.

Twelve pennies were left four days in twelve litres of water, which then had a slightly metallic taste, and the *Spirogyra* plant died in it in one minute. The water generally used in experiment had been only one-fifth or one-tenth so strongly oligodynamic. Indeed, one part of copper in one thousand million parts of water may be fatal to *Spirogyra* cells. The copper went into solution as cupric hydrate combined with carbonic acid. Other metals, silver, zinc, iron, lead, mercury, acted similarly. The salts of these metals could also render water oligodynamic, and the vessels that had contained the solutions retained oligodynamic power, though to a less marked extent.

According to physicists, the saturation of a solution depends on the quantity of substance that a definite quantity of water will take up; as the solution becomes supersaturated, equilibrium is restored by the redeposition of the substance. The molecules of a barely soluble body tend to attach themselves to some other substance, and so pass out of solution. Thus copper, in water which contains carbonic or other acid, gives off its molecules slowly but continuously. These spread through the water, some attaching themselves to the wall of the vessel. As complete saturation is reached, still more molecules adhere to the walls. Should the copper be removed before this stage is reached, the water withdraws some of the molecules from the wall and an equilibrium is established between the copper layer and the solution. If such a solution be poured into a clean glass, a layer of copper molecules is again deposited, and the greater area of wall offered, the more molecules will be attracted from the solution.

In this lies the explanation of the neutralising effect of the various bodies above-mentioned. They simply attracted the metallic molecules. When oligodynamic water was well shaken up with powdered sulphur, and filtered, it was rendered completely neutral, and thus also sugar or salt failed, as they themselves pass readily into solution.

As to the action of gum, albumen, etc., Nägeli explains it on his own "micellar" theory of the structure of organised bodies, of the truth of which he finds here additional evidence. He considers that organised bodies are built up of micellæ, invisible crystalline bodies, formed by the aggregation of chemical molecules, and that colloid substances form with water "micellar solutions"; the copper molecules attached themselves to the micellæ as they would to larger bodies. The Algæ themselves acted as neutralising agents when their number was in excess, and in this case the molecules could only act very slowly or not at all.

Metallic substances would, in the same way, be removed from lakes, rivers, etc., by the presence of large quantities of insoluble substances.

The water used in the experiment was supplied by the town (Munich). It was conducted through lead pipes terminating in the

laboratory in a brass tap. The first litres drawn were strongly oligodynamic, from contact with the lead and with the copper of the tap; after some quantity had been run off, the water was neutral. The distilled water was rendered hurtful by the apparatus used. Neutral water could be obtained at any time by distilling in glass alone.

Nägeli's papers on oligodynamics were handed to Professor Cramer of Zürich in the spring of 1892, who at once instituted a careful examination of the recorded experiments, with the result that this peculiar power of weak metallic solutions was completely verified. He followed very much on the lines of the previous research, testing the effect on *Spirogyra* cells of copper, copper sulphate, mercury, mercuric chloride, and Dr. Löw's solution of alkaline silver nitrate, and the results, with slight variations, corresponded to those already determined by Nägeli.

The water used in Zürich comes from the lake, is exceptionally pure, and is conveyed to the town in iron pipes. With the exception of the first litre drawn, which had been in contact with the brass tap of the laboratory, it was, in Nägeli's sense, absolutely neutral. The distilled water usually employed was prepared in a copper vessel lined with tin. It reacted variously, and especially seemed to lose oligodynamic power if it had been in several vessels, or if it had stood some time after preparation. In this connection, we may also note that he found the oligodynamic reaction was in proportion to the size of the vessel used in experiments. If there was much of the metallic solution in a small vessel, the effect on the cells was rapid; the same solution acted much more slowly when a drop of it was used on a glass slide where the glass surface exposed was so much greater in proportion to the quantity of metallic molecules present. The solution became weaker the longer it was allowed to stand, and repeated boiling or filtering neutralised it altogether. Water distilled in glass was, he also found, invariably neutral.

Professor Cramer chose, for the purpose of research, three species of *Spirogyra*; one of these, *Sp. quinina*, is of a delicate texture with only one spiral chlorophyll band, the other two, *Sp. densa* and *Sp. setiformis*, are coarser. The reaction depended, to a certain extent, on the species; *Sp. setiformis*, with closely-wound spirals, always resisted the action of the metal longer than the other two species. The sensitiveness of the plant was, however, intensified if it had been long under cultivation in the warm temperature of the laboratory, in which case the cells lengthened, and the spirals were, in consequence, looser and more sparse. He used for his control cultures large glass vessels filled with ordinary Zürich water.

Oligodynamic reaction was most easily obtained by water in which copper had remained from one to three days. If a healthy plant growing in neutral water were strewn with copper turnings, the chlorophyll bands very quickly shrunk from the sides of the cells in contact with the copper. A glass slide that had been in contact

with the moist copper stage of the microscope proved strongly oligodynamic after an interval of sixteen days.

Professor Cramer directed his attention chiefly to the action of copper and mercury upon water; he obtained them chemically pure, and placed them in vessels, some of which were carefully stoppered and full to the brim with neutral distilled water, others were only half full, and were supplied with oxygen and carbonic acid. The water in both cases became oligodynamic, the latter more powerfully; but though, in the former, the air had been carefully excluded, the water gave an acid reaction. Zürich water, which was chemically neutral, contained very little chalk, but there was enough to cause discolouration, as the carbonic acid, which had held the lime in solution, was set free and passed over with distillation. Besides this acid, Professor Cramer thinks there might be traces of silicic acid present.

In addition to the neutralising agents enumerated by Nägeli, it was found that iron rust was very effective, as also *Leptothrix ochracea*, an Alga belonging to the Schizophytæ rich in ferric hydrate.

With the various solutions the same effects were produced as already recorded; first that of chemical poisoning in the cells when much of the deleterious agent was present, followed by the equally fatal oligodynamic reaction in diluted solutions. At a certain stage of dilution, if neutral water were employed, all reaction ceased.

ANNIE LORRAIN SMITH.

IV.

Some Extinct Sharks and Ganoid Fishes.[1]

OUR conceptions of some of the more primitive groups of fishes have been greatly modified during the past few years. The discovery of nearly complete skeletons has shown that the fragmentary fossils originally compared with the corresponding hard parts of existing animals truly belong to strange races of which no idea could be formed from their examination alone. Sharks, sturgeons, mud-fishes, and the fringe-finned ganoids, in Palæozoic times seem to have flourished under almost as many modifications as characterise the modern bony fishes in the fauna of to-day; and it becomes more and more evident, as discoveries continue, that the common ancestors of the class date back to a period hopelessly remote, so far as the palæontologist is concerned.

Among those who have contributed an important share to the formation of these new conceptions, Dr. Anton Fritsch, of Prague, merits our special gratitude. He has for many years collected the fossil vertebrata from the Permian gas-coal formation of Bohemia, even preserving the pyritised specimens by removing the decayed bone and taking electrotypes of the impressions; and his great work descriptive of these fossils is the most painstaking and elaborately illustrated monograph of its kind. Quite lately, a new instalment of the third volume has appeared, and the account of the fishes is now extended to include the Acanthodians and the first section of the Palæoniscids.

Several years ago, in an earlier section of his work, Dr. Fritsch published the first detailed description of the singular primitive sharks known as the Ichthyotomi, supplementing the beautiful figures and brief notes of M. Charles Brongniart in his monograph on similar fossils from the French Coal-measures. Now, he adds further to our knowledge of the sharks by a detailed chapter on the Acanthodians; though in this case, unfortunately, he is able to contribute comparatively few new facts of fundamental importance, merely confirming, revising, and systematising the observations of previous authors.

[1] Fauna der Gaskohle und der Kalksteine der Perm-formation Bohmens. Vol. iii., pt. 2. By Professor Anton Fritsch Prague: F. Rivnac, 1893.

The Acanthodian fishes, discovered in Palæozoic rocks from the Lower Devonian upwards, have always presented difficulties to systematists; but it is now generally agreed that they are a peculiar order of sharks. As to the precise significance of several parts of their skeleton, however, there is still much disagreement, and Dr. Fritsch himself wisely refrains from very definite expressions of opinion.

The general aspect of the type-genus *Acanthodes* is shown in Dr. Fritsch's outline figure reproduced below. The cartilages of the head are calcified with granulations, as in sharks; and there are splints covering the jaws, but neither these nor any other skeletal parts have the structure of bone. The roof of the skull is covered at least in part with small dermal plates, and there is a ring of plates round the eye. The gill-arches are so much crowded that Dr. Fritsch thinks the gill-slits must have been covered by a flap of skin, as in the existing Chimæroids. The notochord must have been persistent, and there are rarely traces of the neural and hæmal arches above and below the vacant space it once occupied. The tail is heterocercal,

FIG. 1.—Outline restoration of *Acanthodes bronni*; Permian, Rhenish Prussia. (*After* Fritsch.)

the upper lobe being much produced; and in front of all the fins, except the caudal, there is a strong spine. The body is covered with little quadrangular scales, consisting of dentine enamelled at the surface; and the "lateral line" does not pierce these scales, but runs between two series.

Among the Permian Acanthodians Dr. Fritsch recognises three genera, one of which is destitute of pelvic fins. The large size of the paired fins, however, is one of the chief characteristics of *Acanthodes* itself; and, as pointed out by Professor Cope, there is more evidence of a continuous pair of lateral fin-folds in these Acanthodians than in any other fishes. Some of the Devonian genera (*e.g.*, *Climatius*), exhibit between the pectoral and pelvic fins a close and regular series of paired spines, in every respect identical with those supporting the fins themselves; and it is quite possible that these intermediate spines may also have been connected with fin-membrane.

It is strange to find a group of fishes like the Acanthodians among the sharks—fishes which at the present day never exhibit structures of the nature of membrane-bones, and never have the

cartilages of their limbs excessively abbreviated. It is, however, stranger still to have to regard the Palæoniscidæ as primitive representatives of the modern sturgeons; and yet Dr. Fritsch, following all who have deeply studied the subject, is fully convinced of the necessity of this course. He describes the genus *Trissolepis*, giving the restoration copied below; and this he regards as the type of a distinct family to be placed in the same sub-order as *Palæoniscus* itself.

The Palæoniscidæ, as a general rule, conform to the type shown in Fig. 2; being essentially sturgeons (as Dr. Traquair first pointed out) covered with regular rhombic scales and provided with a complete

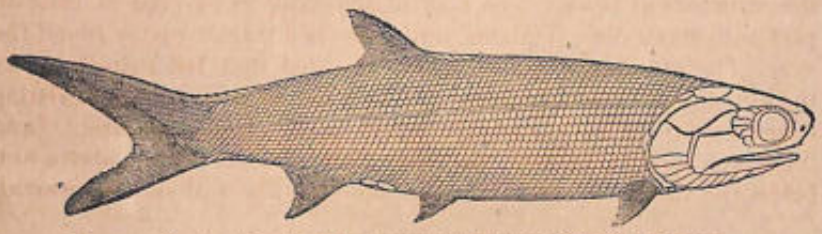

FIG. 2.—Restoration of *Palæoniscus macropomus*; Permian, Germany. (*After* Traquair.)

gill-covering apparatus. In some cases, however, the scales disappear except on the upper lobe of the tail; and in a single instance, briefly noticed by Dr. Traquair, the scales become deeply overlapping and almost or quite cycloidal.

Now, the great interest of Dr. Fritsch's new contribution to the subject consists in the fact that it is the first detailed and illustrated account of one of these ancestral sturgeons possessing cycloidal scales. Moreover, it is to be noted that in *Trissolepis* the cycloidal scales are confined to the trunk, while the rhombic scales persist on the upper lobe of the tail. One and the same fish thus displays

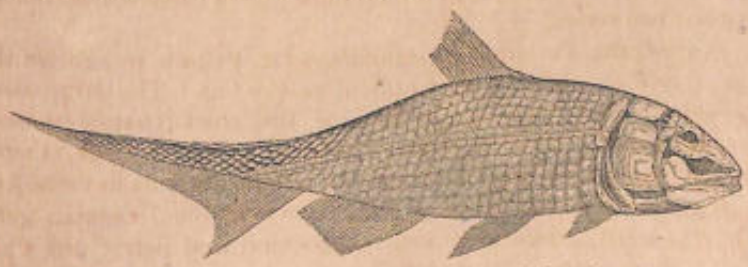

FIG. 3.—Restoration of *Trissolepis kounoviensis*; Permian, Bohemia. (*After* Fritsch.)

features which were not long ago regarded as the essential characters of distinct groups; and the shape of the scales in future must only be cited among the minor diagnostic points of the old "ganoid" fishes. We are acquainted with Jurassic fishes, indeed, in which the anterior scales are rhombic, ribbed, and united by peg-and-socket joints,

while the scales on the tail of the same fish are as thin and deeply overlapping as in a herring.

In a general review it does not seem appropriate to enter more closely into the details of Dr. Fritsch's work; Dr. Traquair has already published a critical notice in the *Geological Magazine* (dec. iii., vol. x., pp. 175–178). We would, however, urge all who are interested in Comparative Anatomy to study the original for themselves.

A. SMITH WOODWARD.

V.

Notes on Warning and Protective Colour in Lepidopterous Larvæ.

IN the April number of NATURAL SCIENCE, Mr. George Carpenter deals with the vexed question of "Colour in Insects." The whole subject of animal colouration has been much debated, and as it is only by means of a long series of careful experiments and close observation that we can hope to arrive at any clear understanding of it, every particle of evidence, however small, may have some value. From this point of view the following notes may not be without interest.

The experiments made by me as to palatability and colour-relation in lepidopterous larvæ were, as Mr. Carpenter correctly states, entirely confirmatory of those previously conducted by Mr. Poulton and others. They have been recorded in detail elsewhere (*Trans. Ent. Soc. Lond.*, 1892, pt. iii.), and I therefore give only a brief sketch of them here, together with a few additional facts which have come under my notice since writing the above-mentioned paper. Experiments as to the palatability of conspicuous larvæ were made with four species, viz., with larvæ of *Diloba caeruleocephala*, *Cucullia verbasci*, *Acronycta psi*, and *Bombyx rubi*. Larvæ of the two first-named species were given to a tame jackdaw, which had been taken unfledged the previous year, and could never have seen any larvæ except those I gave him, unless some had dropped occasionally from a beech tree which overhung his cage. The larvæ of *D. caeruleocephala* were taken feeding freely exposed on pear trees; they were blue, yellow, and black, very conspicuous, and not hairy.

The bird was accustomed to take anything from my hand, and would always seize and attempt to pull to pieces any object presented to him, whether he was hungry or not; he was, therefore, a good subject for experiment.

On offering him a larva he appeared very suspicious, and for a long time would not take it. Then he tasted it, but dropped it at once, shaking his head violently, and evidently disliking it. It was tasted once more, and then left uneaten. The next day he was offered a common smooth green larva (species unknown) and ate it without hesitation. He was not very hungry on either occasion.

Larvæ of *C. verbasci* were taken feeding exposed on the upper side of leaves of mullein; they were green, yellow, and black, very conspicuous, and not hairy. One was offered to the jackdaw when the bird was very hungry; it was refused at first, then tasted once only, and dropped with greater signs of disgust than in the case of *D. caeruleocephala*. After this he would not take anything at all from me for a considerable time, and appeared very uncomfortable.

In the case of *Acronycta psi* and *Bombyx rubi* the animals experimented on were three slowworms (*Anguis fragilis*) and one lizard (*Zootoca vivipara*). These were purposely kept very hungry, but though the larvæ were left with them for three days they refused them entirely, never even attempting to taste them. Both these larvæ, however, were hairy as well as conspicuous, and, therefore—assuming that conspicuousness means unpalatability—were doubly protected. The fact that an animal may possess more than one kind of protection must of course be taken into account in making these experiments, and for this reason the experiments with *D. caeruleocephala* and *C. verbasci* were the most satisfactory, because here the unpleasant attitude was almost certainly taste only. Mr. Poulton says (*Proc. Zool. Soc. Lond.*, March 1, 1887) that Mr. Jenner Weir found the two last-named larvæ to be disregarded both by birds and lizards. This certainly supports Professor Wallace's original suggestion of the unpalatability of conspicuous larvæ to "some, at least, of their enemies," while, on the other hand, the results obtained by me also favoured Mr. Poulton's suggestion as to hunger putting a limit to this method of defence, since the larvæ were tasted, and tasting would be as fatal to them as eating.

Mr. Beddard ("Animal Colouration," p. 164) found that *A. psi* was eaten by *Lacerta viridis*, and at least tasted by a thrush, but he does not mention whether either was hungry on these occasions, a point of some importance. In any case, his results differed from mine in this experiment, but then a different species of lizard was used. *L. viridis* would seem, from other experiments of Mr. Beddard's, to be less sensitive to unpleasant attributes than other lizards, *Z. vivipara*, for instance. This difference of behaviour does, as Mr. Beddard points out (*loc. cit.*, p. 155), show that unpalatable animals with warning colours are not always exempt from attack, but this very fact rather tends to support the view held by Mr. Poulton that this means of defence can only safely be adopted by a limited number. On the other hand, it militates strongly against Dr. Eirig's theory, quoted by Mr. Beddard, that brilliant colours (*i.e.*, abundant secretion of pigment) are the cause of inedibility. Exceptional cases like that of *L. viridis* eating *Acronycta psi* are difficult to explain on this hypothesis, but they appear more comprehensible on the theory of limitation. *A. psi* might be regarded as one of the few examples of the failure as a means of defence both of the unpleasant attribute and its advertising warning colours, and *L. viridis* as having had its power of

eating this larva with impunity developed by the action of Natural Selection, after necessity had first driven its ancestors to the attempt.

My experiments were made on a less extensive scale than those described by Mr. Beddard as undertaken by Mr. Finn and himself, and are, therefore, perhaps, of less value; but as far as my experience went I was not at all able to confirm Mr. Beddard's statements as to the "behaviour of animals when offered inconspicuously-coloured" prey, resembling that with which they approached conspicuous forms. I repeatedly gave common green smooth larvæ and other species with protective colouration both to the bird and lizards, and invariably found them accepted and eaten without the slightest hesitation, except just after distasteful food. I scarcely think the garden slug, which Mr. Beddard saw refused by several birds, is a fair example of a protectively-coloured animal treated as if it were inedible. The slime of the slug would be disliked by many animals, and there is evidence from many of the experiments that it was so disliked, since it was only swallowed "after much rubbing on the ground." The slime would seem to be as effectual a protection, apart from all question of colour, against some pursuers as the hairs of the larva of *Bombyx rubi*; the latter cannot be said to be conspicuously-coloured, and yet was refused by all the animals with which I experimented. I did not find a single case of a larva which was both protectively-coloured *and smooth* being refused by animals; such larvæ were always eaten with avidity. It will, however, be interesting to extend these experiments and note the results carefully, and it is one of the great merits of Mr. Beddard's book that he therein points out so many lines for investigation, and calls attention to many details which are worthy of further consideration.

With regard to experiments on the colour-relation between certain lepidopterous larvæ and their surroundings, the results obtained by me were the same as those obtained by Mr. Poulton previously with the same species, except that in one case my results were, perhaps, slightly more definite.

I reared in different surroundings 19 larvæ of *Rumia cratoegata*, 42 larvæ of *Catocala nupta*, 10 larvæ of *Catocala fraxini*, and 29 of *Mamestra brassicæ*. Dark surroundings produced in *R. cratoegata* very dark brown or nearly black larvæ, while in green surroundings larvæ from the same parent became of varying shades of green or greenish-brown, with touches of crimson exactly corresponding to the red of the thorns and one side of the young shoots of the food-plant. The resemblance to twigs of hawthorn was very perfect, and was heightened by the angular attitude adopted by the larvæ at rest.

For *Catocala nupta*, dark, green, and white surroundings were used, and the dark surroundings produced darker brown larvæ than the green surroundings, while no difference in shade was perceptible between those in green and those in white surroundings. Mr. Poulton

considers (*Trans. Ent. Soc. Lond.*, 1892, pt. iv.) this species to be less susceptible than other Catocalidæ.

Catocala fraxini certainly showed a distinct difference, not only of shade, but of colour, larvæ reared in green surroundings being distinctly greenish in hue, while those in dark were brownish. Mr. Poulton obtained only a difference in shade, otherwise his results were similar.

With *Mamestra brassicæ* I had negative results; the larvæ were not affected at all by surroundings, and I believe this unsusceptibility to be due to the burying habits of this species, which render colour of secondary importance to the larvæ. Mr. Poulton's results differed a little from mine, but he thought that his larvæ were not in such a healthy condition as mine, which might account for this. There are, however, special difficulties in investigating *M. brassicæ*, and Mr. Poulton considers the experiment worth repeating.

Since publishing these results, I made some experiments, at Mr. Poulton's suggestion, on larvæ of *Triphena pronuba*, which I was rearing for him for other purposes. I had 59 larvæ in different surroundings, and as this species, like *M. brassicæ*, buries by day, I used different-coloured materials on the floor of the cylinder, such as white sand, yellow gravel, pounded red flower-pot, and, in one case, small coal. The larvæ buried in all these, except in the coal, which they evidently disliked, but the results were, as in *M. brassicæ*, negative, with two possible exceptions from green surroundings, without any burying materials; these were, however, not very definite. The burying materials were in all cases used in addition to sticks of the suitable colour mixed with the food-plant.

Besides the foregoing experiments, I also made some notes on the red spots in *Smerinthus* larvæ, from larvæ of *S. tiliæ* sent me by Mr. Perkins at Mr. Poulton's request. These larvæ were all from parents which had been spotted in the larval stage. Unfortunately, many were injured by their transit by post, and I only succeeded in rearing four, which were all spotted. I also captured a larva of *S. populi* in which the spots were extremely well marked. The spots were certainly protective in effect, on account of their resemblance to the dark spots or blotches common on leaves of poplar, and this fact was long ago pointed out by Mr. Peter Cameron (*Trans. Ent. Soc. Lond.*, 1880); but although he considered the likeness to *galls* (*Phytoptus*) very striking, it seems to me, in *S. populi* at any rate, far greater to flat spots on the leaf seen with the light shining through them.

In *S. tiliæ* I studied the development of the spots very carefully, and in one individual I found them very linear and strongly suggestive of coloured borders; in the others they were much bolder and rounder, and presented quite a different appearance. Professor Weismann, as is well-known, considers the spots as on the way to develop into borders, while Mr. Poulton takes the view that we

have to do with a gradual contraction of borders into spot-marking. The spots certainly increase in area while developing, and if we are to regard the occurrence of spots as a recapitulatory character this fact would seem to support Professor Weismann's theory. If, however, they are reversionary, which now seems to me more probable, as they do not occur in all the individuals of a species, the increase in area would not have the same importance. The number of larvæ reared by me was, however, too limited to draw conclusions from safely, and had I had more individuals under observation, some might have presented different appearances.

LILIAN J. GOULD.

VI.

Cannibalism among Insects.[1]

IT is known that the caterpillars and larvæ of lepidoptera, crickets, and locusts feed on plants, of which they consume great quantities, in order to reach their full development or to prolong the imago condition, and to fulfil their mission of propagating the species.

Notwithstanding the vegetarianism of the insects mentioned, cases are known in which they abandon their usual habits and become addicted to a flesh diet, and, what is still more strange, they feed upon their own kind.

The larvicide instincts of the caterpillars of certain moths have been known for a long time, the cannibalism having been observed in various species deprived of their liberty, and brought up in confinement, either with or without vegetable food. Thus, for example, the caterpillars of the moths *Calymnia trapezina*,[2] *Agrotis ypsilon*, *Heliothis armiger*, and others, finding themselves in captivity, abandon their vegetable regimen and devour their companions, whether out of disgust at finding themselves imprisoned, or for want of fresh vegetable food, or for other causes of which we are ignorant. In any case, after the development of the disordered appetite, the caterpillars do not any longer care for vegetable aliment, and they endeavour to satisfy, in any way, their newly-acquired carnivorous habit. Out of a number of caterpillars associated together, of the above-named species, finally only one remains, which in the savage warfare, either by its strength or subtlety, has succeeded in saving itself, devouring the last of its companions which placed its existence in danger.

What has been just said only relates to caterpillars in captivity. So much greater is the fact which I discovered, that in the free condition there also exists cannibalistic larvicide. During my voyage in Southern Patagonia, in 1874, I noticed that the cannibalistic instinct was very prominent in the caterpillars of the district, whether free or imprisoned.

[1] Translated from the *Anales de la Sociedad Científica Argentina*, Nov., 1892, pp. 236–238.

[2] The caterpillars of this moth are said to be often carnivorous in the wild state, and to render considerable service to fruit-growers by devouring the destructive larvæ of *Cheimatobia brumata*.—Ed.

All the caterpillars belonging to a particular group or family showed a preference for the flesh of their own relations. They devoured one another in considerable numbers, rarely feeding on the plants suitable for their nourishment. The caterpillars of the family of the Bombyces devoured their kin with skin and hair, and they went as far as to break through the cocoons in order to finish with the chrysalis within.

In a similar manner, the caterpillars of the Noctuids behaved with those of their own kind and with those of the Bombyces.

The caterpillars of these latter attacked also those of the former without any exception. The most voracious of the Noctuids was the caterpillar of *Heliothis armiger*; a single one of these consumed, in twenty-four hours, six or seven other caterpillars.

The caterpillar of the well-known butterfly, *Pyrameis carye*, is also a carnivorous cannibal, but with moderation, preferring always fresh plants to meat, while the others, and chiefly those of the Noctuinæ, after having tasted the flesh of their own kin, would no longer touch vegetable food.

I explain this particular character of the Patagonian caterpillars in the following manner. During the principal part of the summer in Patagonia there is considerable heat and dryness which causes the vegetation quickly to be parched up and scarce. When this happens the caterpillars lose their means of subsistence, but, in order that some may survive, the struggle for existence has taught them another means of subsistence, that is, the flesh of their own kind. Once this instinct has been inherited, the descendants will use it whenever an occasion presents itself, and, in many instances, even when there is no lack of vegetable food. In some cases it is necessity which produces the habit; in others, inheritance which leads up to it; thus new biological characters are formed.

Of other herbivorous insects cannibalism has been noticed in crickets in confinement. The first notice of this strange taste we owe to Sir W. Brodie (*Canad. Entom.* vol. xxiii., p. 137, 1891), whose observation has been recently confirmed by Mr. Philip Laurent (*Entom. News*, vol. ii., p. 180, 1891).

In an assemblage of many crickets kept for certain observations in a rearing drawer, or box (caja de herborizacion), the numbers diminished from day to day; at last only one—not a little fattened—remained by the side of the remains of his former companions.

Hitherto cannibalism among the crickets has been noticed only among captives, but I am now enabled to state that, under certain conditions, cannibalism is present among some Orthoptera in the free state, at all events among the locusts.

In the summer of 1883, in which the excessive heat and drought had brought about the nearly entire disappearance of the vegetation in a good part of the country, and more particularly in the broken country of the Banda Oriental, I had occasion to make a journey

from San José to Mercedes. At one place, Las Piedras, at which the diligence stopped, I noticed great numbers of locusts of the species *Pezotettix vittiger*, *P. maculipennis*, and *P. arrogans*, which covered the ground and rocks. My attention was attracted by the fact of seeing around one locust a number of other individuals of the same species, which were eating its soft parts even while it was yet alive and protesting vigorously. I saw different attacks, in which the conquerors, two or three at a time, got hold of the weaker members of their own kind, throwing them over, and opening the abdomen in order to devour the entrails, these being the softer and more savoury portions, since they still contained some of the vegetable food. Cannibalism here appeared in its lowest development, and the numerous remains of those which had been eaten bore witness to the extent to which the process had been carried.

In the face of facts of this character, it seems certain that nothing is sacred in Nature, when the prolongation of life, for the sake of the preservation of the species, is concerned.

CARL BERG.

VII.

The Classification of Arachnids.

NATURALISTS have long looked forward to a day when the systematic arrangement of living creatures will exhibit the real relationship which they bear to one another. All honest classificatory work helps science to realise this great ideal, but much is of the nature of raw material which may long remain without apparent use. Occasionally, however, we meet with contributions which seem to add considerably, and at once, to the progress of the work; and such are several recent memoirs on the spiders and their allies.

In 1851, Schiödte described a new genus of Malayan spiders, *Liphistius*, of most remarkable structure. This genus was at first classed with the Territellariæ—the tribe which includes the trap-door species, and the well-known large hairy "bird-killers" of the tropics. More recently Thorell has considered that the genus should form a tribe by itself, but be included with the Territellariæ in the sub-order Tetrapneumones, or spiders with four lung-sacs; and now Pocock (1) gives good reasons for assigning to *Liphistius* a still greater classificatory value, and placing it in solitary grandeur over against all other known spiders. Several striking characters seem to support this view. The abdomen in other spiders has lost nearly all traces of segmentation, but *Liphistius* exhibits nine dorsal and two ventral sclerites. The spinning-mammillæ, which in other spiders leave their embryonic position as ventral appendages of the abdominal sternites behind the lung-sacs, and migrate to the extreme apex of the abdomen, retain in *Liphistius* their primitive place beneath the middle of the abdomen. This latter character suggests the names proposed by Pocock for his new divisions, the species of *Liphistius* being called Mesothelæ, and other spiders Opisthothelæ. His primary division of these latter tallies with the old classification into Tetrapneumones and Dipneumones, but he agrees with Simon that the relationships of a genus (*Hypochilus*) of four-lunged spiders are with the latter rather than with the former group, and, therefore, suggests the terms Mygalomorphæ and Arachnomorphæ to supersede those time-honoured names. In all spiders of the latter group (except *Hypochilus*) the hinder pair of lung-sacs are replaced by tracheæ. *Liphistius* agrees with the Mygalomorphæ in having four lung-sacs, but, in several points, it shows affinities with the other division. Alone

among spiders it possesses four pairs of spinning mammillæ. While the Mygalomorphæ rarely have more than two pairs, the Arachnomorphæ always have three, and Pocock thinks that the extra central spinning organ (cribellum), found in some families of the latter, represents the fourth pair of spinners of *Liphistius* fused together.

In this new classification, therefore, the archaic characters of *Liphistius* are emphasised, and it is regarded as a survival of an ancestral form of spider in which the abdominal segments have not yet become fused, and still bear the spinners on their ventral aspect. The animal is, to some extent, a connecting link between the spiders and the scorpion-spiders (Pedipalpi). We are thus led to consider the relationships which exist between the various orders of the Arachnida—a subject also dealt with by Pocock in a later paper (2). His principal contention is that if arachnids are descended from ancestors which must have possessed a long series of nearly similar segments, those modern representatives of the groups in which the segmentation is best preserved must be the most primitive. On these grounds he considers the scorpions as the lowest living branch of the arachnid stem, in opposition to the views of Thorell and others, who have regarded them as the highest. There can be no doubt that Pocock's opinion will meet with general acceptance among biologists.

Deriving the arachnid orders immediately from an ancestor with a long abdomen of twelve segments and a telson, Pocock indicates the modifications which have probably occurred in each group. In the scorpions these segments are all present, but the hinder six are much reduced in bulk to form the post-abdomen, the telson being utilised as the sting. In the scorpion-spiders there has been a great reduction of the hindermost segments; the tail in *Thelyphonus* represents the telson. *Liphistius*, with its much-shortened and partially segmented abdomen, connects these with the higher spiders in which the abdominal segments are completely fused together. Nearly related to the Pedipalpi, but with tracheæ instead of lung-sacs, are the Solifugæ[1] and false-scorpions (Chelifers), with the number of abdominal segments complete, or but slightly reduced. On a higher branch of the stem bearing these come the harvestmen (Phalangida), with the abdominal segments generally reduced in number, and fused with the cephalothorax anteriorly. From these the mites (Acari), with the whole body fused together, must probably be regarded as a degraded offshoot.

The relation of the mites to the rest of the Arachnida has been also recently discussed by Bernard (3), who gives reasons for believing that these creatures are not examples of true degradation, but that they have become fixed at a larval stage of development. Comparing their digestive, vascular, and nervous systems with those of the

[1] Bernard (*A. M. N. H.* (6), vol. xi., 1893, pp. 28–30) has announced recently the discovery of a sensory organ on the pedipalp of *Phrynus*, similar to that on the homologous appendage of *Galeodes*.

higher Arachnids, he shows that they want the greater number of the abdominal segments, while they possess, in a fair state of development, organs homologous with those in the anterior part of the body of the higher Arachnids. He speaks of the Mites as "larval Araneids," but few naturalists will be inclined to regard them as a direct offshoot from the spiders. Extreme smallness—one of the supposed advantages gained by fixation at the larval stage—has been acquired by many true spiders. Moreover, spiders, like most other Arachnids, have no larval stage, but leave the egg in a form similar to that of the adult animal. Whether degraded or arrested, the affinities of the mites seem to be with the harvestmen, from which group they are not readily defined.

Trouessart (4), however, supports the view that, on account of their development with a metamorphosis, the mites should form a special sub-class of the Arachnida, and he separates the vermiform mites, such as the Phytopti, from the rest as a separate order; but the larval stage of mites (in which they have but three pairs of limbs) seems quite a secondary adaptation, for, according to Wagner (5) the fourth pair appear in the embryo, to be afterwards concealed beneath the skin of the larva and then to re-appear during metamorphosis.

It is remarkable that the theory of fixation at the larval stage has also been invoked by Von Kennel (6) to account for the origin of the microscopic "water bears" (Tardigrada), which have generally been regarded as degraded arachnids. He compares them with certain midge-larvæ (*Cecidomyia*) which have the power of parthenogenetic reproduction, and suggests that if male organs were also precociously developed a species might become permanently larval in form. Though he does not assert that the Tardigrada are "arrested" flies, the acceptance of his views—which certainly have much to support them—would probably lead to the removal of the group from the arachnids to the insects, in which the larval stage is often of such supreme importance.

The replacement of lung-sacs by tracheæ in several groups of arachnids opens up a question of much interest. Reference has been made in NATURAL SCIENCE (vol i., p. 524) to the supposed origin of the lung-sacs from the invagination or enclosure of gill-bearing appendages. If this view be accepted, we must regard the abdominal tracheæ in the higher spiders, harvestmen, false-scorpions, etc., as simplified lung-sacs. Pocock (2) suggests that increased lightness would be an advantage derivable from this change; but Bernard (3, 7, 8) considers the tracheæ the more primitive, and the lung-sacs elaborated from them. He would derive the whole ventral series of arachnid breathing-organs, spinning-glands, coxal glands, and poison-glands from the ventral setiparous glands of an annelid ancestor. The tracheæ of insects, on the other hand, he believes to have arisen from the dorsal series of setiparous glands, and he finds the hairy area around the stigmata of the chrysalis of the Vapourer Moth

(*Orgyie antiqua*) recalls an annelid parapodium with its setæ. This supposed difference in origin of the breathing-tubes in the two classes leads to a supposed corresponding difference in the origin of the limbs, those of the insects being traced to the ventral series of parapodia of the annelid, and those of the arachnids (and also crustateans) to the dorsal series. Ingenious as this theory is, the great cleft which it makes between the insects and arachnids by destroying the homology of their appendages, seems a grave objection. The higher annelids have undergone too considerable a development along their own particular line to be safely regarded as approximating to the extinct ancestors of a distinct phylum.

However, in a recent memoir, Sinclair (9) also suggests the derivation of lung-sacs from tracheæ rather than that of the latter from the former. He describes peculiar breathing-organs in the Scutigeridæ, consisting of dorsal slits opening into air-sacs, from each of which a number of tubes, arranged in two semicircular masses, are given off; these organs are believed to represent a stage between ordinary tracheæ and the lung-books of spiders, the lungs of scorpions representing the highest development of the series.

But the relationship between the various kinds of breathing-organs depends upon the question whether the lung-bearing or tracheate orders of arachnids are the more primitive. In the scorpions, we find four pairs of lung-sacs, and the full segmentation of these animals has already been dwelt upon as evidence of their archaic nature. Moreover, while palæontology has not yet thrown much detailed light on the history of the arachnid groups, strong confirmation of this view is derived from the fact that remains of scorpions occur in the Silurian rocks, and that these are the oldest air-breathing animals of which we have certain knowledge. In the scorpion-spiders, and in the lower families of the true spiders, two pairs of lung-sacs occur, but in the higher spiders, as already mentioned, the hinder pair of these are replaced by tracheæ. Here, again, such knowledge of fossil spiders as we possess supports the view that lung-sacs preceded tracheæ, for while four-lunged spiders lived in Carboniferous times, the higher members of the group have only been found fossil in Tertiary formations. The comparatively recent orgin of the higher spiders is also suggested by a study of their distribution, most of the families and genera being world-wide in their range. Marx (10) has recently remarked that among 292 species of spiders from the Arctic regions, none can be referred to special genera; and British naturalists who have read Hudson's "Naturalist in La Plata" must have observed that, while the mammals and birds and insects described are far removed, indeed, from our native animals, in the chapter on spiders we are introduced to such familiar friends as *Tetragnatha*, *Zilla*, *Pholcus*, and *Argyroneta*. Among the four-lunged spiders we meet with the confined and discontinuous distributions so characteristic of old-time groups of

animals. For example, Simon (11) has recently described a new genus *Myrtale* from Madagascar, whose nearest affinities are with *Moggridgea* from South Africa, and *Migas* from New Zealand.

The remarkable group of marine arthropods known as the "sea-spiders" (Pycnogonida) were for long regarded as arachnids by some zoologists, and as crustaceans by others. Of late years, the great authority of Hoek and Dohrn has led to the relegation of these animals to a position independent of either group, indicating their supposed independent descent from vermiform ancestors. Recently, however, Morgan (12) has re-advanced the view of their arachnid affinities, dwelling on the similarity of the polar delamination observed in the division of their eggs to what occurs in the eggs of arachnids, and also on the presence of intestinal branches in the limbs, another arachnid character. The number of appendages is the great obstacle to bringing the pycnogons into line with the arachnids. Their four pairs of walking-legs, their chelate foremost jaws, and their palps are easily represented among arachnids, but what is to be done with the pair of extra limbs, behind the palps, on which the male—a true nursing father in this group—carries the eggs and young embryos? If we are to retain the pycnogons among the arachnids, we must either derive these extra limbs from a segment which has become aborted in the latter, or else, comparing them with the first pair of legs of a spider, regard the pycnogon's last pair of limbs as belonging to the abdomen.

The semi-parasitic life of pycnogons on hydroids, polyzoa, and other marine animals has always suggested that they are a degraded group, and degradation often masks affinity. In recent systematic work on pycnogons, Schimkewitsch (13) and the present writer (14) have directed attention to the relationships between the genera, and the former insists that the most primitive genera are those of most complex organisation. Not only do the number of appendages tend to lessen, the chelæ or palpi or both disappearing, but the number of joints in the appendages decreases and the beautiful and complicated serrate processes on the egg-bearing legs in the higher forms—the Nymphonidæ, for instance—degenerate into ordinary spines as we descend through the various series. There are few groups of animals in which a comparative study of adult living forms does not suggest the lines upon which progress has been made. In the pycnogons, however, as in most parasitic or semi-parasitic groups, such study can only tell us a tale of decline.

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GEO. H. CARPENTER.

VIII.

The Great Barrier Reef of Australia.[1]

THE Great Barrier Coral Reef of Australia is 1,250 miles long, stretching along the coast of Queensland from Torres Straits to Lady Elliot Island. The distance from the outer edge of the reef to the mainland varies from 10 to about 150 miles. Already raw material to the value of £100,000 is obtained annually from the reefs and the intervening water, and is exported from the colony.

The chief items in this produce come from the pearl, and pearl-shell, and the Trepang (or Bêche-de-mer) fisheries. In addition to such commercial interest, the great Barrier Reef is of the greatest scientific interest from the material it provides for collecting information as to the external features and detailed composition of coral reefs.

Mr. Saville Kent, Commissioner of Fisheries to the Government of West Australia, was afforded the opportunity, by the wise liberality of the Queensland Government, of making a detailed examination of this great reef. This sumptuous volume contains the first results of his labours. He hopes to publish fuller and more technical accounts of various animals examined at a later date. The present volume is designed to give both the scientific and the general public an idea of the vast and wonderful medley of life presented by a coral reef.

An interesting and novel feature of the volume is its copious illustration by photo-mezzotype plates.[2] These provide for us a series of pictures of the coral reef of almost unimagined beauty and value. As the reefs are uncovered only for short intervals, and under conditions unfavourable for drawing, we have had as yet, except for the verbal descriptions of observers, no idea of the actual appearance of a reef. At the end of the volume there are sixteen chromo-lithographic plates. It is impossible to deny that these are, artistically, far from pleasing; but even if they are exact representations of the animals, it may well be that the glaring colours of the tropic seas removed from the brilliancy of tropic sun to the cold atmosphere of England seem harsh and crude. Moreover, the æsthetic value of

[1] THE GREAT BARRIER REEF OF AUSTRALIA; ITS PRODUCTS AND POTENTIALITIES. By W. Saville Kent, F.L.S., F.Z.S., F.I.Inst. With a Chart; 48 Photo-Mezzotype plates; 16 Chromo plates; and many Woodcuts. Pp. 380. 4to. London: W. H. Allen & Co., 1893. Price £4 4s.

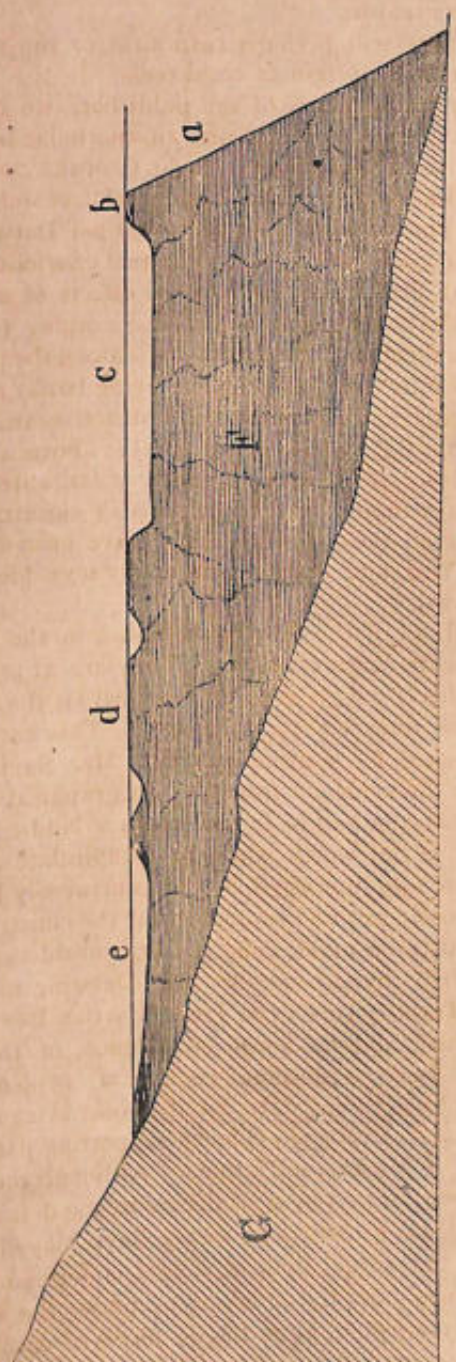
[2] See NATURAL SCIENCE, vol. i., p. 648.

colours is a mere convention—as anyone may observe who takes his pleasure in the Row this season.

Geologists and others will perhaps turn first to the section of the book which deals with the origin of coral reefs.

A figure which by the kindness of the publishers we have been able to reproduce shows how the reef forms an enormous buttress of rock F pressed against the sloping granite rocks G of the coast. Mr. Jukes, and after him the author, made most careful search for any evidence of upheaval, but failed to find facts against Darwin's subsidence hypothesis. At many points along the reef enormous masses of storm-raised coral at first sight simulate the effects of upheaval; but of exact evidence Mr. Saville Kent could find nothing to oppose seriously the subsidence theory. At many stations along the reef, large expanses of dead coral intervene between the living banks and high-water mark. Such banks are subjected to destructive atmospheric influences at every spring-tide, while living coral is above water only at the lowest spring-tides. Dead bivalve shells of large size such as *Tridacna* and *Pinna* occupy their original positions in close contiguity to the dead corals; but all these results could have been caused by upheaval of a foot or two, and there is practically no evidence for a prolonged era of upheaval.

On the other hand, all of the few big breaches in the Barrier's outer rampart are opposite large estuaries. They are at present too remote from the estuaries (from 30 to 60 or 80 miles) for the influence of fresh-water to have retarded coral growth. This condition is by itself strong evidence for subsidence, but Mr. Saville Kent relies most strongly on the evidence from the geographical distribution of animals that Australia had in former times a land-connection with New Guinea. It is not worth while to recapitulate the facts here, as all who are specially interested in the controversy will refer to Mr. Saville Kent's book; but he concludes that the construction of the Great Barrier of Australia under conditions of subsidence, and in accord with the original hypothesis of Mr. Darwin, is proved. Towards the end of Tertiary times the great Barrier Reef existed as a simple fringing reef, and moderate subsidence, of the occurrence of which we have abundant geological evidence, has turned it into its present condition. Mr. Saville Kent brings forward a considerable additional quantity of evidence supporting parts of the reef-formation theories of Murray and others. Although the Barrier Reef as a whole has been formed by subsidence, in the details of the process conditions of local growth and decay have played a large part. Thus, much of the actual bulk of a reef is formed of coral *débris*—*débris* which comes from the breaking up under the influence of storms of the rapidly-growing, more fragile forms of coral. On a reef at any time only a small area is actually alive, and that small area is turned most usually away from the mud and fresh-water on the landward-side, and turned towards the most abundant and freshest



DIAGRAMMATIC TRANSVERSE SECTION OF THE GREAT BARRIER REEF OF AUSTRALIA.

G.—shore of granite. F.—coral reef. *a*.—deep seaward face of reef. *b*.—actual barrier. *c*, *e*.—clear channels. *d*.—inner reefs.

(Vertical scale much exaggerated.)

supplies of sea-water. Again, nearly all masses of coral grow more or less like a fairy ring—living and expanding at the edges, decaying, getting ground down into *débris*, or dissolved by the sea-water at the centre. The action of various forms of life living on the coral also assists this process. Shell-fish grow on the coral and become embedded in it, and round the intruding giant clams embedded in the coral waves a region of decay is usually present.

A valuable part of the book is occupied by an account of the actual corals and coral animals. To those who are familiar only with the bleached and dried specimens of museums and cottage windows, the account of the marvellous colours of the living corals will be a revelation. The soft tissues of the polyps have all the colours of the rainbow—sometimes blended in soft and delicate shades, often in the most startling and violent mixtures. The other reef-living animals—animals like the star-fishes, sea-urchins, sea-cucumbers, crustacea, and shell-fish—which contribute to the lime-supply of the reef, and the various fishes living in the shallows, all are similarly radiantly attired.

Foraminifera are present in abundance, and their calcareous shells form the slimy mass of the white "sandy patches" that intervene between the coral banks.

The Nullipores or coralline seaweeds form encrusting masses over the reefs rather like Madrepores, but growing to a height considerably above the Madrepore localities. Those on the surface are chiefly pink or lilac, and so they give a characteristic appearance to the reefs they infest. Other Nullipores (*Halimeda*) grow in deep pools or on the sea-bottom, and are brilliantly grass-green.

Everywhere on the reef sea-anemones are found in abundance. Most striking are some giants belonging to the genus *Discosoma*. Some of these had discs of between one and two feet in diameter and acted the part of hosts to two species of fish and a species of prawn. In *Discosoma kenti* there were almost invariably one or two specimens of a percoid fish about three inches long. When a stick was thrust into the mouth of the anemone the fish swam out, but when the disturbance was over returned to the seclusion of the anemone's stomach. A prawn about two inches long had similar quarters. Both the fishes and the prawns were brilliantly coloured.

The tentacles of many of the reef-living anemones were conspicuous for length and branchings like the fronds of a fern. Many of the anemones, especially those with peculiar tentacles, have the power of stinging.

Among the corals themselves almost every variety was found, and Mr. Saville Kent has the materials for a large addition to recorded species and genera; but most interesting are his numerous descriptions and figures of the appearance and colours of the living expanded polyps. By an ingenious arrangement of his camera he took many photographs of the expanded corals either in their normal

positions in shallow parts, or when they had expanded in his tanks and tubs.

Among the Alcyonarians, the Alcyonidæ, from their number and abundance, are very conspicuous on the living reefs; but as their most abundant forms possess a flexible corallum with separated spicules of lime, they break up on death and add only to the general powdery *débris*.

Tubipora, the organ-pipe coral, is present for the most part not in large patches, but scattered about, further inshore than ordinary corals, and in regions where there is much muddy sediment. *Heliopora cærulea* was found in abundance, and while the author now confirms Moseley's reference of the Heliopores to the Alcyonarians, he is inclined to believe that a part of the corallum is built up by commensal worms closely allied to the *Leucodore ciliata*, which did so much to destroy the Australian oyster-fisheries. The account in the book, however, would be equally consistent with a purely parasitic relation of the worm to the coral.

A new sheaf-shaped Alcyonarian, called *Xenia pulsitans* by the author, and found in Torres Strait, is remarkable not only for the size and delicate colouration of its polyps, but for certain special physiological manifestations. "The expanded tentacles in this type measure over an inch in diameter. The colour of the stalks and of the main shafts of the tentacles is a pale beryl-green, while the conspicuous tentacular pinnæ, and the substance of the common supporting polypary, are a pale ochreous-brown. The special physiological phenomenon observed of this type was associated with the movements of its tentacles. In all ordinary polyps, whether belonging to the coral-secreting or skeletonless sections, the component tentacles move quite independently of one another, and their action is either irregularly vermiculate, or one of simple expansion and retraction. In the present type, on the contrary, all the eight tentacles move synchronously, opening out and contracting in a continuous measured rhythm, averaging two seconds to each contraction. The action thus observed was in all respects identical with the pulsating contractions of a jelly-fish, and was suggestive of a less remote affinity between the Alcyonarian tribe and the medusiform Hydrozoa than subsists between the last-named tribe and the coral-forming Madreporaria or skeletonless Actinozoa. This suggestion of affinity receives substantial support from the fact that as the radial processes throughout the jelly-fish tribe are invariably a multiple of four, and most commonly eight, they thus correspond in number with the tentacular organs of an Alcyonarian."

Among the Hydrozoa the only generic type that contributes to any material extent to the formation of the reef is that of Millepora. The brilliant *Distichopora coccinea*, which resembles *Corallium rubrum* except that its skeleton is commercially valueless, is found not infrequently.

The pearl and pearl-shell fisheries are of great importance. As an export from Queensland, pearl shell has ranked for the last five or six years as the sixth or eighth most important substance. These fisheries are confined to the tropical area of the Queensland coast, and are intimately associated with the Barrier-reef. The mother-of-pearl shell is collected in water from 7 to 20 fathoms deep. Formerly it was found in areas practically exposed at low tides. Queensland derives a revenue from licenses required to be taken by boats, ships, and sailing masters, and from a small royalty on the amount of shell collected.

Owing to considerable depletion of the more readily accessible fishing grounds, the average weight of shells has decreased. Large shells weighing six to eight pounds a pair are still to be got, but the more ordinary yield averages two-and-a-half pounds the pair. A protecting Act of Parliament now prohibits the taking of shell with a less diameter than six inches across the pearl lining or "nacre."

The specific form of the Queensland waters is *Meleagrina margaretifera*. Actual pearls of large size and fine quality are found, but enormous numbers of the shells contain worthless forms.

Some experiments have been made in the artificial production of pearls, not according to the Chinese methods by introducing metallic or other foreign bodies into the mouth to be covered by a pearly coat, but the process has as yet only a speculative value.

The author made a series of experiments with a view of finding methods of removing young shells from the breeding grounds to shallow water where they might grow to maturity in undisturbed security. He found that young living examples with a diameter of no more than a quarter of an inch might be safely removed by cutting through the byssus. When the animal was placed in an aquarium the remains of the byssus were ejected and a new byssus secreted, and in most cases a secure reattachment formed in the new habitat. The animals have a slight power of locomotion, but nothing comparable with the active movements of pectens.

In removing larger shells from deep water for relaying in shallow water, several methods were tried. Some were placed in shady places on deck and had sea-water thrown over them at intervals. For others, the American method (employed in the case of oysters)—muzzling the shells with wire to prevent the opening of the valves—failed to secure the retention of the fluids in the shell, as they escaped by the byssus groove. The mortality in these two cases was very great, but entirely successful results were obtained when the shells were kept in tubs of water, the water of which was changed every few hours, and the actual shells immersed in basket-work frames in the sea at night.

When these shells were laid down in frames in shallow water where the water was pure and the currents strong, remarkably healthy and active growth took place. It may indeed be said that

Mr. Saville Kent has succeeded in demonstrating the practicability of pearl-shell farming.

An interesting and curious chapter deals with the Bêche-de-mer or sea-cucumber fisheries. The food of the sea-cucumbers consists almost entirely of foraminifera which are swallowed in combination with large quantities of shell and coral sand. So physiologically the sea-cucumbers are the earth-worms of the sea, and pass through their bodies immense quantities of material in process of getting the scanty food substances contained in it.

The animals are prepared entirely for the Chinese market. They are collected by hand in sacks at low water. Immediately on their arrival at the curing stations they are placed in large cauldrons of water and boiled for twenty minutes. They are then split open, gutted, partially dried, and finally smoked, the favourite wood being that of the red mangrove (*Rhizophora mucronata*). The prepared animals should be perfectly dry and crisp. Mr. Saville Kent does not, unfortunately, give further directions in the matter, though one would have liked to know the recipe for their final cooking, and any commendation or otherwise of their flavour.

Little seems to be known of the rate of growth of these Holothurians. A good many forms are present. The cotton-spinners have no commercial value; many others from the soft texture of their tissues cannot be cured. The author sent a complete set of specimens to the British Museum, where they have been described by Professor Jeffrey Bell. *Holothuria mammifera*, called by the appetising Chinese name Se-ok-sum, appears from its market price to be the surest tickler of the Celestial palate, but *Actinopyga obesa*, under the name of Hung-hur, runs it close.

While the Bêche-de-mer fisheries have yielded an average of £23,000 a year, the Queensland oyster fisheries produce only about £8,000 a year. The oyster is *Ostrea glomerata*, the "rock oyster." Other forms are quite edible and wholesome, but have not yet secured a market. The oysters occur in the tropical waters, and so come into the Barrier Reef fauna, but the actual fisheries are, for the most part, south of the reef. *Ostrea glomerata* has a number of well-marked varieties. The shallow-water forms are the most typical, and have luxuriantly frilled and convoluted marginal borders, and bright colours. The deep-water varieties have a smoother and more ponderous form, often an abnormally elongated contour, and are much less conspicuously coloured.

The old travellers' tales are realised, and in the region of the Barrier Reef oysters are to be found growing on trees. That is to say, one of the most favoured habitats of the "rock oyster" is the exposed roots and respiratory shoots of the white mangrove (*Avicennia officinalis*.)

The oyster has many enemies. A small boring whelk (*Urosalpinix faviæ*) does immense damage, especially among the young oysters. Mr. Saville Kent seems doubtful as to whether or no the star-fish

deserves its evil reputation in this matter, but until its innocence be established he recommends its wholesale destruction.

The common sting-ray, *Trygon pastinaca*, is most destructive, and *Cestracion philippi*, the Port Jackson shark, makes use of its crushing teeth to very evil effect among the oysters. Boring sponges, birds, and various other forms of life prey on the oyster, but the most destructive pest is the small worm, *Leucodore ciliata*, whose ravages were first described by Mr. Whitelegge.

In a special chapter, entitled "Food and Fancy Fishes," the author deals with some of the 900 recorded species of Queensland fish. Among food-fishes, those of the Northern district belong to the Indo-pacific or Oriental region. The trumpeters and barracutas, which are so important in the fish-markets of New Zealand, are practically unrepresented in Queensland. The best known forms are *Lates calcarifer* (the "Cockup" of the Calcutta market), and the smaller *Lates colonorum*. Mullets, gurnards, sea-pikes, flat-fish, and many well-known forms abound. Eight species of herring (*Clupea*) have been recorded, but, as yet, have not been utilised for commercial purposes. Altogether, the "unvintageable sea" seems to be remarkably prolific, and Mr. Saville Kent makes out a good case for regarding the Barrier Reef as one of the most remarkable commercial advantages of Queensland. In a concluding chapter entitled "Potentialities," he points out how, under proper scientific advice, this commercial advantage admits of almost indefinite extension. As the population of Australia increases, the value of so large an area of prolific sea-life will become almost inestimable, and, quite apart from the scientific value of Mr. Saville Kent's beautiful volume, Queensland is to be congratulated on taking so efficient means as the assistance of this work to develop its resources. On the side of practical utility it were hard to see how Mr. Saville Kent could have done better work. He has surveyed the actual resources of the reef and has shown the small use that is at present made of them, bringing to the task not only a trained scientific mind, but a large special experience.

To those who spend their days in laboratories and museums, and who will eagerly turn to the separate scientific accounts that have been, and will be, published on Mr. Saville Kent's collections, this work is equally invaluable; for here are the animals, not as specimens with names new or old, hinging on some obscure point in anatomy or doubtful question of priority, but the animals alive, in their actual places in the world, in their crowded environment of friend and foe, of sunlight and waters. In this short sketch of the book very little has been said of its most valuable scientific feature—the faithful and minute account it gives of the actual appearances presented by a living coral reef; but as the value of this account is inseparably associated with the beautiful photographic plates, those interested must be referred to the book itself.

SOME NEW BOOKS.

THE GERM-PLASM: A Theory of Heredity. By August Weismann. Translated by W. Newton Parker, Ph.D., and Harriet Rönnfeldt, B.Sc. Contemporary Science Series. Pp. 477. With 24 illustrations. London: Walter Scott, 1893. Price 6s.

THE new volume of Professor Weismann, of which the translation is before us, departs from the historical method in which we have received his work. This is a deliberate presentation of his theory, incorporating, expanding, and explaining the ideas which have been shaped in his various essays. At the same time, it contains a number of novel hypotheses subsidiary to the theory, but necessary to it. It is not too little to say that the book is an excessively hard nut to crack, and that many will declare the kernel bitter who have not bitten through the epicarp.

Speaking generally, this expanded account is a closer and more detailed theory of the structure and mechanism of the germ-plasm, bringing it closer into relation with, on the one hand, recent advances in actual observation of the structures and changes in the nucleus connected with cell-division, and on the other, with the observed details of inheritance, variation, asexual reproduction and regeneration of tissues.

As before, the doctrine of the continuity of the germ-plasm is firmly insisted on, and it may be said that this, of all parts of Professor Weismann's theory, has been subjected to the least successful criticism, and has received the most startling corroboration. This germ-plasma Weismann identifies with the chromatin fibril in the nucleus of egg-cells and sperm-cells. Before fertilisation, the nuclear fibril breaks up into a series of loops which Weismann calls idants. In ordinary sexual eggs the number of idants is twice halved, and by what Weismann calls these two reducing divisions the polar bodies are extended. It will be remembered that, originally, the first polar body was considered by Weismann to serve for the extrusion of that part of the nuclear matter which, having served to guide the maturation of the ovum, became useless when the ovum was mature. In his completed doctrine, he finds in the two polar bodies of normal sexual cells, and in the single polar body of parthenogenetic ova, a mechanism for reducing the bulk of germ-plasm—part of the apparatus for phylogenetic variation. The actual nuclear matter which controlled the maturation of the ovum he supposes to have passed into the protoplasm of the ovum, and, therefore, to be out of the reckoning when the behaviour of the nucleus is being interpreted.

When the nuclear loops or idants are closely examined they exhibit a series of lumps or divisions as if they were built up of separate pieces like draught counters strung together. These sepa-

rate pieces Weismann names "*ids*," and suggests that the facts of inheritance might be accounted for if each id contained the possibility of producing the individual. On such a hypothesis Weismann shows most ingeniously and convincingly how the observed marshalling and sorting, division and rejection of idants and ids would form a mechanical apparatus compatible with the complex phenomena of inheritance to be accounted for. The next stage of his theory still is along the path of observation. A certain number of idants with the contained ids remain unused in each individual development. Occasionally, but rarely, they are passed at once into a cell which becomes the future mother sperm or mother ovum cell, and so the sperm-cells and egg-cells of the individual arise directly from the idants present in the fertilised egg-cell. More frequently, however, the set of idants and ids are handed on passively from cell to cell during the development of the individual, until they ultimately reach the place in the individual where the sexual cells are to be formed. This path of the germ-plasm he calls the "germ-track," and, as will be remembered, it was his original hitting upon such germ-tracks in the Hydrozoa that led him to suspect the existence of a germ-plasm. Turning again to that part of the germ-plasm which is to form the actual individual, we come to entirely theoretical matter. It consists of *ids*, and each *id* contains the possibility of a complete individual. The *id* is not a structureless substance, but possesses a definite historical architecture—the expression of the past history of the species. Each *id* is built up of smaller units, the "determinants;" and there is a "determinant" for each cell or group of cells in the animal or plant body which can vary independently. As development goes on the *ids* gradually break down, throwing off in the order determined by their historical architecture the determinants to rule the structure of the "determinates" or independently varying parts of the organism. Thus ultimately we come to determinants passed during cell-division into their appropriate cells. In these cells the determinants break down into the final units or "biophors" which pass through the nuclear membrane into the protoplasm of the cell. Thus the "idioplasm" or chromatin derivative which rules the cell consists of Weismann's theoretical determinants, and it rules the cell by the passage of actual material particles into the protoplasm. These biophors—the ultimate units of the germ-plasm—are, at the same time, the primitive units of life, and Weismann conceives life as having originally consisted of independent biophors.

For each of the units of the germ-plasm Weismann postulates the common property of living material, the power of growth and reproduction by division. This has of course been observed in "idants" and is a ready inference for "ids."

It is to be noted that a number of ids, each with the complete power of producing an individual, are present in the development of an individual. Thus there comes about the struggle between the determinants and biophors coming from ids with slightly different peculiarities, and thus the particulate variability of the parts of an organism and the particulate resemblances to parents and ancestors have a possible material apparatus.

As the whole volume consists of a subtle and ingenious translation of biological phenomena into the terms of this theoretical apparatus, it is obvious that only a study of the book can give an idea of the exactitude with which the phenomena correspond with the apparatus; and for this reason a sketch like the above removed from the com-

plexity of the observed phenomena of which it is the abstraction necessarily will seem artificial. To those who are inclined as they read these lines to cry out on the artificiality, I can only say—work through Weismann's application of his theory and see what a master key it seems to the confused tangle of life.

While at first sight this theory seems to present only a formal simplification of the facts of heredity, inasmuch as it seems to put on the biophor the whole mystery of life, I am inclined to think that this objection is not valid; for if the mechanical marshallings and evolutions and devolutions of the biophor and its higher aggregations do actually form a picture in little of the larger complexities of the visible world, we can at once remove from the problems to be solved these actual complexities. We shall, in fact, by Weismann's help, effect a separation of those problems of living material which are functions of the complexity of organisms and not actual difficulties of living matter itself; and those who are familiar with the recent advance in our knowledge of the elaborate marshallings and evolutions and devolutions of the chromatin in dividing nuclei, can hardly say that Weismann's postulated movements of the elements of the germ-plasm make a large demand on credulity; but this depends upon our being able to see clearly that the mechanism is a possible one. I freely admit that as yet I have not assimilated Weismann's book sufficiently to follow the clear image that is obviously present in his mind. He expressly states that he has felt the difficulties in giving up an epigenetic view of ontogeny.

It will be seen that, leaving out the foregoing considerations, we are led up to the ultimate problem in the biophor. That is really the problem of assimilation, and is the factor as yet unexplained in any theory of biology. The possible migration of biophors from the nucleus to the protoplasm usefully shifts the problem of "fernwirkung" (the translator rather unhappily renders this "emitted influence," instead of the obvious "action at a distance") to the protoplasm itself. When we understand how protoplasm can feed and grow, we shall probably have little trouble in understanding the nucleus.

P. C. M.

THE MICROSCOPE: Its Construction and Management. Including Technique, Photo-Micrography, and the Past and Future of the Microscope. By Dr. Henri Van Heurck. English Edition. Translated by Wynne E. Baxter, F.R.M.S., F.G.S. With plates, and upwards of 250 illustrations. London and New York: Crosby Lockwood & Son, 1893.

THE English microscopist has, for at least a generation, shown that he is sufficiently in earnest to be cosmopolitan. He can welcome warmly good English work, in either the manufacture or the use of the microscope, or in the excellence of a treatise on its principles and application. But there is definite evidence that he has learned to prefer the quality of an instrument or a handbook to its source. In fact, there is danger that the foreign maker, having obtained by sheer excellence and moderate prices an assured position in this country as the producer of the principal optical elements of the microscope, that the prejudice runs wholly in favour of instruments of foreign production, to the sacrifice of much that is of really higher excellence made in this country.

In the matter of handbooks, however, there is probably no country that has met the wants of the beginner and the skilled amateur so fully as England. Carpenter's "Revelations of the Microscope" has, in each of its seven editions, held its own not only in England and America, but has been largely used on the Continent. Nevertheless, and, perhaps, indeed for this very reason, there is no country in the world where the great Diffraction theory of Abbe, explaining the principles of vision with the modern microscope, has been so critically and, at the same time, so warmly and generally received as in England. At the same time, there are few working microscopists whose shelves are without *Handbuch der Allgemeinen Mikroskopie* by Dr. Leopold Dippel; and it may be safely predicted that the splendid *Theorie der Optischen Instrumente nach Abbe* by Dr. Siegfried Czapski, which has just come from the publishers' hands in Breslau, will anywhere find a more appreciative circle of students than in England; but the English microscopist, who is not only a student of what the microscope does and can reveal, but also a critic and connoisseur of the principles and modes of manufacture of the instrument, really needs books of the class above indicated. The day for handbooks that are better illustrations of the art of the printer and the wood engraver than of the science and art of microscopy has passed away.

In the treatise now before us great pains has been taken to present in its most attractive form the facts of microscopical science as known to us ten or fifteen years ago, and to make an appendage to these of the most recent knowledge which has come to us as the result of theory and practice in more recent times; but between the old and the new there is no coherence. The treatise purports to cover all the area of modern microscopy, in the interests and for the benefit of the amateur. That it fails of its purpose there can be no doubt. It does this partly because of its method. It adopts the older style of teaching, and simply forces the new optical doctrines and details of the new methods of manufacture and manipulation (where given at all) into a place or position in the text without preface or explanation. The result is that we obtain an account of the "Theory of Microscopic Vision" which is in itself excellent, but quite beyond the range of the class of reader for whom the book is ostensibly written, because, clear as Abbe is in the exposition of his great theory, it needs at least a chapter of explanation and introduction to make it fully accessible to the ordinary, and especially the non-mathematical, reader. The result is, that this book appears to us to fail from the fact that it directs the student to go beyond his depth, without even the semblance of assistance on the one hand, and, on the other, guides him with great care over shallows where no guide was needed, save the catalogues of certain English and foreign opticians—that is, as to the forms of certain instruments mostly on the same type.

It is quite true that the tyro wants above all things to know how wisely to purchase an instrument; but this is not unfolded to him by an indiscriminating description of the microscopes of certain makers however diverse, but by an unbiased description of the *essentials* of *any* good instrument, and such an account of practical tests as would enable a beginner at least to see how far a certain pattern of stand, or a specific instance of workmanship, answers to this set of requirements. And in furnishing these data there need be no more dogmatism than in an experienced photographer's affirmation

of what is indispensable in a thoroughly good and practical camera—always in both cases keeping the specific object which is sought in view.

It is true that Dr. Van Heurck has presented us with a very attractive model of a microscope for "the study and photography of Diatoms, and all delicate researches," but the model has not many new or specially advantageous features when considered as one of the many English models before the public. It has, as we think, in common with some of the very finest English workers, some distinct disadvantages; but it certainly overcomes the difficulty of the working of the fine adjustment on good conditions better than any instrument that we know of the Jackson model (which is in practice an inferior form) that adopts similar methods. The fine adjustment is delicate, and the instrument is throughout supplied with admirable facilities for accuracy and ease in use. Hundreds of skilled workers would refuse it at once as representing the best possible form of a modern microscope; but much has been done to soften the evils inherent in the form. Yet all these are costly, and when they are effected we are bound to submit that it is not the best form of the instrument that is either possible or accessible; and, above all, it is not the best form of the instrument that can be constructed at a low expenditure.

The amateur wants a useful, well-made instrument at a low cost. So does the medical student. What, therefore, is essential to a good microscope in any form? What form of stand has the largest number of points in its favour, practically considered? and which are the stands of the modern makers, English, Continental, or American, that, at a moderate cost, most largely meet these requirements?—these are the considerations which are of the greatest value to the purchasers of modern handbooks, from this side of the subject, and its efficient discussion would involve a consideration of that most practical of all questions in such a treatise, the relative values of the English as against the Continental stands, or *vice versâ*; but this is wholly avoided.

There is one other point which we desire to touch but lightly, it is that here and there in the book phraseology has been adopted that certainly does not represent, if it does not absolutely run counter to, the diffraction theory of microscopic vision. It is not necessary to dwell on this; probably it may in some instances be inadvertence; but in a treatise intended to teach *ab initio* the doctrines of Abbe on the optics of the modern microscope, especially as there is some obscurity in the inculcation of these, it is unfortunate to find incidental passages that imply not only complications but contradictions.

The translator's work has manifestly been done with care and sincerity, and most fairly presents the meaning of the author; and the book, though large and heavy for use at the work-table, is unusually good.

There are many points of excellence in the work, and some that give evidence of the practical skill of the author; indeed we can see the possibility in a second and greatly revised edition of a book of high quality and usefulness; but it will involve the excision of much that is now useless in its pages, and the rendering useful by ample and lucid preface and explanation of much in it that, although useful in itself, is doubtfully, if at all, useful in the form and relations in which it is presented to the enquirer in the book as it now stands.

THE GLACIAL NIGHTMARE AND THE FLOOD. A second appeal to common sense from the extravagance of some recent geology. By Sir Henry H. Howorth, K.C.I.E., M.P., F.G.S. 2 vols. 8vo. Pp. xxvii and 920. London: Sampson Low & Co., 1893. Price 30s.

SIR HENRY HOWORTH, in the two volumes which now lie before us, stands forth as the champion of an almost universal deluge. It is not, he is careful to explain, the deluge of Noah; but it appears to be a considerably worse one, which exterminated the mammoth and could transport erratic blocks as large as houses across country for distances of many leagues. In fact the book is a nine-hundred page supplement to his bulky volume on the Mammoth and the Flood, and is to be followed by still another one. It is not altogether easy to understand the reason of the appearance of this mass of undigested extracts. The new book seems to be intended as a sort of homeopathic remedy for the "Glacial Nightmare"; but, unlike most homeopaths, the author, instead of using a small dose, attempts to cure by the exhibition of a far more serious incubus than the one we are suffering from.

The method adopted in these volumes is to put together under several heads extracts from various opinions as to the mode of formation of the Drift or Diluvium. We thus find in the first three chapters quotations from writers between 1719 and 1840, who referred the transportation of erratic blocks to the agency of water. Then follow chapters on the champions of icebergs, and of glaciers, and two more on the "growth and culmination of the glacial nightmare." We read next a chapter on the "alleged recurrence of glacial epochs and on supposed inter-glacial beds," followed by others entitled "appeals to transcendental physics and astronomy" and "meteorology." Chapters XI. to XVII. represent, apparently, the views in favour with the author, for they contain selected extracts from the evidence of such witnesses as could testify in any degree against recurrent ice ages, against a glacial period in the southern hemisphere, against the power of ice to do anything in particular, and against the occurrence at any period in regions now temperate of accumulations of ice other than large glaciers. Chapter XVIII., the author's own, is entitled "The distribution of the drift can only be explained by invoking a great diluvial catastrophe."

Sir Henry Howorth's volumes will be useful as giving references to old and forgotten writers on diluvial theories, and we have ourselves noticed several that were new to us. The more modern portions, and those relating to glaciation, are most imperfect. The Scandinavian writers who have done so much to increase our knowledge, are, for instance, almost ignored, or only quoted at second-hand from the "The Great Ice Age"; even the English and American literature of recent years has not been properly examined. After going through the two volumes carefully, and noting how time after time the author brings forward untrustworthy witnesses in favour of his own case, and omits to call the strongest on the other side, we cannot recommend the "Glacial Nightmare" to the student as giving either an accurate or impartial summary of the present state of our knowledge. We observe that the volumes are marred also by an enormous number of errors and misprints. Only ninety of these are corrected in the author's own "table of errata," but ten of the corrections are themselves wrong! Where quotations have had their punctuation or wording altered, inverted commas should not be used; and we still less like to see them inclosing inaccurate translations

from foreign memoirs. We do not intend to imply that there is any wilful distortion of the quotations, for the errors are just as numerous in those in favour of Sir Henry Howorth's own views as in those from opponents. It is merely another instance of the carelessness so conspicuous throughout the book.

GUN AND CAMERA IN SOUTHERN AFRICA: A Year of Wanderings in Bechuanaland, the Kalahari Desert, and the Lake River Country, Ngamiland. With notes on Colonisation, Natives, Natural History, and Sport. By H. A. Bryden. 8vo. Pp. xiv. and 544. Illustrated. London: Stanford, 1893. Price 15s.

SOUTH and East Africa being at the present time on the "boom," the reading public is almost overwhelmed with the number of works appearing in rapid succession on that country, its peoples, and its products. Most of these, unfortunately, have little interest for the

Head of Lechée Antelope (*Cobus lechée*)

naturalist, who too often cannot but regret that the lack of suitable training has rendered so many of the pioneers of civilisation unfitted for giving any account of the animals with which they meet. This, however, is not the case with Mr. H. A. Bryden, who has already made himself known as an observer of wild animals in their haunts, in his work "Kloof and Karroo in Cape Colony," as well as in various articles contributed to the *Field* and *Land and Water*. The present volume naturally contains a large amount of matter which is chiefly interesting to those desirous of obtaining information as to the nature of the country and its development; but there are several chapters which cannot fail to be profitable reading to the zoologist. Among these we may especially mention those bearing the titles "Natural History Notes," "The Giraffe at Home," "The Waterway and Water-fowl of the Botletli," "The Game Birds of Bechuanaland," and the "Present

Distribution of the Large Game of Bechuanaland, Ngamiland, and the Kalahari."

In the Livingstonian epoch, every book on Southern Central Africa contained accounts either of new mammals or of the countless swarms in which species previously known had been met with. Alas! these days are over; and Mr. Bryden's account of the mammals of the districts he visited is, to a great extent, one continued lament on their decadence or disappearance. Thus he speaks of hartebeest surviving only on one protected farm, and records the disappearance of the last brindled wildebeest from the districts he visited; while he confirms the general opinion as to the total extinction of the quagga. Perhaps the most interesting chapter in the whole book is that relating to the giraffe, whose last haunts in Southern Africa are the thirsty regions of the northern Kalahari and Khama's country. The author dwells on the imperfect idea we obtain of this magnificent ruminant from the dwarfed and pallid specimens seen in menageries; and urges on our museum authorities how important it is that they should obtain the skin of an old chestnut bull before it is for ever too late. He mentions that in Khama's country the giraffe is only safe during the lifetime of the present chief, but urges that one of the English companies should, when the time arrives, take up its protection. In its last native stronghold, in the northern Kalahari, the author states that there is a prospect of water being obtained (although he does not say how), and if this should be the case, and protection afforded in Khama's country be withheld, then good-bye to the giraffe in Southern Africa. Surely, under these circumstances, our Zoological Society ought to spare no expense in endeavouring to procure a pair of these animals; and as money—if only there be enough—will do most things, it ought to effect this. We may add that Mr. Bryden is convinced that, in the Kalahari, the giraffe, in common with several antelopes, never drinks.

We have not space to refer to the author's observations on other mammals and birds; but we may point to him that if he considers it necessary to add the scientific names of well-known animals—a custom we consider perfectly superfluous in a popular work, and only irritating to the reader—he might take care to use the proper ones. *Oryx capensis*, for instance, is not the correct title for the gemsbok, neither should the secretary-vulture be alluded to as *Sagittarius secretarius*. Then, again, although we are aware that it is rash to say among what strange associates any particular bird may *not* have been placed by modern systematists (!), yet we scarcely think the statement (p. 350) that jacunas "are usually placed by naturalists in the family of Palamedeidæ, or screamers, between the snipes and rails" can be accepted as up to date.

On the whole, however, the book is, for an amateur naturalist, remarkably free from zoological blunders; and while its pleasant style and beautiful illustrations cannot fail to render it attractive to the general reader, it contains many observations on the habits and distribution of South African animals which must give it a considerable amount of value to the zoological student. R. L.

THE NESTS AND EGGS OF BRITISH BIRDS, WHEN AND WHERE TO FIND THEM; being a Handbook of the Oology of the British Islands. By Charles Dixon. 12mo. Pp. xii. and 371. London: Chapman & Hall, 1893. Price 5s.

MR. DIXON is a prolific writer on British birds and their ways. Only recently we had to notice a respectably-sized volume on the Game-

Birds and Water-fowl from his pen, and now we have the smaller work before us. In his preface, the author tells us that the idea of the work occurred to him about a dozen years ago, since which date he has been assiduously collecting material, and, accordingly, with the numerous other works on the subject for his guidance, it ought to be as near perfect as possible. It might be thought that by this time the subject of British birds was well-nigh exhausted, but as Mr. Dixon's work treats solely of those species nesting in the British Islands, and is mainly confined to their nesting habits, it will doubtless fill a void and command a ready sale, more especially as the larger works of Morris and Seebohm, treating more or less specially of eggs and nests, are too expensive for many purses. Like all Mr. Dixon's productions, the work before us is well and pleasantly written, and the amateur naturalist, as well as every young person interested in this fascinating study, cannot do better than forthwith provide him- or herself with a copy.

Of course we miss illustrations, but as figures of eggs are not of much use unless coloured, the cost of plates would have made the book so expensive as to have defeated one of the objects of its production, and the student must accordingly do the best he can without their aid. In regard to classification, the author may be ranged among the "lumpers," seeing that he puts all the non-diving ducks in the genus *Anas*, and employs the genera *Charadrius*, *Totanus*, *Scolopax*, etc., in a wide sense. Considering, however, that there is but an interval of some two months between the date of publication of the present work and the one on Game-Birds, it seems rather a pity that he could not have definitely made up his mind what names he was going to employ, as it is rather puzzling to the beginner to find the Dotterel alluded to in one work as *Charadrius morinellus*, and in the other as *Eudromias morinellus*. Moreover, a little more attention to the index at the end of the volume would have been desirable, seeing that the word "snipe" is omitted therefrom. The mention of snipe reminds us that we consider Mr. Dixon to be wrong in persistently stating that the common species is not gregarious, although we are fully aware that the individuals do not mass together after the manner of ruffs and plovers. No one who has shot in a Bengal "jhil," with snipe rising as thick as flies all round him, can ever possibly think of these birds as being anything else but gregarious.

All these points are, however, but trifling blemishes in a volume which leaves nothing to desire in the way of "get-up," and fully deserves all the success we can wish it. R. L.

THE GEOLOGY AND PALÆONTOLOGY OF QUEENSLAND AND NEW GUINEA, with 68 plates and a geological map of Queensland. By Robert L. Jack, F.G.S., F.R.G.S., Government Geologist for Queensland, and Robert Etheridge, Junior, Government Palæontologist (New South Wales). 4to, Pp. xxx. and 768. Brisbane: James Charles Beal. London: Dulau & Co., 1892. Price £2 2s.

IN Europe we scarcely realise how much good scientific work is being done in our Australian colonies. Geology in particular is well studied, for the prosperity of these colonies is so largely dependent on their mineral resources, and it is so essential in an arid region to understand where water can be obtained by artesian borings, that considerable sums are willingly devoted to the making of careful

geological surveys. Queensland, though behind certain of the other colonies, has now made the very handsome contribution to geological literature which lies before us, published under the authority of the Minister of Mines.

Mr. Jack, after ten years' experience on the Geological Survey of Scotland, was appointed Geologist for Northern Queensland in 1877, and since that time has been unravelling the geology of the country and studying its minerals and mines. A number of isolated reports have already been published by himself and his assistants, and materials for the present work have been accumulating for the last fifteen years. In 1881 Messrs. Jack and Etheridge, Junr., determined to combine their labours, and published an Australian Geological Bibliography. Then followed a handbook explanatory of the exhibits in the Colonial Exhibition of 1886, which in some measure led up to the volumes which have just appeared.

So great a mass of detail and so many subjects are treated of that it is impossible critically to review the book, and we can only give an outline of its contents. Speaking generally, for the stratigraphical and mining sections, Mr. R. L. Jack is responsible, while Mr. R. Etheridge, Junr., has undertaken the palæontology. The method of arrangement adopted is mainly stratigraphical, each series of rocks, beginning with the oldest, occupying a separate section. The minerals and fossils are treated of according to the age of the deposits that contain them, instead of being relegated to separate appendices, as is perhaps more usual. In many respects this arrangement is the most convenient, but it is not altogether satisfactory, for in the absence of any subject index it is very difficult to find the references to particular minerals, unless one already knows the age of the formations in which they occur. Persons, places, and fossils are well indexed.

The oldest rocks in which fossils have yet been found in Queensland are the Middle Devonian, though below these occur various slates and schists of unknown age. Then follow unconformably the Permo-Carboniferous, Trias-Jura, and Upper and Lower Cretaceous. Between the Upper Cretaceous and the Lower Volcanic and Drifts, here doubtfully classed as Miocene, there is a wide gap, and the real age of the Tertiary rocks of Queensland is by no means settled, for fossil evidence is absent. The curious deposit of auriferous sinter at the celebrated Mount Morgan Gold Mine is considered by Mr. Jack to be of Tertiary date, though the evidence is not conclusive.

The section on Post-Tertiary rocks will be one of the most interesting to the European geologist, for in these rocks occur the remains of the wonderful extinct marsupials of Australia, and of numerous extinct birds. Discussing the question of glaciation in Australia, Mr. Jack writes: "No evidence of a Post-Tertiary Glacial Period has ever, so far as I am aware, been met with in Queensland, unless the presence of temperate plants on some of our tropical mountains be taken to afford the necessary proof." Further on, however, he mentions a recent visit to the celebrated glaciated rocks near Adelaide, in company with Professor Tate. Mr. Jack, after ten years' study of glaciation in Scotland, is a far better authority than most of the geologists who have discussed the matter, often without going to the spot, and we are interested to learn that he "came to the conclusion that Professor Tate's observation was correct in every particular, and, in addition, satisfied [himself] that the movement of the ice must have been from south to north."

The morainic *débris* has travelled from a spot forty-five miles to the south.

We have already remarked on the great importance to Queensland of its mineral wealth, and readers will not be surprised to find that a large part of this monograph relates to mines and mining. Indeed, we are somewhat surprised to see that the Government of a young country like Queensland has been so far-seeing as to understand the necessity of studying also the scientific aspects of the subject, and has gone to the expense of publishing so good a series of plates of fossils. This is as it should be, for even looked at from a purely economic point of view, it is most essential that the relations of the different deposits should be thoroughly understood, and their fossils ascertained, for the suitability of large areas for habitation depends mainly on the supply of water from artesian wells; other barren districts may be made profitable by the discovery of new mineral resources. One has, of course, no right to expect completeness at this early stage, and we think that Mr. Jack shows a thorough appreciation of the necessities of the case when he writes in his preface that "The highest function of a Geological Survey is to lay a basis for future scientific observations by accurately mapping the relations of the various formations met with in a given district." He "cannot say that this beau ideal has been reached in Queensland. In every country, and especially in every new country, it becomes necessary in the first place to give attention to districts remarkable for the presence or prospects of mineral deposits."

LES ALPES FRANÇAISES: la flore et la faune, le rôle de l'homme dans les Alpes, la transhumance. By A. Falsan. 8vo. Pp. viii. and 356. With 77 figures in the text. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

WE have already noticed the first part of M. Falsan's work on the French Alps, namely, that dealing with the mountains, streams, glaciers, and meteorology, or the inorganic phenomena. The second part, now before us, treats of the animal and plant-life prevalent to-day or in past ages, as well as the part played by man. M. Falsan has been fortunate in securing efficient helpers for the special sections of his book. Thus, the Marquis de Saporta is responsible for the Palæobotany, which forms the subject of the second chapter, while, at the end of Chapter I., a general introduction by the author to the ancient Alpine flora, and its relation to the present one, the Marquis and M. Marion explain their theory "sur l'origine montagnard de la flore des Alpes."

Dr. Magnin follows, in Chapter III., with a succinct account of the vegetation of to-day, discussing, first, the influence of altitudes and the consequent modifications of species; secondly, the zones of vegetation, of which he makes four, viz.: (1) The Préalpes, or western outer Alps; (2) the granitic central Alps; (3) the south-west Alps; (4) the maritime Alps. In each case are mentioned the plants especially characteristic of the zones, and their geographical or altitudinal subdivisions. Next he briefly refers to the influence of the aspect, whether north or south, S.E. or N.W., and the nature of the soil as regards the marked difference between the vegetation of the silicious and calcareous districts, and finally discusses its relation with neighbouring regions, the central and eastern Alps, the central plain and the Pyrenees.

M. Falsan himself deals with the stratigraphical palæontology and the ancient fauna in Chapter IV., and in the succeeding chapter with the modern vertebrate zoology; while Chapter VI., devoted to the insects and molluscs, is the work of MM. C. Rey, C. Chantre, Falsan, and Locard. The Marquis de Saporta, in the next chapter, tells of the appearance of man in the Alps, his origin, and early immigrations, and traces his history upwards through the various ages to historic times, and finally, in the last, with the aid of M. Charles de Ribbe, gives an interesting historic account of his yearly transmigration with his flocks and herds, its disastrous effects, the efforts to combat the evil, dating from the Middle Ages, and the results of modern legislation. The illustrations are generally good, some of the fishes (by Leblanc) extremely so, and the publishers are to be congratulated on this valuable addition to their Contemporary Science Library.

THE FUTURE OF BRITISH AGRICULTURE; How farmers may best be benefited. By Professor Sheldon. Small 8vo. Pp. 158. London: W. H. Allen & Co., 1893. Price 2s. 6d.

In a series of eight chapters the author discusses some points of very vital interest to the farmer and the nation at large. The answer to the query "Will grain raising pay?" depends on whether the farmer can form a sufficiently powerful and intelligent combination to secure a fair arrangement in the relation of landlord and tenant. Protection "and that new economic craze 'Bi-metallism'" are ruled out of court. To place a duty on food imports would merely subsidise farming interests to the detriment of the rest of our industries. If we protect any we must protect all, and then we shall be no more forward. Once we were a wheat-exporting people, but since 1868 the acreage of wheat growth has fallen from nearly 4 millions to less than 2¾ millions in 1892. Canada, with its scores of millions of acres in the north-west and a favourable climate, will be the chief wheat-exporting country of the future, while mixed farming, stock, grass, grain, root, and green crops will be our mainstay.

One chapter is devoted to "the Beef of the future," another to "our breeds of sheep and how to mend them," and three to dairy farming, all of which contain valuable hints. Professor Sheldon speaks with authority, his book is well written, and promises to bring those who will read it more in touch with the agriculturist, his ways, and his difficulties.

CATALOGUE OF THE SNAKES IN THE BRITISH MUSEUM (NATURAL HISTORY). Vol. I. By G. A. Boulenger. London: Trustees of the British Museum, 1893. Price £1 1s.

THE first volume of this Catalogue comprises the families of Typhlopidæ, Glauconiidæ, Boidæ, Ilysiidæ, Uropeltidæ, Xenopeltidæ, and Colubridæ Aglyphæ (part), and contains descriptions of 523 species. The classification adopted is nearly similar to that formulated by Mr. Boulenger in his earlier work on the Reptilia and Batrachia of British India (1890), only some slight changes having been made in the arrangement of the genera. The Boidæ, and more especially the pythons, are regarded as the most primitive snakes; and the unnatural character of the old classification of the Ophidia into the

harmless and poisonous groups is particularly emphasised. A figure of the skull, with all the bones lettered, accompanies the definition of each family, and there are twenty-eight plates devoted to external form.

Au Bord de la Mer. By E. Trouessart. [Bibliothèque Scientifique Contemporaine.] 16mo. Pp. 344, figs. 149. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

This is a handbook to the Geology, Fauna, and Flora of the coast of France from Dunkerque to Biarritz, and will be of considerable service to those who wander along these shores. The volume begins with a sketch of the geology, special reference being made to the cliffs, and, after touching on the littoral zones, proceeds to discuss in order, from the Algæ onwards, the life to be met with in the waters. It should form part of the outfit not only of naturalists, but also of those who desire a casual acquaintance of the animals and plants they meet with during a seaside holiday.

An Analytical Index to the Works of the late John Gould, F.R.S. With a Biographical Memoir and Portrait. By R. Bowdler Sharpe, LL.D. 4to. Pp. xlviii., 375. London: Henry Sotheran & Co., 1893. Price £1 16s. net.

This is a valuable index containing nearly seventeen thousand carefully verified references to the genera and species of birds and mammals described in the works and memoirs of Mr. Gould. Both the scientific and the English names are quoted, and a number of extra synonyms are added from the more recent monographs of other authors, which in a few years will have familiarised naturalists with a certain set of names not occurring in Gould's works, though the species may be duly figured therein. The Biographical Memoir is accompanied by an excellent photographic portrait, while the Index is prefaced by a detailed list of Mr. Gould's 18 folio works and 300 small papers. We congratulate Dr. Sharpe on the successful completion of his laborious task, for which ornithologists owe him a debt of gratitude. The volume is beautifully printed, and a large-paper edition has also been prepared.

Curtice's Index and Register of Periodical Literature. Weekly. No. 1, vol. i., April 3 to April 8, 1893. Price 6d.

This interesting new Index, which proposes to contain references to important subjects in daily newspapers, weekly, monthly, and quarterly publications, will be an invaluable help to all who are anxious to keep posted up in special subjects. It will largely depend on its fulness for its success, and the request of the promoters in asking for two copies of a journal in exchange cannot be considered unreasonable.

A French translation of the Right Hon. Professor Huxley's "Science and Religion" has been added to Messrs. Baillière's "Bibliothèque Scientifique Contemporaine."

MR. ELLIOT STOCK has just issued a literal transcription of Captain Cook's Journal during his first voyage round the world, made in H.M. Bark "Endeavour," 1768–71. This is the first occasion on which the complete original MS. has been published, and it is edited, with notes and introduction, by Captain W. J. L. Wharton.

MESSRS. CHAPMAN & HALL have published a new edition of Rev. H. N. Hutchinson's "Extinct Monsters." A few of the restorations by Mr. Smit have been re-drawn and improved, and a large number of woodcuts have been added to the text.

THE Dorset Natural History and Antiquarian Field Club has issued its thirteenth volume (dated 1892). The chief things in this portly "Proceedings" (244 pp.) concerning Natural Science are, a paper by the president, Mr. Mansel-Pleydell, on the occurrence of *Lamprothamnus alopecuroides*, Braun, a Chara for which only one other British locality is known; a paper on "Some Monstrosities of *Littorina rudis*, Maton," by E. R. Sykes, in which all the specimens discussed were gathered in the Fleet Backwater, where the water becomes slightly freshened; and a contribution from the Rev. O. Pickard-Cambridge on the British Species of False-Scorpions. This last is practically a monograph, is illustrated by three excellent plates, and should prove of great assistance to those who devote themselves to Chelifers.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Mr. J. Ward, of Derby, has been appointed Curator of the Cardiff Museum.

Dr. Hermann von Ihering has been appointed Curator of the Zoological Department of the San Paulo Museum, Brazil.

Mr. Frederic V. Coville, of the Agricultural Department at Washington, U.S., has been promoted to the directorship of the Botanical Division, which recently became vacant through the death of Dr. Vasey.

Professor H. Newell Martin has resigned the Chair of Biology in the Johns Hopkins University, Baltimore, which he has held for seventeen years. We regret to add that the Professor's ill-health has necessitated this course.

Mr. John H. Cooke, B.Sc., Lecturer on Mathematics in the Maltese University, has been appointed to lecture on Geology in the same institution. Mr. Cooke will also have charge of the fossils and rock-specimens in the Museum of the University, which have hitherto been much neglected. Fortunately, nearly all the known Maltese fossils of importance are preserved in the British Museum.

The University of Edinburgh has conferred the honorary degree of LL.D. upon Dr. Ramsay H. Traquair, F.R.S., the eminent ichthyologist. The University of London has promoted Mr. J. W. Gregory, B.Sc., of the British Museum, to the degree of D.Sc.

There are several newly-appointed Professors of Botany. Mr. W. Bottomley succeeds to the Professorship at King's College, London. Dr. N. Wille, of Aas, has been appointed Professor in the University of Christiania and Director of the Botanic Gardens. Dr. Ferdinand Pax, formerly Privat-docent in the University of Breslau, has succeeded the late Professor Prantl as ordinary Professor and Director of the Botanic Gardens in that University. Professor M. Möbius, of Heidelberg, has been appointed Librarian and Teacher of Botany at the Senckenberg Institute at Frankfort, in succession to the late Dr. Jännicke. Dr. H. Mayr is the new ordinary Professor in the Forestry School at Munich. Mr. Percy Groom has been appointed Demonstrator in the University of Oxford, and Dr. Dreyer, of St. Gallo, takes the place of Dr. A. Koch as Assistant in the Plant-Physiological Institute in the University of Göttingen. At Göttingen, also, the position of Assistant in the Botanical Museum and Garden, vacated by Dr. Hallier, is now filled by Dr. Giesseler, formerly Assistant in the Botanic Institute at the University of Jena. Dr. Hallier, it may be added, has been appointed Assistant in the Botanic Garden at Buitenzorg, in Java.

AT a meeting held at the House of Commons on May 11, the Committee who have prepared a draft charter for a Welsh University explained their views to the Members for Wales. It is proposed that residence in the new University shall be essential for graduation, but it is hoped that a large number of valuable scholarships will be available for competition.

THE sum of £3,500 has been voted by the University for the improvement of the Oxford Botanic Gardens. The glass houses, according to Professor Vines, are in a very bad state of repair, besides being inadequate and old-fashioned. When the gardens were leased from Magdalen in 1876, it was stipulated the University should execute repairs at the cost of £5,580, but of this sum only £2,000 have hitherto been expended. The present vote is, therefore, a somewhat tardy fulfilment of the obligation.

A NEW American Marine Biological Laboratory, under the direction of Professors C. L. Edwards and A. J. Smith, is being founded at Galveston by the University of Texas. We have also received a circular from Messrs. Sinel and Hornell, of Jersey, announcing the establishment of a Biological Station in connection with their business in the Channel Islands.

THE Yale University, New Haven, U.S.A., is publishing bibliographies of the writings of its Professors. A useful list of the numerous contributions of Professor O. C. Marsh to Vertebrate Palæontology (1861–1892) is to hand.

RESEARCH as a part of Technical Education is promoted, we are glad to see, by some of the County Councils. We have just received from Dr. William Somerville a "Report on Manurial Trials in the County of Northumberland," which gives some useful results from a number of experiments on various artificial manures.

AMONG recent bequests, it is reported that the Marquis Ricci of Genoa has left a large sum for the foundation of a new scientific institution in his native city. The late Earl of Derby bequeathed £2,000 each to the Royal Society and Royal Institution of London.

MR. F. BRADY, C.E., has presented to the British Museum portions of the cores of Carboniferous rocks from the Dover boring. Several pieces of shale contain plant-remains sufficiently well preserved for identification, and they are now exhibited in the gallery of fossil plants at South Kensington.

THE Manchester Museum has just added to its series of useful handbooks a "Catalogue of the Types and Figured Specimens in the Geological Department." The Catalogue is reprinted from the Report of the Museums' Association for 1892. We have also received a second edition of Professor Marshall's "Outline Classification of the Animal Kingdom," which is now supplemented by an "Outline Classification of the Vegetable Kingdom" by Professor Weiss.

WE hear disquieting news from Brighton, where the Corporation is considering proposals for the enlargement of the museum. The Booth collection of birds, which was wisely placed by the founder in a situation as far as possible from the sea air and town smoke, is, we understand, in danger of being removed to the midst of the town, close to the shore. The Natural History collections in the museum, including the unique series of Sussex Cretaceous fossils, are also destined to be very inadequately provided for, judging from the plans we have had the privilege of inspecting. Surely, if the members of the Corporation committee are inexperienced in such matters and cannot obtain the advice of any resident naturalists, it is their duty to secure the services of an expert. We hope that wiser counsels will prevail.

We have received the third part of the second volume of the *Actes de la Société Scientifique du Chili*. Dr. Fernand Lataste contributes most of the small zoological notes, and another instalment of Borne's memoir on *Latrodectus* is published.

An excellent summary of the work of the Scientific Societies of Australia during the past year appears in the recently-issued *Year Book of Australia* for 1893. One new society was founded at Bathurst, New South Wales.

The Cothenius Medal of the German Leopold-Caroline Academy of Naturalists has been awarded to Professor Dr. Adolf Fick of Würzburg, in acknowledgment of his researches in the physiology of muscle.

The Geographical Society of Berlin has bestowed the Humboldt medal upon Dr. John Murray, of Edinburgh; and the Geographical Society of Paris has awarded a gold medal to Dr. Fridjof Nansen.

The two gold medals of the Royal Geographical Society have been awarded this year to Mr. Frederick C. Selous and Mr. Woodville Rockhill. Mr. Selous is the well-known hunter who has contributed so much to our knowledge of the "big game" of Africa. Mr. Rockhill is an American who has made many careful surveys of the "Land of the Lamas." The other awards of the Society are as follows:—The Murchison grant to Mr. R. W. Senior, of the Indian Survey; the Gill memorial to Mr. H. O. Forbes, for researches in New Guinea and the Malay Archipelago; and the Cuthbert Peek grant to Mr. Charles Hose for his explorations in Sarawak, Borneo.

Some months ago we referred to the fact that the Royal Geographical Society had opened its doors to women Fellows. This innovation, which was decided by the Council, instead of by a Special General Meeting of the Fellows, was looked upon as illegal and outside the jurisdiction of the Council under the Charter. A Special General Meeting, therefore, was called on April 24 last to consider the matter. Some 250 Fellows were present, and after an animated discussion, the question of admission of women as Fellows was negatived by 145 to 104. Although we heartily approve of the action of the Fellows, we are sorry to hear of this result, as we feel that the women who have been already elected Fellows of the Society have considerably more claims to the Fellowship than one-half of the male existing members. There has been for some time past much grumbling with regard to the difficulty experienced in getting a seat when some distinguished traveller is discoursing for their information, the reason being that so many ladies are admitted as visitors that numbers of the Fellows themselves are crowded out. Several circumstances account for this, one of which is that most of the meetings of the Geographical Society partake more of the evening "At Home" than of the scientific nature. At the Annual Meeting of the Society on May 29, we understand that Sir Mountstuart Grant-Duff resigns the Presidency, and Mr. Clements Markham is nominated to succeed him.

In the list of fifteen candidates selected this year by the Council of the Royal Society of London for election to the Fellowship, Natural Science is represented by the following names:—Professor J. Cossar Ewart, Dr. W. T. Gairdner, Sir Henry H. Howorth, Mr. E. T. Newton, Mr. C. S. Sherrington, Professor E. C. Stirling, Professor J. W. H. Trail, and Dr. A. R. Wallace. The list will be submitted to a meeting of the Fellows on June 1.

AT the Soirée of the Royal Society of London on May 10, there was little novelty in the Natural History Exhibits. Colonel Swinhoe showed a series of butterflies illustrating protective mimicry; Dr. G. H. Fowler exhibited a series of oyster shells, to illustrate the various modifications and rate of growth; the Zoological Society of London contributed a collection of lepidopterous insects reared in the insect house, and the Marine Biological Station sent some marine invertebrata from Plymouth; Dr. D. Sharp illustrated the sound-producing apparatus of ants; Professor Williamson showed the microscopic structure of some Carboniferous plants, and Mr. E. Wethered the micro-organisms in limestone. Mr. H. O. Forbes exhibited bird-remains from New Zealand and the Chatham Islands, and Mr. E. T. Newton had some casts of the skulls of the extinct Triassic reptiles discovered near Elgin.

AT the Annual Meeting of the Linnean Society on May 24, the officers were re-elected, Professor Charles Stewart retaining the presidency for another year. The newly-issued part of the *Journal* of the Society (Zoology, vol. xxiv., no. 154) contains a paper on the Buprestidæ of Japan, by G. Lewis; descriptions of Crinoids from the Sahul Bank, North Australia, by Professor Jeffrey Bell, and of Land-shells from Borneo, by Edgar A. Smith; a discussion of the affinities of the genus *Madrepora*, by George Brook; and on two new species of *Rhax*, by H. M. Bernard.

THE *Quarterly Journal of the Geological Society* (vol. xlix., part 2), which appeared in the last few days of April, is noteworthy as containing a paper on bivalved mollusca—a subject comparatively neglected in these recent years. It is very encouraging to find, too, that the Society has not been niggardly in regard to illustrations, for no less than four plates, containing 60 figures, are devoted to Dr. Wheelton Hind's paper on *Anthracoptera* and *Anthracomya*. Dr. G. J. Hinde contributes two short papers on Radiolarian rocks; the Rev. P. B. Brodie records some Cestraciont remains from the Keuper of Shrewley; and Mr. Lydekker has a note on a Dinosaurian Vertebra from the Wealden. Mr. Wethered continues his work on the singular structure he refers to *Girvanella*, but we are not prepared yet to consider this under the head of palæontology. Petrology is represented by Mr. Simmons, "On the Petrography of Capraja"; by Miss Raisin, "On the Variolite of the Lleyn"; while Professor Judd returns once more to the controversy, carried on between Sir A. Geikie and himself, over the succession of the rocks in Skye. Messrs. Fox and Teall describe "Some Coast-sections at the Lizard"; and foreign geology is treated of by Lieut. Frederick, on the New Hebrides group, and Mr. Power, on the Pambula gold deposits, New South Wales. In the May number, too, as is the custom, the President's Address is printed *in extenso*, together with the Annual Report, with obituary of deceased Fellows. Mr. Hudleston in his address has given a critical *resumé* of the "Recent work of the Geological Society," and also a readable account of the *Quarterly Journal* for 1892, while at the same time briefly sketching British geology published elsewhere.

THE Trinidad Field Naturalists' Club has issued pt. 7 of its first volume of Proceedings. Mr. Oldfield Thomas contributes a list of the known mammals of Trinidad, with valuable hints for collectors who can still add greatly to knowledge of the subject. Mr. J. H. Collens records some personal observations on the Trinidad manatee, which is now almost extinct; and Mr. W. M. Crowfoot furnishes a preliminary list of Trinidad butterflies. There are other minor communications, and the club appears to be in a very prosperous condition.

OBITUARY.

JAMES WOOD MASON.

WE regret to announce the death of Professor James Wood Mason, which took place at sea in the middle of May at the early age of forty-nine. The deceased naturalist had for some time been in failing health. He went to India in 1869 as Assistant in the Indian Museum, Calcutta, and in 1885 succeeded Dr. J. Anderson as Superintendent of that institution, and also as Professor of Physiology in the Medical College. In addition to his work in Calcutta, he undertook two expeditions (in 1871 and 1873) to the Andaman and Nicobar Islands, where he made considerable collections, and in 1880 he investigated, on behalf of the Government, the life-history of the destructive "tea-bug" in Assam. He made a special study of crustacea and insects, paying particular attention to the Phasmidæ and Mantidæ, a work on the latter group being unfortunately cut short by his untimely death. His contributions to science are contained in numerous papers in the *Journ. Asiat. Soc. Bengal*, the *Ann. Mag. Nat. Hist.*, and other journals.

DR. MOLESCHOTT, Professor of Physiology in the University of Rome, died on May 20 at the age of seventy. A Dutchman by birth, he became Professor of Physiology, Anatomy, and Anthropology at Heidelberg in 1847, removing nine years later to Zurich, thence to Turin, and in 1879 to Rome.

MR. WILLIAM COTTON OSWELL, the well-known African traveller, died on May 1 at the age of seventy-five. The death is also announced of the traveller and botanist, Dr. J. BRAUN, who had been exploring Madagascar.

AMONG other recent deaths we also have to record those of Mr. EDWARD VIVIAN, of Torquay, Editor of MacEnery's "Cavern Researches" (1859); of Dr. JOHN PASSERINI, Director of the Botanic Gardens in the University of Parma; of Professor FERDINAND [illegible]NFT, the veteran geologist; of Professor ROBERT HARTMANN, the [illegible]man anatomist and anthropologist; and of the entomologists, [illegible]. HONRATH and J. BIGOT.

CORRESPONDENCE.

Natural Selection and Lamarckism: A Correction.

On page 337, line 17, I would ask the reader to substitute the word "evident" for the word "admitted," as I find I was in error in supposing that "it is admitted" that Natural Selection would evolve a general or widely-diffused sense of touch. Mr. Spencer's views on the matter are more extreme and exclusive than I gathered from his article. In the latest number of the *Contemporary Review*, he denies that either general or special sensitiveness of the skin results from Natural Selection. I am sorry that I misinterpreted his view on this point; but I fail to understand how a great philosopher can accept Natural Selection as a factor of evolution and yet suppose that it would play no important part in developing a sense so necessary for safety and survival as that of touch.

Wm. Platt Ball.

Mr. Hick and Calamostachys.

In a paper which appears in the number of Natural Science for May (vol. ii., pp. 354-359), Mr. Hick has, if I mistake not, brought before the public for the third time his views respecting the structure and affinities of *Calamostachys Binneyana*. I am not disposed to enter upon what, to be of any value, must be a prolonged and detailed controversy; but were I to allow this paper to be circulated unnoticed by me, such a course might be equivalent to an admission that I had made serious blunders, which Mr. Hick had not only corrected, but that his evidence furnishes what he claims to be "a complete solution of the difficulty thus presented." This claim I must definitely decline to recognise. I fear that the difficulties in the way of obtaining this solution are greater than Mr. Hick realises.

For several months my friend, Dr. Scott, and I have been working together, at the Joddrell Laboratory at the Kew Gardens, re-investigating the entire subject of the Carboniferous Calamariæ, including under that comprehensive term the two genera *Calamites* and *Calamostachys*, having as material for our investigation such a collection of sections of these objects as has no existence outside my cabinets. We hope to place before the Royal Society the results of these minute and careful studies, elaborately illustrated, before the close of the present year. This memoir will be our true answer to Mr. Hick's statements. Meanwhile, that gentleman must excuse me if I decline to recognise the accuracy of a considerable number of statements contained in his paper, or admit that he has settled the controverted questions so conclusively as he claims to have done.

W. C. Williamson.

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INDEX.

An asterisk (*) indicates that a figure is given.

Aderhold, R., 469
Adjuma-yama, 259
Æpyornis, 76
**Aetheolepis*, 449
African exploration, 81
Age of earth, 14
Agriculture and birds, 70
*Air-sacs of birds, 346
**Alcelaphus lichtensteini*, 386
Aldabra group, 166
Algæ, 87
Alsatian geology, 169
American Assoc., 156, 403
American botany, 328
American Mus., 155, 231
Ammonoidea, 408
Ammonites, classification of, 140
Amœba, structure of, 252
Amphioxus, 303
Anemones, alpine, 167
Anomalopteryx, 318
"Antarctica," 54
Antarctic whaling, 3
*Antennæ of Trilobites, 162
Anthropological Inst., index to publications, 259
Ants, 267
Aptornis, 312
Apus, 325
Arachnids, 239, 441
**Archæopteryx*, 275, 400
Argentine Museum and Lydekker, 94
Ascidians, 222
Athens Bot. Mus., 154
Auckland Mus., 236
Australasia, 458
Australian flora and fauna, 187
Australian minerals, 230
Australian Mus., 236

Bacilli, 252
Bacteriology, 150
Bajocian, 405
Balæna australis, 3
Bather on Phylogeny, 238
Bather on Science at World's Fair, 336
Bats, Italian, popular names of, 15
Bay, J. C., 165
Beecher's work on Brachiopoda, 15
Beecher on Trilobites, 163
Bees, 267
Beddard on earthworms, 109
Bergen Mus. Year-Book, 469
Berlin Bot. Gard., 154
Berne Univ., 154
Bibliography; *see* Recent Literature.
Biology, 229
Birds, 226
*Birds, air-sacs and bones, 28, 346
Birds, Dictionary of, 146, 391
Birds, Gadow on, 149
Birds of Holland, 236
*Birds, respiration of, 28, 346
Bird's wing, 275
Bison in Caucasus, 10
Blake on Ammonites, 140
*Bones of birds, 346
"Borderland," 82
Borneo and its mammals, 302
Borneo closed to collectors, 419
Botanical bibliography, 165
Botan. Congress at Genoa, 166
Botan. Congress at Madison, 328
Botanical indexes, 379
Botanists, index to, 301
Botany and geography, 152
Boulder Clay, 423
Boulders in coal, 256
Brachiopoda, 15
Brachydiastematherium, 84
Branchipus, 325
Breeding of animals, 436
Brinton, D. G., 75
Bristol Nat. Soc., 237
British Assoc., 161, 293, 313, 314, 403
British East Africa, 462
British Eocene Bryozoa, 257
British Guiana, 71, 312
British Hepaticæ, 306
British Mus., 155, 311 (2)
British reptiles, 306
"Broads," 151
Broeck, E. v. d., 5
Bronn's "Klassen, etc.," 305
Brooks on *Salpa*, 222
Bryozoa, Eocene, 257
Buckman on Bajocian, 405
Buckman on Recapitulation, 138
Bull. Dept. Geol. Univ. California, 170
Bulman on Glacial Period, 261
Burkill, J. H., 7
Butler, A. G., 235

Cacao, 92
Cacaoroth, 418
Caffein, 418
Calamostachys, 80
Calcutta Zool. Gard., 311
Caldwell and *Ornithorhynchus*, 8
Californian Acad., 235

Californian geology, 170
Canterbury College, 235
Cape Town Mus., 470
Careless titles, 331
Carini Cave, 455
Carlisle Mus., 469
*Carpenter on Arachnids, 441
Carpenter on habits of Ants, Bees, and Wasps, 267
*Carpenter on Insects, 441
Carus, J. V., 260
Caves explored, 12, 455
Caves, Maltese and Sicilian, 455
Cecidomya, 353
Cephalaspis in Thurso flags, 255
Cephalopod Eggs, 112
Ceratodus, 8
Ceylon Lepidoptera, 153
*Charterhouse Museum, 40
Cheddar cliffs, 401
Chicago Mus., 470
Chicago, Nat. Sci. at, 336
Chitons, 396
Clarke, C. B., 8
Climate and life, 261
Coal fossils, 398
Cockerell, T. D. A., 235
Cockerell on Slugs, 420
Cockchafer fungus, 86
Cold and plants, 261
Cole on Geology in Education, 332
Cole on the Petrographical Microscope, 465
Collecting, 419, 466
Collections—
Deby (diatoms), 155
Gregory (diatoms), 155
Greville (diatoms), 155
Newberry (books), 311
O'Meara (diatoms), 155
Ralf (diatoms), 155
Smith (diatoms), 155
Worthen (fossils), 397
Zigno (fossils), 236
Colour of Fishes, 32
Columbia Plains, botany exploration, 236
"The Conchologist," 422
Conchology, recent progress, 35
Conifers, 249, 411; distribution of, 250
Conrad's fossil shells of Tert. of N. America, 230
Copley medal, 470
Cornish mineral, new, 258
Correlation of Geological Formations, 404
Coryanthes wolfii, 327
Cotteswold Nat. F. Club, 471
Coulter, J. M., 235
Cretaceous sediments, 228
Croydon Micro. Club, 237
Crustacea, Palæozoic, 305
Cunningham on Variation, 282
Cynips, 353

Daisy, malformation of, 416
Dall on Polar Axis, 331
Dall on World's Fair, 336
Dalla Torre, K. von, 469
Dammer, U., 154
Dartmoor granite, 257
Davis, J. W., 260
Dawkins on Glastonbury Lake-Dwellings, 344
Dawson, J. W., 154
Deecke, W., 75
Demonstrators in Science, 321
Dendy, A., 235
Deserts in Britain, 367
Development of Insects and Spiders, 441
Dicotyledons, 326
Dimorphism of Foraminifera, 6
Diseases of plants, 418
*Distrib. of birds, 100
Distrib. of earthworms, 109
Distrib. of Mammalia, 93
Dorpat Univ., 75
Dover boring, 92
Drake del Castillo, 247
Dreyer, Dr., 154
Drift deposits, 423
Drought and plants, 8, 315
Dublin, Geology of, 421
Dwarf animals, 254

*Eagle, 350
Earth's age, 14
Earthworms, 109
Eastwood, Miss A., 235
Educational and professional science, 321
Elliot, G. F. Scott, 83, 419
Embryos, 112
Emendation of nomenclature, 472
*Emeu, 146
Energy of bacilli, 252
Eozoön, 14
Epiphytes, 172
Essex and Chelmsford Museum, 76
Essex Field Club, 76
Ethnology of E. Africa, 304
Eurypterida, 124
Evolution of decoration, 68
Evolution of ethics, 62
Evolution of life, 222
Expeditions—
To Columbia plains for plants, 236
Gregory's (E. Africa), 81, 169, 259
To Labrador, 169
To Peloponnesia for plants, 251
To Western Territories for fossils, 237
Elliot (Uganda; Botan.), 419
Extinct Mammalia in Balkans, 84
Extinct Mammalia, distribution of, 93

Fauna of Siberia, 331
*Feathers, 197
Feeding of fishes, 244
Finn on Telegony, 436
Fisher, Ed., 154
Fisher, W., 75
Fishes and temperature, 163
Fishes, breeding of, 244
Fishes, colours of, 32
Fish-scales, 448
*Flamingo, 147
Flight of birds, 346
Flint implements, 229
Flora of Dakota Group, 346

Flower on Museums, 158
Flowers on Leaves, 167
Foraminifera, 5
Foraminifera, Sherborn's Index to the, 466
Forbes on *Anomalopteryx*, 318
Forbes on "Antarctica," 54
Forbes on Moas, 318
Fossils, importance of, 407
Fraas, E., 12
French Assoc., 157
Fullerian Professor, 469

Galls, 353
Gamble, F. W., 154
Ganoids, 449
Gasteropoda, liver of, 35
Gazetteer of Gt. Britain and Ireland, 395
Gedge, Ernest, 395, 420
Geikie, on work of Geol. Survey, 330
Geographical Distrib. of Animals, 288; of Birds, 100; of Earthworms, 109; of Mammalia, 93
Geographical Soc. and ladies, 77
Geological Congress at Chicago, 156
Geological formations in America, 406; in Europe, 404
Geological Soc., index to journal, 155
Geol. Survey, 235, 300
Geol. Survey, report of, 258
Geol. Survey of India, 78
Geol. Survey of Ireland, 235
Geological Text-Book, 464
Geologists' Assoc., alteration of Rules, 156
Geology in education, 332
German Anatom. Soc., 312
Giard, A., 86
Giessler, Dr., 154
Glacial deposits, 423
Glacial deposits of the Tyne, 170
Glacial Period and Fauna and Flora of Britain, 261, 423
Glacialists' Magazine, 170
Glaciers, 58
Glaciers in Central France, 256
Glastonbury Lake-Dwellings, 344
Glen Roy, 257
Goodrich on Arachnids, 239
Gravels and Sands, High-Level, 423
Greenock N. H. Soc., 398
Gregory, J. W., 81, 257, 259
Gregory on *Eozoön*, 14
Gregory on Rock-forming Minerals, 465
Grob, A., 469
Groom on Epiphytes, 172
Growth of plants, 414
Guernsey Soc. Nat. Sci., 156
Gurke, M., 154

Hail, 395
Haliclystus, abnormalities of, 33
Hamilton on fishes, 163
Hamilton on sleep in animals, 240
Har Dalam Cave, 455
Haslemere N. H. Soc., 76, 312
Hawks of U.S., 70
Headley on bird bones, 346
Headley on bird respiration, 28
Hectocotylisation, 421
Hedley on Australia and New Zealand flora and fauna, 187
Hemeræ, 405
Hennings, P., 154
Henshaw on "Origin of Endogens," 130
Herdman, W. A., 90
Heterostyly, 251
Hick and *Calamostachys*, 80
Highland Geology, 169
High-Level Sands and Gravels, 423
Hong Kong Mus., 470
Hornbill, 350
Hornell on Mimicry, 31; Fishes, 32; *Palinurus*, 33; *Lucernaria*, 33, 204
Horses, 437
Howorth, H. H., 75
Howell, W. H., 154
Human frame, 300
Hume, W. F., 235
Hurst on *Archæopteryx*, 275
Hurst on *Lucernaria*, 209
Hurst on Phylogeny, 316
Hurst on Recapitulation, 239
Hutton on Moas, 317
Huxley and the Owen Memoirs, 422
Huxley on "Evolution and Ethics," 62
Huxley's Romanes lecture, 16, 62
Hyatt, A., 242

Ice action, 423
Ichthyosaurus, distribution of, 255
Ichthyosaurus restored, 10
Illinois Survey, 397
Indexes, 259
Indexes to Bot. and Zool. nomenclature, 379
"Index Kewensis," 305, 379
Indiana Acad., 398
Influence of Sires, 436
Inheritance of Characters, 408, 436
Insects of the house, 394, 445
Intern. Zool. Congress prize, 77
Invertebrata, zoology of, 69
Irpfel Cave, 12
Irving, A., 397
Irving on Bonney's "Do glaciers excavate?", 58
Isaria, 86
Isopoda, 168

Jackson's Index, 379
Jena Univ., 154
Jersey, 315
Jersey Biol. Station, 397
Johns Hopkins Univ., 154
"The Journal of Malacology," 422
"Jumping" species, 398
Jura, 331
Jurassic of Britain, 405
Jurassic of the Jura, 331
Jurassic of Yorkshire, 311
Jurjeff Univ., 75

Kaunhowen, F., 235
Kenia, Mt., 259
"Kew Bulletin," 165
Kingston Mus., 311
Kirk, T., 251

Kolarsth, 418
Krüger, F., 469

Labrador, 169
Lady "fellows," 77
Lake-dwellings, 344
Lake Superior, 243
Lake vegetation, 248
Lamellaria and *Leptoclinum*, 90
*Lampreys, fossil, 128
Land, underground waste of, 170
Lankester, Ray, 4
Laramie mammals, 237
Latter on Charterhouse Museum, 40
Latter on Plague of Wasps, 273
Laurie on Eurypterids, 124
Lavis on *Eozoön*, 14
Lavis on Pipernoid Structure, 218
Lawson, A. C., 243
Leaves, rainfall and forms of, 95
Leicester, Geology of, 421
Lemon's Scrap Book, 322
Lepidodendron in Devonian, 256
Lepidoptera of Britain, 69
Lepidostrobus, 417
Leprosy, 79
Leptoclinum, 90
Leptonychotes, 4
Lesquereux's Flora, 326
Lilium martagon, 409
Limestones, dwindling of, 164
Limulus, 441
Lincolnshire Nat. Union, 157
Lindahl, J., 397
Linnean Soc. Medals, 76
Lions in India, 14
Lithinus, 312
Liverpool Geol. Soc., 312
Liver of Gasteropods, 35
Lizard, movement of, 253
Lobodon, 4
Locomotion of animals, 253
London Water Supply, 258
Lucernaria, abnormalities of, 33
Lucernaria, 204, 319
Lunar craters, 48
Lydekker and Argentine Museums, 94

McClure, C. F. W., 397
Macdougal, D. T., 235
McGill College, 154
Madagascar fauna and flora, 192
Madras Mus., 311
Magdalenian period, 12
Magnin, A., 248
Maia squinado, 31
Major on Italian names of Bats, 15
Major on Squirrels, 9
Malacológical Soc., 237, 398
Malay flora, 409
Malta Mus., 92
Maltese Caves, 455
Mammals of America, 288
Mammoth Cave, effect of air on senses, 13
Manchester Geol. Soc., 398
Manchester Micro. Soc., 156
Marey's photographs, 253
Marine Biol. Assoc., 155
Marine Biol. Labs., 311
Marine Zoology, 466
Marsupials and Placentals, 168
Masdevallia, 153
Mashunaland, 384
Masters, M. T., 249
Matabililand, 384
Matthew, W. D., 162
Mechanical Evolution, 253
"Mediterranean Nat.," 260
Melbœurne Mus., 236
Menodus, 84
Meteorology, 73
*Microscopical methods, 112
"Midland Nat." (death of), 171
Migration, 299
Miliarakis, S., 154
Miliolidæ, 5
Millport Biol. Station, 469
Mimicry, 312
Mimicry of *Lamellaria*, 90
Minchin on Methods in Microscopy, 112
Miocene squirrels, 9
Miura, S., 259
Moa in Queensland, 253
Moas, 317
Mocquerysia, 168
Mollusca of N. Zealand, 90
Monocotyledons, origin of, 130
Moon, surface of, 48
Moore, J. E. S., 252, 325
Mountain ranges, 371
Movements of plants, 415
Munro, R., 257
Museums; *see* under name.
Museums Association, 1, 157
Museum, how to construct a, 158
Museums, provincial, 1

"National Trust for Places of Interest and Beauty," 401
Naturalists' Journal, 260
Nautiloidea, 408
Nautilus, 408
Nearctic region, 288
Newberry fund, 397
New Guinea, geology of, 93
Newspaper science, 12, 259
New Zealand, 461
New Zealand, flora and fauna, 187
Nomenclature, 472
Nomenclature of Orchids, 327
Norf. and Norw. N. H. Soc., 312, 398
Nottinghamshire Geol. and Nat. Hist., of, 301
Nuclear division, 325, 409
Nummulites, dimorphism of, 6
Nuthetes, 254
Nutrition of plants, 18
Nyassaland, 462
Nyassaland timber and precious stones, 171
"Nyassa News," 171

Obituaries—
Bain, Thos., 399
Baldamus, A. K. E., 467
Bennett, Geo., 467
Brook, G., 232
Charlesworth, E., 308, 468
Crosskey, W. H., 399

Obituaries (*continued*)—
Davis, J. W., 160, 231
Dinning, W., 468
Emin Pasha, 307
Hagen, H. A., 467
Hantken, Max, 233
Higgins, H. H., 159
Homfray, D., 234
Jenyns, L., 309
Kiaer, F. C., 234
Miura, S., 259
Nyman, C. F., 74
Pascoe, F. P., 159
Pratt, Anne, 234
Rae, John, 160, 231
Reid, Wm., 74
Schnitzer, E., 307
Seaton, H. E., 74
Semper, K., 74
Shrubsole, G. W., 233
Strauch, A., 310
Stur, D., 468
Viallanes, H., 160
Vine, G. R., 234
Œdema of Tomato, 418
Ogilvie, Miss M. M., 154
Oken's Library, 80
Oldham, H. Y., 75
Oliver, Daniel, 76
Ommatophoca, 4
Ontogeny, 138, 204, 238, 316
Orchids, 153, 327, 410
Orchids in Borneo, 419
Organisation of Science, 241
Ornithorhynchus, 8
Orthoceras, 408, 420
Ostracoda of Wyoming, 255
Owen Memorial, 76, 468
Owens Coll., 154
Owls of U.S., 70
Oxford Univ., 321
Oxford Univ. Jun. Sci. Club, 4
Oysters, 304

Padua Univ., 236
Pahang flora, 409
Palæontographical Soc., 155
Palæontology by Woods, 462
Palæontology by Zittell, 227
Palæospondylus, 128
Paliurus, fighting stratagem of, 33
Palms in Cretaceous, 256
Parallel roads of Glen Roy, 257
Pasteur Inst. in India, 75
Pea crops, early ripening of, 14
Pelargoniums, diseased, 418
Pelseneer's classification of Mollusca, 36
Percival on Plant Nutrition, 18
Perfection of Man, 300
Phascolonus, 254
Phycological Memoirs, 87
Phylogeny, 138, 204, 238, 316
Picardy flint weapons, 229
Pipernoid structures of rocks, 218
Pitcher plants, 85
Placentals and Marsupials, 168
Places of beauty, 401
Plant-cells, 90
Plant diseases, 418
Plant growth, 414
Plant movements, 415
Plant nutrition, 18
Plant respiration, 413
Plants in lakes, 248
Plants of Newfoundland and Labrador, 17
Plants of Polynesia, 247
Plasmodium, 4
Pleske, T., 469
Polar Regions, 397
Pollard, H. B., 154
Polynesian plants, 247
Popular Geology, 89
Potato, shooting, 91
Poulton, E. B., 75
Prague Mus., 312
Prague Univ., 469
Prestwich on Submergence, 324
Prestwich on Uniformitarianism, 330
Princeton Coll., 397
Protective mimicry, 31
Protoplasm, 4, 325
Protozoa, 112, 252
Pterygotus, 124
Purdue Univ., 235
Pycraft on Feathers, 197

Queensland, geology of, 408
Queensland Geol. Survey Mus., 236

Rainfall and forms of leaves, 95
Raisin, Miss C. A., 83
Ramsay, E. P., 235
Reade on High-Level Gravels, 423
Reade on Mountain Ranges, 371
Recapitulation Theory, 138
Recent Literature:—
Arachnida, 447
Archæopteryx, 281
Botany, 165
Coal-fossils, 398
Deserts and Steppes, 370
Epiphytes, 186
Eurypterida, 127
Feathers, 203
Fish Scales, 450
Index Plant. Phan., 305, 379
Indiana, 398
Insecta, 447
Lacernaria, 204, 217
Madagascar, 196
Maltese and Sicilian Caves, 457
Mollusca, 38
Palæozoic Crustacea, 305
Telegony, 440
Zool. Record, 393
Red Deer with 10 points, 253
Reid on Deserts or Steppes in Britain, 367
Rendle on "Origin of Monocotyledons," 130
Respiration of birds, 28
Respiration of plants, 413
Reviews:—
Balfour's Decorative Art, 68
Barret's Lepidopt. of British Isles, 69
Brinton's Pursuit of Happiness, 67
Britten and Boulger, Index to Botanists, 301
Bronn's Klassen, etc., 305

Reviews (*continued*)—
Brown's Nat. Map of Scotland, 301
Brown's S. Australian Minerals, 230
Butler's Household Insects, 394
Cambridge Nat. Sci. Manuals, 395
Carr's Nottinghamshire, 501
Cassell's Gazetteer, 395
Cassell's Year-Book of Science, 230
Conrad's Fossil Shells, 230
Dean's Oysters, 304
Drude's Botanical Geography, 152
Emerson's English Lagoons, 151
Fisher's Hawks and Owls of U.S., 70
Furneaux's Collectors' Handbook, 466
Gadow's Birds, 149
Geikie's Geology, 464
Hampson's Lepidoptera, 153
Hatschek's *Amphioxus*, 303
Heterocera of Ceylon, 153
Hose's Borneo, 302
Hudson's Birds in a Village, 226
Hume's Cretaceous Sediments, 228
Jackson's Index Plants, 305
Journal of Marine Zoology, 466
Loewinson-Lessing, Tables of Rock-forming Minerals, 465
*Newton's Dict. of Birds, 146, 169, 391
Paulitschke's Ethnol. E. Africa, 304
Pilloy and Bourbier's Picardy, 229
Rankin's Zambesi, 462
Rodway's British Guiana, 71
Romanes's Weismannism, 389
Russell's Hail, 395
Schenk's Bacteriology, 150
*Selous's Africa, 384
Shipley's Zool. of Invert., 69
Stürtz's Siebengebirge, 304
Vogdes's Crustacea, 305
Waldo's Meteorology, 73
Wallace's Australasia, 458
Wood's Palæontology, 462
Woolward's *Masdevallia*, 153
Zittel's Palæontology, 227
Rhinodon, 311
Richardson, B. W., 75
Richthofen "Festschrift," 260
*Rock-forming Minerals, 465
Rock structures, 218
Romanes lecture, 62
Rothera on Galls, 353
Rothschild Mus., 76, 236
Roy. Saxon Mining Acad., 154
Royal Societies Club, 171
Royal Society Medals, 470
Russia, Geol. Map of, 92
Rutley on Limestones, 164

Salpa, 222
Sanderson's Address, 293
Sardinia Soc. of Med. Sci., 237
Scales of fishes, 448
Scent in fishes, 244
Scharff on Pre-glacial fauna, 400
Scherren on *Lucernaria*, 319
Schinz, H., 235
Schists, 295
Schlumberger, C., 5
Scientific Clubs, 171
Scientific rubbish, 322
Scientific terms, 242
Scorpion, movement of, 253
Scotland, naturalists' map of, 301
Scottish mammals and birds, 14
Scudder's Nomenclator, 381
Scyphomedusæ, 204
Seals, antarctic, 3
*Sedgwick Museum, 451
Seebohm's address, 297
Seedlings of Conifer, 411
Seeds, distribution of, 7
Seeley, H. G., 254
Selborne, botany of, 329
Selborne Society, 157
*Selous in Africa, 384
Senecio johnstoni, 81
*Sharpe on Distrib. of Birds, 100
Shell Growth, 35
Sherborn on Madagascar, 192
Sherborn's index to Foraminifera, 466; to genera and species, 382
Siberia, 331
Sicilian Caves, 455
Siebengebirge, 304
Sierra Leone, 83
Singapore flora, 409
Sight in fishes, 244
Siphuncles, 420
*Skua, 350
Sleep in animals, 240
Slimonia, 124
Slugs, 420, 422
Smith, Miss A. L., on Rainfall and Leaves, 95
Smith, Lorraine, 246
Smithsonian Institute, 236
Snake, movement of, 253
Snowdon, 401
Soc. Belge Géol., 237
Soils and flowers, 152, 167
Sollas, W. J., 235
Soraner, P., 469
Spangolite, 258
Spencer, Baldwin, 75
Spencer, L. J., 311
Squirrels, 9
Staining fluids, 112
Stamens, 251
Starch, 18
Steppes in Britain, 367
Stewart, Chas., 469
Stirrup, M., 256
Stoney, J., 262
Stratigraphy no evidence of age, 407
Styles, 251
Stylonurus, 124
Submergence of Europe, 324, 423
Surface of moon, 48
Swiney Lectures, 311

TAPIRS, distribution of, 253
Teall on Crystalline Schists, 295
Teall on Uniformitarianism, 295, 330
Tebb on Leprosy, 79
Technical instruction, 155
Telegony, 436
Temperature and fishes, 163
Terms used in Science, 242

Terracciano, A., 469
Theobromin, 418
Thrushes and mud-lined nests, 14
Thyroid gland, 246
"Timehri," 312
Tipula, 446
Titanotherium, 84
Toula, F., 84
Townsend, C. H. T., 311
Toynbee Hall, 314, 315
Traquair, R. H., 255
Tricuspodon, 254
*Trilobites, 162
Tring Mus. *v.* Rothschild.
Trinidad Field Nat. Club, 312
Tristram on Naturalists, 298

Uline, E., 235
Uniformitarianism, 295, 350
Universities; *see* under name.
Univ. Coll. of Wales, 156
Univ. Extension Teaching, 16

Variation, 282
Vegetal galls, 353

Waghorne's plants, 17
Wallace on Australasia, 458
Wasps, 168, 267
Wasps and toads, 168
Wasps, plague of, 273
Watts, W. W., 235
Weismannism, 282, 389
Whaling, 3
Wheat, old and new, 418
White, Gilbert, and Botany, 329
Wild flowers and drought, 8
Willis, J. C., 7
Wilson, G., 245
Winchester College, 402
Woldrich, Dr., 469
Women fellows, 155
*Woodward, A. S., on Fossil Lampreys, 128
Woodward, A. S., on Fish-scales, 448
Woodward, B. B., Recent Conchology, 85
Woodwardian Mus., 451
World's Fair and Nat. Sci., 336
World's Fair, Geology at, 422

*Xiphidium, 444

"Year-Book of Science," 230
Yorkshire Nat. Union, 471

Zambesi, 462
Zebbug Cave, 457
"Zigzags at the Zoo," 421
Zittel on Palæontology, 227
Zittel's "Palæontology," 93
Zones, 405
*Zoo-Geograph. Areas, 100
Zoological Indexes, 381
Zool. Record, 393, 422
Zool. Society, date of Proceedings, 470
Zurich Bot. Garden, 235

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NOTES AND COMMENTS.

The Museums Association.

THE season for annual congresses has arrived, and in Britain the Museums Association meets first among scientific bodies. This year, as we have already announced, the meeting is to be held in London, under the presidency of Sir William Flower, K.C.B., who delivers the opening address on July 3. The Association has already done much towards discussing the methods of Museum arrangement, and agreeing upon concerted action in various matters; but a meeting in the metropolis, where Museums, backed by the resources of the Government, have attained their greatest development, ought to lead to particularly important results.

Truly enough, there is much to be done in reforming many of the provincial Museums, if their efficiency is to be maintained and extended in accordance with modern ideas. Last year we had occasion to remark upon the peculiarities of some of the older foundations;[1] and, notwithstanding all the efforts of the Museums Association, we fear it will be long ere the collections "illustrating the travels of the local gentry" give place to the adequate representation of the Natural History of the neighbourhood in which each Museum is situated. Even newly-founded institutions, such as that at the Brassey Institute, Hastings, which begin well and with good intentions, soon find the difficulty of refusing to accept "curios" from local travellers; and until the governing bodies of the Museums are composed exclusively of scientific men who understand such matters, it is hopeless to expect any noteworthy reform.

We cannot help thinking, however, that the greatest difficulty in attempting reform results from the ignorance of most of the governing bodies of the Museums as to what constitute the qualifica-

[1] Natural Science, vol. i., p. 321.

tions and duties of a Curator. If the Museums Association could make its voice heard on this subject in the Town Councils of the land, it would be doing good service. Very frequently a Librarian is supposed to supervise the Museum; he may have an assistant for the actual work, but even if that assistant be an expert, he is allowed no independent judgment. Only within the last year, the Committee of one great public Museum in Britain refused to take the advice of an expert and appoint a trained Curator, preferring the claims of a respectable tradesman, who happened to have local influence, and who may or may not prove a success in his new duties. The climax was reached in our experience, however, a few months ago, when the following advertisement appeared in a contemporary—not in an ordinary newspaper, but in a journal that comes regularly under the notice of scientific men:—

> WANTED.—A resident Curator, Meteorological Observer, and Caretaker for the Museum and Library Buildings of the Royal Institution of Cornwall, Truro. Salary, £50 per annum, with rooms, coals, and gas. Applications in Candidate's own handwriting, stating Age and Scientific Qualifications, with Testimonials and References, to be forwarded, not later than February 14, to Major Parkyn, Truro.

We have no information as to the result of this munificent offer to a trained naturalist, but if anyone could be found at the price, it would be of great interest to the Museums Association to learn from Major Parkyn the nature of the "scientific qualifications" he obtained so cheaply. Even if the salary were adequate, it would be difficult to find any one man competent to undertake so many duties; and if the funds of the Royal Institution of Cornwall are so low that it cannot afford more than its present expenditure, it might at least be satisfied with a caretaker to keep things free from dust and mildew until better days.

Curators who know anything of their work cannot be had for less pay than a gardener or coachman, whatever the people of Truro and similar towns may think; and when the Museums Association has duly established that fact, it might profitably direct attention next to the question of "honorary curators." In theory it is, no doubt, an excellent idea to have a committee of specialists to assist the curator in the scientific details of the various departments over which his work ranges; and in many instances it is not possible otherwise to ensure accuracy in the labelling of the specimens. In practice, however, so far as our experience goes, the honorary curator is usually an ornamental personage who never looks at the cases from any other point of view than that of a draper arranging his shop window. We know a certain "honorary curator of the department of conchology" in one of our largest provincial museums, who had not even heard of Woodward's "Manual" until long after he had entered upon office. When such nonentities are appointed, they

simply meddle in general questions of administration and arrangement, which ought to be solely under the control of a competent curator; and the sooner the system now in vogue is abolished, the better will it be for the development of the museums.

We heartily wish success to the Museums Association in its efforts to benefit the community in these and other directions; and under the brilliant patronage with which the London meeting is favoured, much progress ought to result from the proceedings of this year.

The Antarctic Whaling Expedition.

As many of our readers are doubtless aware, a whaling expedition comprising four ships—the "Balæna," the "Diana," the "Active," and the "Polar Star" left Dundee in September last to try their fortune among the whales and seals of the Antarctic Ocean. Their destination was the region visited by Ross in his third Antarctic voyage in the years 1842–43; and the prime object of search the southern Right Whale (*Balæna australis*). Seeing how rarely the Antarctic regions are visited, and how imperfect is still our knowledge of their geography and natural history, it was arranged that the medical and some of the other officers of the expedition should endeavour to make observations, and collect specimens, so far as the exigencies of their other duties permitted.

The expedition has now returned safe and sound, and one of its members has contributed a preliminary account of its doings to the *Times*, from which we take the following summary.

It appears, indeed, that while much important information has been acquired as to the meteorology of the Antarctic, and some additions made to geographical science, the general scientific results are not so satisfactory as was hoped would have been the case; this being mainly due to the time of all persons on board being fully occupied in capturing and flensing seals, during the period when the vessels were in the most interesting regions. During a short stay in the Falklands, the officers learnt that the Antarctic fox is becoming well-nigh extinct; while Darwin's prophecy that the wild horses and cattle would eventually be replaced by the more remunerative sheep is being rapidly fulfilled. Moreover, through incessant pursuit, the fur-seal, although still found, is comparatively rare.

The first iceberg was sighted in lat. 59° 18′, and the same day (December 16) Cape petrels, and other members of the same order, were met with in shoals, while the seas were found to be absolutely swarming with various species of finner whales. Soon after the first seal was descried and promptly despatched; the species is not identified, although it is stated to be one of those first discovered by Ross. On the 22nd, flocks of sheathbills were met with, while, on the next day, the presence of the lovely white petrel and the olive colour of the water indicated the proximity of the pack-ice.

Although no right whales were sighted, a large number of seals, of various kinds, were taken, and by the middle of February the vessel in which the *Times'* correspondent sailed had secured its full quota. Three different kinds of seals were taken, which are described as the "large seal," the "white Antarctic seal," and the leopard seal. The species of the last is, of course, certain, while the second would appear to be the crab-eating *Lobodon carcinophaga*, and the first is probably Widdel's sea. (*Leptonychotes weddeli*). Besides these, it is stated that the party came across a fourth kind of seal, described as of large size, with a small head, small flippers, thick blubber, and a rather woolly pellage. Possibly this may prove to be the rare *Ommatophoca rossi*, at present known only by two immature examples; but, in any case, it is to be hoped that skulls and skins of all these seals have been brought home in a state fit for scientific examination. The writer describes the Antarctic seals as very foolish creatures. "The present generation have never seen man, and they survey him open-mouthed and fearful, during which process they are laid low with club or bullet. Sometimes they are so lazy with sleep that a man may dig them in the ribs with the muzzle of his gun, and, wondering what is disturbing their slumbers, they raise their head, which quickly falls pierced with a bullet. There may only be one seal on a piece of ice, which is usually the case with the larger kind; but the smaller kinds lie in half-dozens and tens, and as many as 47 were seen on one piece. Seldom do any escape—one cartridge means one seal."

PROTOPLASM.

AT a recent meeting of that very up-to-date society, the Oxford University Junior Scientific Club, Professor Ray Lankester exhibited a fine specimen of a creeping plasmodium. On the general subject of protoplasm he said that a very common error at present was to abuse the word protoplasm. It is *not* the name of any chemical substance, but of the living slimy material which is to be seen round the nucleus constituting the substance of all living cells. To this substance the name was first given some forty years ago, and for this substance the name must be retained. It was quite true that probably a definite chemical substance of very high complexity was present in all protoplasm—a substance to which the name "Proteil" might be applied if that name had not been appropriated for the hypothetical basal form of all matter. This chemical substance might never be isolated and worked out. By its very nature it was extremely unstable—changing its composition, vanishing, dissolving almost as you looked at it—and in living protoplasm besides this substance there must be present always a number of chemical substances, some of them on the way up to the highest point, some on the downward path. The late Professor Moseley always insisted on a similar

view. He used to say that the protoplasm probably of every different kind of cell, and certainly of every different plant and animal, was different. There were a great many protoplasms, for protoplasm was simply the name of a physical appearance always found in living cells, and in actual chemical composition varying from cell to cell, from organism to organism, and from moment to moment; but underlying all this flux of living material, no doubt, there was one definite chemical compound identical in all living matter.

Recent Work on the Foraminifera.

The chief activity in the study of Foraminifera has of late years been removed to Italy, and we are indebted to Fornasini, Tellini, Dervieux, Malagoli, and Mariani for a great number of papers, chiefly on the Tertiary forms, of more or less importance. The last one to hand is by Dervieux, who writes upon the Tertiary *Frondicularia* of Piedmont (*Bull. Soc. Geol. Ital.*, vol. xi. (2)). There are eleven forms figured, most of which are well-known, and some of which are common both to the Chalk and Tertiary deposits elsewhere. Three are named as new, but surely fig. 17 is *F. medelingensis* of Karrer.

Schlumberger, of Paris, has contributed to the *Mémoires* of the Zoological Society of France (vol. vi.) another of those elaborate researches into the Miliolidæ—*Monographie des Miliolidées du Golfe de Marseille*. The material was supplied to him by M. Marion, and was obtained from the "zone à Bryozoaires," in the gulf of Marseilles, at a depth of 30–40 metres.

Schlumberger picked out the Miliolines, numbering about 20, and has described and figured them with his customary exquisite care. No *Biloculinæ* were found, but there were 3 *Spiroloculinæ*, one receiving a new name (*inæquilateralis*); 1 *Sigmoilina* (*costata*, n. sp.); 4 *Triloculinæ* (*marioni*, n. sp.); 10 *Quinqueloculinæ* (*stelligera*, n. sp.); 2 *Massilinæ* (a new genus "of which the early chambers are arranged in five symmetrical planes, as in *Quinqueloculina*, while the last are placed in two planes or a single common plane of symmetry passing through the axis of the poles as in *Spiroloculina*") the species being *secans* (*Quinqueloculina*, d'Orbigny, no. 43 Tableau Méthodique) and *annectens*, n. sp.; and 3 *Adelosinæ*. The illustrations are given in four plates, photographed from drawings by the author, and the paper is further illustrated by thirty-seven of those beautiful sections that M. Schlumberger has delighted us with so often. We regret to see our old friend *Q. secans* disappear, but we have long since recognised the jeopardy of all *Miliolinæ*, which now cannot be safely named without the precaution of making sections and studying the internal arrangement of the test.

The most interesting event to be chronicled is the return of the prodigal son, M. E. Van den Broeck, who has fed for long off Japanese husks and now returns to his foraminifera. Van den Broeck has

published an "Etude préliminaire" (*Bull. Soc. roy. Malac. Belgique*, vol. xxviii.), and an "Etude sur le Dimorphisme des Foraminifères et des Nummulites en particulier" (*Bull. Soc. Belge Geol.*, vol. vii.). The latter is an elaborate paper of 41 pages, and discusses at length the views of Hantken, De la Harpe, Munier-Chalmas and Schlumberger, Tournouër, Fischer, and others, and states the following conclusions:—*A*. That dimorphism in the Foraminifera is met with in all types where conditions of life are favourable to existence. *B*. Dimorphism is "of absolutely initial origin and in relation with the origin of life itself." *C*. The characteristic of dimorphism in the Foraminifera is, as a general rule, furnished by the contrast existing between the dimensions of a section of the adult of two forms of the same species, and the dimensions *in an inverse sense*, and infinitely more accentuated, of the section of the central or initial chamber. *D*. From the similarity of external characters, etc., it is easy to assure oneself that the two forms *A*. and *B*. not only belong to the same group or zoological subdivision of one genus, but even to the same species. *E*. As to the difference in shape of the two forms in the adult state, the character of the spire, form and number of chambers, they never present but secondary variations. *F*. The difference of the initial stage appears due to the fact that *the Foraminifera reproduce themselves by two distinct processes* (*fission* and *gemmation*) [previously pointed out by Fischer (1870) and Dollfus (1890); see M. Van den Broeck's paper, p. 15]. *G*. Gemmation is well-known among the Foraminifera, Fission among the Fresh-water Rhizopoda. The shells of Foraminifera may be a true colony, of which the individual of each successive chamber may, by a kind of fission, be the product of the division of the preceding, remaining attached to the colonial skeleton by a non-liberated test. *H*. Whether the differential characters noticed in the dimorphous tests are connected with the shape and disposition of the chambers or the external characters, or whether these characters are connected with the spire, form, number, or grouping of the chambers in the forms *A*. and *B*. of the same species, or even the respective abundance of *A*. and *B*. in the same habitat, we ought to state, in favour of the principle of initial dimorphism, that these differential characters find, without trouble or argument, their *raison d'être* precisely *in the natural consequences of initial dimorphism*, due to the contrast in the reproductive processes. *I*. The sole objection that may be opposed to this interpretation, consists in the fact that to justify two initial forms in the same species it is necessary to prove—which does not yet appear to have been fully done—the existence of embryonic stages or at least young individuals showing, as in *A*. and *B*., large and adult forms, the divergence into megasphere and microsphere of the initial chambers. *J*. One may be able reasonably to add to the phenomenon of expulsion, setting aside the last external chambers of Nummulites, the fact that never has anyone noticed or figured an adult Nummulite, entire and intact, showing a mouth or opening perfectly preserved. *K*. Looking at the

question of the duality of the process of the Foraminifera, as much as at that of the support afforded to the Theory of Fission by the method of individual examination of the contents of each Foraminiferal chamber, the importance which must be attributed to the *question of nucleus among the Foraminifera* demands a complementary system of observation. For further detail and argument, the student must be referred to Van den Broeck's paper, as we have already exceeded the space at disposal; but the communication is so interesting and important that it appears an obvious advantage to quote at this length.

The Distribution of Seeds.

We are glad to observe the tendency of biologists at Cambridge to wander more and more from the long-prevalent exclusive fashion of section-cutting, and to attempt to solve some of the wider problems in the science. Another interesting item of research in this direction was communicated to a recent meeting of the Cambridge Philosophical Society, when Messrs. J. C. Willis and J. H. Burkill recorded their observations on the flora of the pollard willows near the University town.

The plants occurring in the tops of the willows near Cambridge have been recorded during the last few years, and amount to 80 species, occurring 3,951 times altogether in about 4,500 trees. Of these 80, only 18 form more than 1 per cent. of the total number of records. The rest have only a small number of records. As Loew has pointed out in a recent paper, these plants are of interest from the points of view of distribution of seeds and of epiphytism. Classifying them according to means of distribution, we find that 19 species have fleshy fruits; 1,763 records (44·6 per cent.) of these occur. Three species with burrs have 651 records (16·4 per cent.); 34 species with winged or feathered fruit or seed have 996 records (25·1 per cent.); 7, with very light seeds, have 421 (10·6 per cent.); and finally, of plants whose means of distribution is poor or somewhat doubtful, we have 17 species with 120 records (2·9 per cent.). It is thus shown very strikingly how the various distribution-mechanisms succeed, only the better ones showing in the list in any numbers. The bird-distributed plants figure much higher here than in such cases as, for instance, the flora of the churches of Poitiers (Richard).

The observations show clearly the fact that a seed is only carried a short distance by its distribution mechanism. Plants were always found upon the soil, within 250 yards at most, of those found in the trees. An analysis was made as far as possible of the birds' nests found in the trees, and pieces, often with ripe fruits, of many plants in the list were discovered in them, so that probably this means of distribution is of some importance.

With regard to epiphytism, Loew considers these plants as beginning this mode of life. Like true epiphytes, they possess good

methods of seed distribution. Their position does not call for any special means of supporting themselves, and the supply of humus is plentiful. *Mycorhiza*, which Loew found on many, was not observed in the few examined. The size of many of the shrubs, *e.g.*, Elder, *Ribes*, Rose, etc., was very remarkable; some elders were 3 inches thick or more, and as much as 12 feet high. Experiments are in progress upon the growth of plants in willow humus.

Expeditious Scientific Research.

Referring to Biology at Cambridge, we cannot refrain from recording that at the meeting of the Zoological Society of London on June 6, the exhibition of an egg of *Ornithorhynchus* was made the occasion for some indignant remarks on the action of a member of the University, in suppressing and rendering useless for the purposes of science, materials collected at the public expense in the haunts of *Ornithorhynchus* and *Ceratodus* in Australia. If Mr. W. H. Caldwell is unable to make use of the specimens he was deputed to collect, it is high time that he reported to that effect to the public institutions that endowed his research; and it would be well if in future the Royal Society added some clause to the conditions on which the Government grant is awarded, preventing any repetition of such circumstances. As biologists, we are glad to learn that one of the German Universities has undertaken the work of supplying us with information concerning the early stages in the life-history of the duck-bills and *Ceratodus*, and will shortly publish the preliminary results; as Britons, we cannot but regret that it should be necessary to depend upon the resources of a foreign nation for the solution of some of the most important problems in the Natural History of one of our own colonies.

Wild-flowers and Drought.

In the *Journal of Botany* for June, Mr. C. B. Clarke gives a few notes on the effect of the warm, dry spring on our wild plants. "A great number of annuals," says the writer, "usually regarded as autumn annuals, have already run their course." On May 6 a quantity of *Valerianella olitoria* was collected, not only in ripe fruit, but with the whole plant whitened as seen in corn-fields after harvest. The fruits contained abundance of perfect seeds ripe. On the same day various banks in Surrey were seen to be golden-yellow with the Mouse-ear Hawkweed (*Hieracium pilosella*). Two other Hawkweeds, (*H. murorum* and *H. sylvaticum*) were gathered in flower, the latter also "off flower" within the next eight days; their normal flowering season, according to the "Students' Flora," is July–September. The Heath *Erica tetralix* was almost as precocious, for a large bundle was picked in full flower at Bournemouth on May 18. Examples in full flower of nearly all the common autumnal weeds

were collected, such as *Centaurea scabiosa*, *Erythræa centaurea*, *Jasione montana* and others.

One effect of the drought was the shortening of the stems of many plants, thus the Marsh Thistles were flowering plentifully with stems less than one inch long. A small Pond-weed (*Potamogeton polygonifolius*) had flowered abundantly with stems one to two inches long on nearly dry mud; while the same species, abundant also in the streams, showed no signs of flowering; the warmth of the water causing, on the other hand, a luxuriant growth of stem and leaves. This was also generally found to be the case with water-plants, which have not flowered much earlier than in some other seasons. It is noticeable that in no case was there indication of blossom on the Sundews or *Hypericum elodes*, though all three plants were observed in every state of moisture from saturation to desiccation.

Dr. Forsyth Major on Squirrels.

In criticising a certain book in a previous number of this Journal, we had reason to call attention to the circumstance that, in our opinion, the author had selected too comprehensive a title. We have now to mention a case where an author has done himself injustice by choosing a heading to his paper which errs in the opposite direction. We refer to a paper just published in the *Proceedings of the Zoological Society* (1893, pp. 179–215) by the well-known palæontologist, Dr. Forsyth Major, under the designation "On some Miocene Squirrels, with Remarks on the Dentition and Classification of the Sciurinæ." Any person reading the title would, we submit, imagine that the fossil squirrels were the important part of the paper; whereas, as a matter of fact, they form but an insignificant portion of its contents. The paper is, indeed, not only a re-classification of the squirrels, but also comprises a most important discussion on the question of trituberculism and the origin of the various types of mammalian dentition; the title giving not the slightest inkling of the latter portion of its contents. Although, at present, we have been able to give it only a hurried perusal, while we are by no means sure that we agree with many of its conclusions, the paper appears to us one of the most important that have been published for some years.

In regard to classification, Dr. Major, following the suggestions of some previous writers, utterly scouts the division of the squirrel-family into the Sciurinæ (true squirrels and flying squirrels) and Arctomyinæ (susliks and marmots); and divides it into the Sciurinæ Pteromyinæ, and Nannosciurinæ. The former embraces the grooved-toothed squirrels (*Rhithrosciurus*), the spiny squirrels (*Xerus*), the true squirrels (*Sciurus*)—the latter genus including the chipmunks (*Tamias*)—and the susliks and marmots. The second sub-family is represented by the three genera of flying squirrels; while the third includes the pigmy squirrels (*Nannosciurus*), which have been generally

placed in *Sciurus*, from which they are distinguished by their dormouse-like dentition. It will thus be seen how radical are the changes in classification.

Fossil teeth indicate that flying squirrels were abundant in the Miocene; and their occurrence there, with other circumstances, leads the author to conclude that they have originated quite separately from the Sciurinæ, even if they belong to the same family.

To enter into a discussion of the author's opposition to tritubercularism would greatly exceed our limits; but we may mention that he believes that the Eutherian dentition has been derived from a direct modification of the multituberculate type, and that tritubercularism is a secondary feature due to reduction. Certainly, one of the figures of a squirrel's molar reminds us very strongly of a molar of the duck-bill, and, as far as it goes, is suggestive of the derivation of both from a type in which the cusps were arranged in parallel longitudinal series. To believe, however, that "*Microlestes* may prove to be a remote ancestor of the Eutheria," if it had incisor teeth at all like those of its ally *Plagiaulax*, is, however, very difficult. We shall await with interest what the Transatlantic palæontologists have to say on the subject.

The Bison in the Caucasus.

Hitherto there has been much uncertainty as to the localities where the bison still remains in a wild state in the Caucasus, and naturalists are accordingly much indebted to Dr. Radde for clearing up this point in a brief communication published in the *Proceedings of the Zoological Society* (1893, pp. 175–7). The Doctor tells us that the animal is confined to the district around the sources of the Laba and Bjellaja on the north side of the range, extending eastwards from the former locality to the springs of the Selentschûk. It is, however, everywhere scarce, being generally met with only in twos and threes, although occasionally as many as five have been seen together, and once the tracks of a party of seven were detected. The survivors appear to have lost their original settled habits, and to have become wanderers in their last refuge, having, in some cases, crossed the main ridge of the Caucasus and made their appearance on its southern flanks. As there is, unfortunately, but too much reason to fear that the Caucasian bison will, ere long, have ceased to exist, we are glad to hear that Dr. Radde hopes to be able to publish an exhaustive memoir on the anatomy, distribution, and habits of this animal.

A Restoration of Ichthyosaurus.

Some months ago Mr. Lydekker referred in these pages (vol. i., p. 514) to Dr. E. Fraas' discovery of a specimen of the extinct marine reptile, *Ichthyosaurus*, showing the form and proportions of the dorsal and caudal fins. In the second edition of the Rev. H. N. Hutchinson's "Extinct Monsters," to the issue of which we referred

RESTORATION OF ICHTHYOSAURIAN REPTILES. By J. SM'T.
(*From* H. N. Hutchinson's "Extinct Monsters," second edition.)

last month, Mr. Smit has attempted a new restoration of the animal based upon this discovery; and by the courtesy of the author and publishers (Messrs. Chapman and Hall) we are able to reproduce the sketch on the previous page. The curious downward bend of the vertebral column into the lower lobe of the caudal fin is well indicated; and a life-like drawing of this kind affords a much more vivid conception of the animal than the original outline sketch of Dr. Fraas. We may add that Mr. Smit has executed other new restorations for Mr. Hutchinson's second edition, and criticisms of the first issue have been turned to profitable account.

CAVERN EXPLORATIONS.

LITTLE has been done of late in the exploration of caverns—perhaps there remains now little to be learned concerning the ancient fauna of which they provide evidence. Dr. Eberhard Fraas, however, has been examining the Irpfel cave in the valley of the Brenz, Würtemberg (*Zeitschr. deutsch. geol. Gesell.*, 1893), and Mr. E. Harlé has published, in the *Comptes Rendus* of the Natural History Society of Toulouse, an elaborate summary of the distribution of the Quaternary mammals in the south-west of France.

The cavern in Würtemberg investigated by Dr. Fraas proves to have been a hyæna-den, with some evidence of occupation also by the cave bear and man. The animals found distinctly indicate a Steppe-, not a Forest-fauna; and it is quite evident that the reindeer was contemporaneous with the mammoth, rhinoceros, and hyæna. It is also definitely proved, from the occurrence of flint-flakes, that man lived with these animals in Southern Germany.

The researches of Mr. Harlé also show that the country to the north of the Garonne was a great steppe—not a forest—at the same period. Remains of the Saiga antelope and *Spermophilus* are widely spread, and in four instances they have been found associated. The steppe is believed to have been replaced by forests at the end of what is termed the "Magdalenian" period, when there is reason to suppose that the climate became warmer and more humid.

OUR MONTHLY SELECTIONS.

THE office boy, or his equivalent so far as scientific matters are concerned, is more conspicuous than ever in the newspapers this month. We must, however, content ourselves with three selections from the Natural History provided for the edification of the general public.

Last month the *Westminster Gazette* tried to discourse learnedly on botanical questions, and provided some amusement for the readers of the June *Journal of Botany*; but this month it has even excelled itself by an attempt at a popular exposition of the Insect House in

the London Zoological Gardens. The fact that the journalist failed to count the legs of the stick-insect correctly may be excusable, if he were only a casual visitor; but he might at least have mastered some of the rudiments of zoology before he recorded his observations on the Atlas moth. This, we are told, "is rather bird than insect, and the wings have a depth and softness of plumage which seem to bring it within the animal kingdom." If moths are not animals, and if birds are animals that have evolved from insects, it is a misfortune the *Westminster Gazette* does not expound to us its new philosophy.

For further strange notions of zoology we may turn to the *World* of May 31, where we read:—"I have entitled this story 'Ponsonby and the Pantheress,' because Ponsonby's nocturnal visitor undoubtedly belonged to the genus Carnaria, species *F. pardus*, the *Pardalis* of the ancients." The confusion between *order* and *genus* is delightful, while the meaning of the mystical "*F.*" is apparently left to the intelligent reader.

Finally, in a notice of the skeletons of a man and horse recently mounted side by side in the Natural History Museum, published in the *Graphic* of June 10, we obtain the following information:—"In the *carpus* (wrist-bones) of the horse a further reduction in the number of bones as compared with man takes place, but it is especially in the bones which constitute the hand in man (*metacarpus*) in which extraordinary modifications will be noticed. It will be apparent that what is popularly known as the knee in the horse's foreleg is not the knee, but the middle finger in the human hand, greatly developed and strengthened to sustain the enormous weight of the horse's fore-quarters. It has the same number of parts as the human middle finger, and is terminated by the hoof, which corresponds to the nail of that finger." The uninstructed public are likely to be greatly benefited by the information that the terms metacarpus and hand are synonymous; while when they read that the horse's knee is the equivalent of the human third finger, even their proverbial faith in the so-called popular scientist is likely to receive a rude shock.

THE *Daily Chronicle* of June 10 quotes some curious observations recorded by Dr. Hovey on the effect of the pure air of the Mammoth Cave of Kentucky on the senses of smell and hearing. After coming out of the cave, the odours of the outside world, of the trees, grass, and flowers, are so strangely intensified that the different trees can be distinguished by scents inappreciable after breathing the ordinary air for half-an-hour. This effect is, to many sensitive natures, so over-powering that some of the visitors suffer from headache and nausea by a too sudden change from the densely oxygenated atmosphere of the cavern to the more impure one outside it. Hearing is also stimulated. All of the cave animals—which are blind—are so keenly endowed with this sense, that they dart away with rapidity should even a grain of sand fall on the surface of the water. Yet, if perfect silence

is preserved, so unconscious are they of the presence of an enemy that they can easily be captured by the hand or net when swimming about.

EARLY in the century lions were comparatively abundant in India, but at present they appear to be exclusively confined to the Gir of Kathiawar, where they are preserved by the Government. Two fine specimens are reported to have been recently shot by the Governor of Bombay and a party of sportsmen.

WE have received from Professor Duns a copy of the abstract of his paper on the early history of some Scottish mammals and birds, appearing in the *Proc. Roy. Soc. Edinburgh*, session 1892–93. It is a plea for the more "adequate description of the environment, physical and vital, of the mammals and birds earliest recorded by Scottish observers." The author enumerates the old publications to which reference may be made.

IN reference to the drought, the *Gardeners' Chronicle* of June 10 gives statistics showing the unusually early ripening of the first Pea crops. The record (supplied by Messrs. Hurst & Son) extends over ten years, and is as follows:—1883, June 21; 1884, June 26; 1885, June 24; 1886, June 24; 1887, June 25; 1888, June 26; 1889, June 16; 1890, not given, probably about June 18; 1891, June 27; 1892, June 15; 1893, June 4–6 at the latest.

IN the previous week's issue of the same journal the statement is reported that, owing to the drying up of ponds and watercourses, thrushes in the southern counties, especially the dry county of Hampshire, have been forced to build their nests without the usual mud lining. The question is put: How many droughty years would be needed to destroy the habit entirely?

IN reference to Mr. Clarence King's recent ingenious estimate of the age of the Earth (NATURAL SCIENCE, vol. ii., p. 81), the Rev. Osmond Fisher contributes an interesting article to the current number of the *American Journal of Science* (ser. 3, vol. xlv., pp. 464–468), pointing out that the rigidity of the Earth cannot be relied upon in estimating its age. He considers the supposed fact that the globe is rigid, still remains to be proved; hence no argument can be founded on it.

THE so-called primæval fossil, *Eozoon canadense*, has been subjected to so much destructive criticism that there are now few believers in its organic nature; but until this month no analogous structure had

been recorded as found under conditions that could explain the origin of so curious an arrangement of different minerals. At the meeting of the Geological Society of London on June 7, however, Dr. Johnston Lavis exhibited some ejected blocks of metamorphosed limestone from Monte Somma displaying a perfect eozoonal structure. They have been studied most carefully by Dr. J. W. Gregory and himself, and their microscopical characters correspond in all details with those of the original Canadian specimens. In many cases, on account of their freshness, the Monte Somma blocks exhibit some of the pseudo-organic structural details, such as the stolon-tubes, in far greater perfection than does the true so-called *Eozoon canadense*.

In the *Transactions of the Connecticut Academy* (vol. ix., March, 1893), Dr. C. E. Beecher has a paper on the "Revision of the Families of the Loop-bearing Brachiopoda." He arrives at the conclusion that the Terebratellidæ certainly appeared before Jurassic times, because they were well represented by several characteristic genera (*Kingena*, *Ismenia*, *Zellania*, and *Megathyris*); that among recent species, several separate generic and specific names may have been given to stages of growth of a few species; and that species must be based upon surface ornaments, colour, and form, with certain limits, and genera only upon structural features developed through a definite series of changes, the results of which are permanent in individuals evidently fully adult.

A second paper by the same author follows, on "The Development of *Terebratalia obsoleta*, Dall." This is illustrated by three plates, of which the first shows the development of the brachial supports in the Terebratellidæ, dealing chiefly with the *Megathyrinæ*, the *Dallininæ* and the *Magellaniinæ*; while the second and third plates are almost entirely concerned with the same structure in *Terebratalia obsoleta*. Like so much of Dr. Beecher's work, this is thoroughly worked out, and presents points of interest as well to the general reader as to the specialist.

In the *Zeitschr. für romanische Philologie* for April there is an interesting paper on the Italian popular names of the Bats, by Dr. C. J. Forsyth Major. Dr. Major is, fortunately, both a naturalist and a philologist, and one can therefore depend upon his determinations and researches. Not only from Italy itself has the author busied himself in rescuing these interesting words, but also from Corsica, Vaud, Savoy, the Jura, the Vosges, Dauphiné, Provence, etc., etc. Among the words in use that Dr. Major traces to the Latin *Vespertilio*, are:—Tuscany, *vipistrello*, *vispistrello*, *pipistrello*; Florence, *pripistrello*; Figline, *pimpistrello*; Prov. Massa, *spertello*; Prov. Macerata, *spiridillo*; Tenerano, *papastrello*; Premilcuore, *baibastrel*; Imola, *balbastré*; Padora, *barbastrégio*; Venice (ant.) *barbastregio*, *barbastelo*, *barbastrigo*, *barbastrillo*;

Tyrol (Ital.), *barbustéll*. The reference of several names from the dialects of Corsica and Sardinia to that of the Basques may meet with some opposition. We are glad to read the references to Homer, and to find due recognition of his observations on natural objects. This is not the first of these interesting contributions from Dr. Major's pen, and it is to be hoped that his researches in European Miocene Mammals, on Samos and on Madagascar, will yet leave him time to favour us with more of this laborious and interesting philology, the greater part of which is the result of personal investigation in the localities themselves.

IN the June issue of the *New Review* the Rev. S. A. Barnett makes some remarks on University Extension Teaching which are of interest as regards Natural Science. The Societies are just now threatened by the demand for popular teaching. Signs are not wanting that the popular lecturer is supplanting the thorough teacher, and while the former fills the largest hall in the neighbourhood, gives his twelve lectures, gets in fees which cover expenses, and satisfies everybody, the latter, "who handles his subject humbly and would make his students humble" and eager to learn, draws small classes and hardly gets employment. University teaching, says the writer, must dare to be unpopular, sending its teachers not to meet but to create a demand. "One of the best teachers in East London, whose mark remains after ten years, never drew a large class and never was popular; but he was thorough in his work, a master of his subject, and by his enthusiasm made others enthusiastic." Of course, this means that University teaching must be endowed.

Another danger is the absence of organisation. "Each society keeps, as it were, a shop, of which its lectures are the stock, and they are labelled with attractive titles. Active local secretaries for country towns or London districts come to the shop to buy, and are often led to the purchase only by appearances." There is thus a want of continuity, and the practice of taking "tit-bits" here and there is far too prevalent.

A concerted plan of action between the three great Societies and the establishment of definite series of courses stretching over two or three years would avoid the serious waste of time and effort which under existing conditions must sometimes occur.

IN one respect, the second Romanes' lecture at Oxford, like the first, was disappointing. Professor Huxley, like Mr. Gladstone, has a great reputation for the manner and form as well as for the matter of his addresses, and many of the audience of this second lecture, like many of the audience of the first, came expecting a brilliant display of the arts of oratory; but the Sheldonian theatre is among the worst

auditoria in the world, and during the greater part of the lecture those not immediately in front of the lecturer had some difficulty in hearing. We notice elsewhere the matter of the lecture, which has since been printed and published by Macmillan & Co.

The Rev. A. C. Waghorne, late of New Harbor, Newfoundland, has a collection of Newfoundland and Labrador plants for disposal. After 17 years' labour as a missionary, he is obliged by ill health to retire, for a time, from regular clerical work, and, being entirely without means of subsistence, hopes to derive some income by the sale of his plants. He is not himself a botanist, but his plants have been named by Messrs. Macoun, Warnstorf, Vasey, Bailey, Farlow, Hervey, Bebb, Underwood, and Eckfeldt. He proposes to issue complete lists of all he has collected in Newfoundland and on the Labrador, and to distinguish those of which he has material to spare. He hopes to have his list of mosses and lichens, at least, ready this month; the rest will probably not be prepared till the winter. A list of Sphagnaceæ has already been issued. As Mr. Waghorne will spend the summer in Labrador, applications for specimens should be made to the Rev. J. H. Bull, Whitbourne, Newfoundland.

I.

Recent Researches on Plant Nutrition.

IT was well-known before 1771 that a candle, in burning, vitiates the air, and on that account will not continue to burn in a closed air-space; but Priestley, in his "Experiments and Observations on different kinds of Air," records for the first time experiments made about this date which show that the restoration of air thus vitiated can be brought about by the intervention of plants.

Perhaps it was, to some extent, by accident that he was led to try the effect of placing a sprig of mint under a glass jar standing over water. The sprig he observed continued to grow for some months, and after the experiment the air inside the jar was found to be capable of supporting the combustion of a candle, and a mouse placed in it showed no signs of inconvenience. This was considered a remarkable fact, since it was to be expected that as air was known to be necessary for the support both of vegetable and animal life, the effect on it by the life-processes would be the same in both cases—in one case, deterioration was known to take place, and in the other, the same results, it was thought, ought to follow. The fact that a candle would burn in air which had supported plant-life for some time seems to have suggested to him the idea that possibly air injured by processes of combustion may be restored by the action of vegetation, and he accordingly devised experiments to test this hypothesis.

Sprigs of mint and spinach were enclosed in air in which a candle had been allowed to burn itself out, and it was found that the air was restored to its original condition, so far as combustion was concerned, in from two to eight days.

The processes of combustion and respiration were not understood at the time. Priestley seems to treat them as quite distinct, and again goes on to flatter himself that he has "hit upon one of the methods employed by Nature" for the restoration of air vitiated by *animal respiration*.

That some great operation was continually being carried on in Nature for the restoration of air rendered noxious by breathing was obvious, and Priestley, like many others, had tried various experiments in order to elucidate the matter—absorption by the earth, sea, and fresh-water, condensation by pressure, action of various gases, *e.g.*,

sulphur dioxide, as well as the influence of light and heat. All these seemed to yield no definite results, but ultimately the action of plants was found to be the chief cause. Mice were allowed to die in a closed glass vessel, and the confined air was then divided into two parts, and enclosed in glass vessels placed upside down over water, so that no air could enter or escape. Into one, sprigs of mint were introduced, and after eight days a mouse placed in it lived perfectly well, whereas, in the other vessel not so treated, mice died almost instantly.

By many experiments extending over two or three years, the reversal of the effects of animal respiration by plants was established and also the discovery made that green aquatic plants placed in sunlight in water containing "fixed air" (carbon dioxide), give off "dephlogisticated air" (oxygen). When the evolution of oxygen stopped, the introduction of fresh-water was followed by a brisk development of more bubbles, thus supporting the view that the oxygen came from the "fixed air" dissolved in the water, and not from the leaf.

Ingenhousz, in 1779, confirmed and extended Priestley's observations. The latter had suggested that the main purifying effects upon the air were due to the mere growth of the plant, but Ingenhousz conclusively proved that the influence of the sun's light upon the plant was an essential factor; in darkness, although kept at the same temperature as during the daytime, no purification took place, but rather the reverse.

Senebier, working independently about the same time as Ingenhousz, went over the same ground and laid stress upon the fact that only the green leaves give off oxygen in sunlight, and also stated more precisely that the amount of oxygen evolved was dependent upon the amount of carbon dioxide present in the water used, distilled water yielding practically no gas at all.

The observations, so far, had been chiefly qualitative in character, and it was not until 1804 that De Saussure's quantitative investigations were published.

By this time Lavoisier's famous researches had given an intelligible account of the processes of combustion and respiration—the composition of carbon dioxide was ascertained and the part played by oxygen in producing it was fully understood. The more extended use of the balance had also asserted the indestructibility of matter, and under the new *régime* progress became possible in all branches of science.

De Saussure, taking advantage of the facts and spirit of this new chemistry, introduced accuracy and terse completeness into his work. Various species of plants were placed in air of known volume and composition, and after exposure to sunlight for thirty-six hours the air was examined and found to have been altered in composition. The volume remained the same, but the carbon dioxide had disappeared and a nearly equal amount of oxygen had been substituted for it.

He also established the absolute necessity of carbon dioxide for plant growth, pointed out its injurious nature when present in more than comparatively small amounts, and, finally, made the most important discovery that, while the decomposition of carbon dioxide and exhalation of oxygen are going on in sunlight, the plant is increasing considerably in dry weight. This increase is, moreover, greater than can be accounted for by the fixation of carbon alone, the excess being due to the simultaneous fixation of the elements of water—hydrogen and oxygen—supplied from the ground through the roots.

With all this accumulated knowledge, it is somewhat difficult to understand why chemists and plant physiologists did not at once begin to inquire into the nature and composition of the product first formed in green leaves as a result of the processes described. The whole question of carbon-assimilation by leaves had been well opened, but the experiments were regarded by many of the chief workers in this field as having not even the remotest connection with nutrition of plants, and were consequently cast aside, or at least neglected and almost forgotten. This was largely due to preconceived notions respecting plant food, and the state of chemical knowledge at the time was such as to check progress. The early Aristotelian teaching—that the sole food-substance of all plants was "humus"—prevailed, and chemists were almost compelled to adopt this view because of their conceptions regarding the formation of organic compounds. Animal, vegetable, and mineral chemistry had been found to be an inconsistent classification, as some compounds belonging to the first two groups were found to be identical; but a more disastrous division into organic and inorganic chemistry took place, the latter dealing with elements under the influence of physical and chemical forces as at present understood, whereas into the former was introduced a "vital force" which helped to get over difficulties otherwise not amenable to treatment, and at the same time made attempt at a reasonable explanation somewhat superfluous. It was dogmatically asserted that substances formed under the influence of the vital principle could not be produced artificially, and, unfortunately, Wöhler's synthesis of urea in 1828 was incomplete, and remained for a long time the only case tending to support an opposite opinion. The existence of such a force, which was even capable of the creation of plant-ash out of nothing, made careful observation unnecessary, and it was not until after the complete destruction of the humus-theory of plant nutrition by Liebig in 1840, and Boussingault a little later, that the mind was free to look at the significance of the work of the early experimenters.

From this period down to the present time the assimilation or preparation by plants of organic compounds from simple substances, such as carbon dioxide, water, and ammonia, supplied to them from the surrounding atmosphere and soil, has been the subject of numerous and elaborate investigations. The recognition of the decomposition of carbon dioxide with elimination of oxygen by

green parts of plants as an assimilatory process, dates from the beginning of the century; the essential importance of light was also recognised by the early workers, but it is to Sachs that we owe the discovery of the connection between this carbon-assimilation and the production of a definite substance—starch—in the chloroplasts of the leaf.

The occurrence of starch granules in the chloroplasts was made known by von Mohl in 1837, but Sachs, in 1862, observed that it was induced by the same conditions as those known to be necessary for the decomposition of carbon dioxide by plants, namely, exposure to light, and his memoirs of 1862–4 deal with the influence of light on starch production in the chloroplasts. He shows that where carbon dioxide is being absorbed there is a constant production of starch when the plant is exposed to sufficiently bright daylight, a subsequent disappearance and removal of this starch from the leaf at night, and for the first time explains the connection between carbon-assimilation and starch production by the leaves. Godlewski, in 1873, strengthened Sachs' conclusions by the demonstration that no starch-grains are formed in the leaf-chloroplasts when carbon dioxide is missing from the surrounding atmosphere, even when the plant is exposed to bright light.

When decomposition of carbon dioxide and exhalation of oxygen is going on, starch in nearly all cases makes its appearance in the green chloroplasts of the leaf, yet Sachs' work does not show that starch is the immediate and direct product of this assimilation, although the fact that the volume of oxygen given off equals the volume of carbon dioxide absorbed was at first taken as a support for this hypothesis. That a complicated compound, such as starch, should be actually formed directly from carbon dioxide and water was highly improbable, and since Sachs' first papers there has accumulated a large amount of evidence pointing to the formation of some much simpler carbohydrates, which are transformed into starch by the chloroplast, probably only when they are in excess of the immediate requirements of the cell for its own nourishment and respiration—that starch is, in fact, always a reserve material and is only the first visible product in a large series beginning with carbon dioxide and water.

Böhm (1877) showed that starch can be formed in the chloroplasts from materials which have been elaborated elsewhere than in the cell in which starch-production is going on, and Schimper (1880) paved the way for further research in this direction by his discovery of the starch-forming corpuscles or amyloplasts in parts of plants not green (tubers, stems, &c.) and their morphological identity with chloroplasts. These bodies are colourless and are concerned with the formation of starch out of ready-formed sugars supplied to them, and can be changed into chloroplasts by exposure to light in many cases.

Schimper's conclusion that the chloroplasts of the leaf parenchyma alone have the power to produce starch except by assimilation from

the atmosphere was shown to be incorrect by Meyer (1885–6), who found that starch is readily formed by the mesophyll chloroplasts when the leaves are floated on solutions of dextrose, levulose, cane-sugar, galactose, and even mannite and glycerine.

Baeyer's suggestion that the first formed product of the carbon assimilation is formaldehyde—a compound which undergoes easy condensation into sugar-like substances—has been considerably supported by recent work on the sugars, notably by Emil Fischer's syntheses of dextrose and levulose, although attempts to feed chloroplasts with aldehyde or its derivatives have met with only partial success.

The most recent and at the same time one of the most important contributions ever made to the study of plant-metabolism, is that of Messrs. Brown and Morris, communicated to the Chemical Society in April last. In their paper they deal with the occurrence of starch, diastase, and sugars in leaves, and the work will undoubtedly take high rank in the annals of plant physiology.

Sachs, in 1884, endeavoured to determine the weight of starch produced during the day by estimating the dry weight of two definite equal areas of the leaf, taken symmetrically from opposite sides of the midrib, one in the morning and the other in the evening after the day's exposure to sunshine. The assumption was made that the differences in dry weight were due to gain in *starch*, and the results showed in the case of a sunflower an increase of 9·14 grains per square metre in ten hours. Similar experiments carried out on plucked leaves with their petioles immersed in water yielded greater increase in weight, owing to the dissolution products of starch not being able to flow into the stem under these circumstances.

Messrs. Brown and Morris point out that this method merely indicates the *total assimilated products* which are manufactured under the given circumstances, and not necessarily the amount of *starch* produced. Sachs thought otherwise, as he assumed that all the assimilated products of the leaf must at one time or other pass through the form of starch in the chloroplasts, an assumption for which there is no evidence. These observers bear testimony to the applicability and accuracy of Sachs' dry weight method of estimating the rate of assimilation, and go on to determine the amount of starch which is present at any one time in the leaf, and the proportion which it bears to the total products of assimilation.

Leaves were rapidly dried at 75–80° C., or killed by the action of chloroform vapour, and then finely powdered and the fats and chlorophyll extracted with ether. Ten grams of the powdered leaves were then taken and extracted with alcohol; the starch in the powder was then gelatinised by hot water, and finally determined by estimating of the maltose and dextrin produced after the action of diastase upon it for about two hours.

Two different batches of Tropæolum leaves picked after the same amount of isolation were found to contain 6·408 and 6·545 per cent. of starch respectively, results which are representative of the accuracy which can be attained by their method.

Estimation of the starch in the leaves of sunflower while attached to the stem, at five a.m. and again at five p.m. on a fairly bright day, showed an increase of 1·4 grams of starch per square metre during this period, while the total assimilation-products actually formed during this time, estimated from the dry weight of similar leaves separated from the stem and their petioles immersed in water, was over twelve grams per square metre. It is here seen that if Sachs' view is correct—that all the assimilated materials pass through the form of starch—the formation of the latter substance and its dissolution must be carried on at an exceedingly quick rate. It is, however, far more probable that much of the assimilated material passes out of the leaf into the stem of the plant without ever being organised into starch by the chloroplasts.

Messrs. Brown and Morris go on to discuss the occurrence and periodic variation in the amount of diastase present in leaves, and in this direction they have achieved much success. Incidentally, it may be observed that the whole of their work arose out of the discovery of diastase in dry hops when they were prosecuting an enquiry into the cause of secondary or "cask fermentation" in beer.

The function of the ferment diastase in the breaking down of starch into the soluble substances sugar and dextrin when seeds are allowed to germinate, has been recognised almost from the time of its discovery and isolation by Kirchoff, Payen and Persoz, and Dubrunfaut at the beginning of this century, and a study of this dissolution of starch led to the generally expressed belief that the same or a similar ferment is concerned with the disappearance of starch from the leaf chloroplasts.

Attempts, however, to isolate diastase from green leaves were not made until about 1877, when Kosmann and Baranetzky took up the work. Small traces were found in the leaves of various species of plants by both observers, and Baranetzky expressed the opinion that the smallness of the amount present at any one time was to be attributed to its being used up as fast as it is produced.

Brasse, in 1884, also conclusively proved its presence in leaves, though Sachs in the same year, in his paper on assimilation, was unable to decide whether the dissolution of starch in the leaf is due to some special power of the chloroplast, or to soluble ferments.

Wortmann, in 1880, from experiments of the most questionable character, concluded that diastase is missing from practically all leaves, and even when present plays no part in starch dissolution, the process being attributed by him to the action of the living contents of the cell.

Messrs. Brown and Morris find, however, that, instead of leaves

possessing little or no diastase, there is always present more of this ferment than is sufficient to transform all the starch they can ever contain at one time, and in some cases the amount is such as would transform a quantity of starch more than equal to the whole dry weight of the leaf itself.

The conflicting results previously obtained respecting this subject are almost entirely due to the fact that although diastase is soluble in water, it cannot readily be extracted from the leaf, on account of the tenacity with which it is held by the protoplasm; the presence of considerable amounts of tannin also renders it insoluble. However, by specially drying the leaf, and using the powdered tissue itself, it is possible to demonstrate the presence of leaf diastase in all cases, and these experiments give proof of its identity with the malt ferment; the production of the sugar maltose and dextrin from soluble starch is brought about by both alike.

The diastatic activity of the leaves of various species of plants was compared, the results being very remarkable. The leaves of leguminous plants stand far ahead of all others in this respect, the garden pea heading the list. The relative diastatic activities are stated in the amount in grams of maltose, which 10 grams of the air-dried leaf will produce from soluble starch in 48 hours at 30° C. Twenty-six species were examined, and the following are typical results:—

Pea (*Pisum sativum*)	240·30
Scarlet Runner (*Phascolus multiflorus*)	110·49
Clover (*Trifolium pratense*)	89·66
Vetch (*Vicia sativa*)	79·55
Potato (*Solanum tuberosum*)	8·16
Sunflower (*Helianthus annuus*)	3·94
Onion (*Allium cepa*)	3·76
Cotyledon umbilicus	4·61
Funkia sinensis	5·91
Hymenophyllum demissum	4·20

The work points to a connection between starch-forming power and the diastatic power of leaves, but must be extended before any general statement can be given.

Changes in diastatic power of different portions of the same leaf at different times of the day prove that the ferment increases as fast as the leaf is being depleted of its starch and other assimilated products. Considerable accumulation takes place in the dark. They believe that the secretion of diastase is only carried on by the protoplasm when the soluble nutriment of the cell has been used up or carried out into the stem. At this starvation point diastase is secreted in order to bring about the supply of soluble food-materia from the reserve starch in the chloroplast.

The last part of their memoir is concerned with the sugars of the leaf, and this part of the work indirectly supports the unexpected view that cane-sugar is the first sugar to be synthesised in the assimi-

latory process, and at the same time establishes almost beyond doubt that the starch dissolution in the leaf is mainly brought about by the action of the diastase which they have proved is so generally present.

It is in the determination of the nature and variation in the amount of the different sugars present in the leaves of plants that Messrs. Brown and Morris have laid the foundation of a more complete and extended knowledge of the complex chemical changes occurring in plant carbon-assimilation, and their work may be characterised as the first which has placed the subject on more than a hypothetical basis.

When diastase acts upon starch the result is the formation of a sugar—maltose—and dextrins, which latter on further action are also changed into maltose. If, therefore, this ferment is concerned in the dissolution of starch in leaves it ought to be possible to prove the existence of maltose in these organs. This they have satisfactorily accomplished by processes which are too technical to be introduced here, but about which there can be no doubt. The only other sugars found to be present in Tropæolum leaves were cane-sugar, dextrose, and levulose. Simpler pentoses were looked for but none discovered. Experiments were afterwards directed towards the quantitative determination of these sugars in the leaves at different times of the day and night, in the hope that some light would be thrown on their function, translocation, and genetic relation to each other, and to the carbohydrate starch, and to ascertain something about the sugars which stand between the first products of assimilation and starch and those which are to be looked upon as derived from the latter by chemical transformation.

A considerable number of leaves of Tropæolum were picked at 5 a.m. and separated into two batches. One set (*a*) was dried at once and the carbohydrates estimated, and the leaves of the other set (*b*) were placed with their petioles in water and exposed to sunlight until 5 p.m., and then a similar series of leaves (*c*) was picked after exposure to sunlight during the same period, but still attached to the plant. The starch and sugars of these three samples were determined with the following results :—

	a	*b*	*c*
Starch	1·23	3·91	4·59
Sugars—			
Cane-sugar	4·65	8·85	3·86
Dextrose	0·97	1·20	0·00
Levulose	2·99	6·44	0·39
Maltose	1·18	0·69	5·33
Total sugar percentage on dry leaf	9·79	17·18	9·58

From (*b*) and (*c*) it is seen that the production of starch in cut leaves does not go on so rapidly as in leaves which are allowed to remain attached to the plant.

Sachs first noticed that the dry weight of a plucked leaf increases considerably when isolated and its petiole placed in water, and this increase is seen to be due mainly to the accumulation of sugars, not starch. The total sugars in (*b*) have been nearly doubled, and this is mainly due to the increase in cane-sugar, a result which militates against the generally received opinion that a glucose is the principal "up-grade" sugar first formed by the leaf.

The changes which the sugars undergo when assimilation is prevented are well brought out by the following experiment. A considerable number of leaves were plucked in the afternoon of a fairly sunny day, and dried immediately (*a*). Another similar set (*b*) were taken and placed in the dark for twenty-four hours, their stems being dipped in water. The result of the analysis gave :—

	(*a*)	(*b*)
Starch	3·693	2·980
Sugars—		
Cane sugar	9·98	3·49
Dextrose	0·00	0·58
Levulose	1·41	3·46
Maltose	2·25	1·86
Total sugar percentage in dry leaf	13·64	9·39

Total loss of sugars and starch in (*b*) = 4·96 per cent.

The loss or destruction of starch and cane-sugar is due to respiration. The disappearance of the cane-sugar has been accompanied by increase in levulose. Seeing that the conditions of the experiment preclude the possibility of formation of levulose by assimilation, this fact points to the conclusion that the cane-sugar is first inverted, and the resulting dextrose and levulose are used up at uneven rates by the respiratory processes, the former, together with a little maltose, contributing chiefly for this purpose. The experiment, at the same time, renders it almost certain that the increase of levulose in (*b*), previously described, is derived from cane-sugar inversion, and not from direct assimilation.

Looked at from all points of view, Messrs. Brown and Morris's results undoubtedly point to the conclusion, so far as Tropæolum is concerned, that cane-sugar is the first sugar to be synthesised as a result of the carbon-assimilation by the leaf, and that only after a certain concentration of this substance in the cell-sap is starch formed as a more stable reserve material. In the leaf, cane-sugar suffers inversion, and ultimately passes from cell to cell and out into the stem as dextrose and levulose, the starch being at the same time translocated as maltose.

The whole work is full of suggestion, and is a model of consummate skill and accuracy. Plant physiologists have reason to be grateful for the improved methods of research devised by these two workers, and the foundation which has been laid for further investigation, as well as for the results obtained. Taken in conjunction with the extraordinary haziness manifested in the chemistry of

even modern plant physiology, the work tends to emphasise the necessity that those who would further progress in this department of biology must first be chemists.

Considering the great labour and patience which these researches must have involved, it may, perhaps, seem ungrateful to express regret that the function of chlorophyll meets with no consideration. Notwithstanding the researches of Engelmann, Pringsheim, Timiriazeff, and many others, this part of the subject is little understood, and now that the morphological and physiological identity of the coloured chloroplast and colourless amyloplast is completely established so far as starch-production from already assimilated materials is concerned, the quite separate function of constructing a proto-carbohydrate from carbon dioxide and water by the former, due to its possession of chlorophyll, is of especial interest.

J. PERCIVAL.

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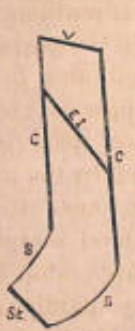
II.

The Respiration of Birds.

JOHN HUNTER, I believe, held that birds did not draw breath during flight, but merely used the air stored in the sacs. Extraordinary as this view may seem, he was led to adopt it by what is a very real difficulty, namely, that the movement of the breast in breathing would seriously derange the machinery of flight. Mr. Edmond Alix, in his *Essai sur l'appareil locomoteur des Oiseaux*, gives what I believe is the right solution of the problem, viz., that the bird during flight moves its back, not its breast, up and down in the act of breathing; but he does not explain how this is effected. My own investigations have led me to the conclusion that the muscular movements necessary to flight themselves to a great extent bring about the enlargement and contraction of the thoracic cavity without which respiration cannot take place. The muscle called the latissimus dorsi, which arises from the vertebræ, and, after passing over the scapula, is inserted in the humerus, raises the backbone during the down stroke of the wing, bringing the bird's body into the position which is required for horizontal flight; and not only is the backbone raised and brought to, or nearly to, the horizontal, but at the same time the ribs are straightened, and this causes an expansion of the air-sacs. The effect of this raising of the back may be seen if a bird is suspended by its backbone, when the weight of the sternum and the breast muscles hanging upon the ribs causes them to straighten out at the junction of their costal and sternal portions. The muscles, also, which pass downwards to the ribs from the posterior ends of the scapulæ, will help towards the same result, and, besides that, will broaden the roof of the thoracic cage, the upper part of the rib tending to become more horizontal when the lower part is raised. The action of these last-named muscles is assisted by the movement of the scapula. It will be found that during the down stroke, when the front margin of the wing is lower than the hind margin, so that the undersurface of the wing looks downwards and backwards, the anterior end of the scapula is depressed very slightly, and this causes a considerable raising of the posterior end. Thus in birds, as in many crustaceans, progression itself aids greatly the process of respiration. Another point worth noticing is that in this the great pectoral plays an important part, for it is due to the great strength of this muscle of

that the contraction of the latissimus dorsi causes a movement of the body and not of the wing. All possible work is put upon the pectoral muscles, so that the centre of gravity may be low down, a point of great importance in a flying-machine; but though the flight muscles help so largely towards respiration, of course much is done by what are commonly spoken of as the respiratory muscles. The levatores costarum, which I have found very highly developed in the domestic pigeon, arising from the vertebræ, passing backwards and attaching to the ribs some way down, tend to make the upper part of the rib horizontal, thus broadening the chamber beneath. The triangularis sterni, which arises from the inside of the sternum, from its anterior lateral end, and attaches to the sternal ribs, works towards the same object from below. The action of the external intercostals is not so easy to understand. In man they raise both the ribs, which they connect, and at the same time raise the breastbone; but in birds the pectoral muscles render the sternum heavy, and, what is far more important than this, the weight of the body hanging upon the wings during flight, and the pressure of the wings inwards upon the coracoid bones and clavicles, must make the breastbone practically immovable. What, then, will happen when the external intercostals contract? It must be remembered that the contraction of a muscle tends to shorten the distance between its two ends. The muscular band, passing obliquely back and downwards, will lower the front rib to which it is attached, and raise the hinder; the loose muscular connection of the vertebral column with the shoulder blades allowing some depression of its anterior and a corresponding raising of its posterior part. Moreover, the backbone of most birds that I have examined bends downward easily, and through a considerable arc just anterior to the ilium. The raising of the hindmost ribs which articulate with the vertebræ behind the point where the bend takes place, will aid the vertebral muscles in straigthening the back. The accompanying diagram illustrates the working of the external intercostals.



c. = Costal rib.
e.i. = External intercostal muscle.
s. = Sternal rib.
st. = Sternum.
v. = Vertebral column.

The contraction of the muscle will lower the anterior and raise the posterior rib.

Wishing to test these conclusions by experiment, I suspended a freshly-killed pigeon by its wings, and inflated the air-sacs by means of a blowing tube. The backbone, a little in front of the thigh

joint, moved rather more than half-an-inch—the movement of the sternum being almost too slight to measure. I do not wish to represent this experiment as one of much value. However, the conditions of flight were so far reproduced that the weight of the body was hanging upon the wings, and so hindering the movement of the breast, while leaving the back free; but there was none of the pressure—which during flight must be very great—of the wings upon the coracoid bones and clavicles. But would not the only effect of this pressure be to render the sternum and the bones united with it still less ready to move?

It will be observed that I have said nothing upon the vexed question of the action of the internal intercostals. They are so weak that the result of their action, whatever it may be, cannot be a large factor in the problem of respiration. When a bird is standing or running, it is obvious that the back is fixed to the legs, so that it cannot rise and fall. Clearly, then, the process of respiration must be different, and if a bird be watched when it is filling and emptying its air-chambers in order to utter a loud note, the moving forward of the breast may be easily seen. As I have said, the main obstacle to its motion during flight is, not its weight, but the tension and pressure at the shoulder joint. With regard to the abdominal air-sacs, the raising of the hinder-quarters by the legs, when the bird is standing, will itself expand them. The air will be expelled by the contraction of the abdominal muscles, which will draw the sternum inwards. In connection with this, I may mention that Mr. F. E. Beddard has found the oblique septum muscular in the puffin, and I myself have confirmed the observation. When a bird is sleeping with its breast resting upon the perch, or lying upon the ground with nearly all its weight upon its breast, it must adopt a method of breathing similar to that employed in flight. Often geese and other birds, while lying on their breasts, will exert a great deal of voice-power, and then the rising of the hinder part of the back is distinctly visible. The fore part may appear to rise a little, but this appearance is due, I think, to the raising of the ribs by the levatores costarum. The legs, apparently, have no weight upon them, so that the back is not held down as in standing. The muscles called into play must be the external intercostals and the triangularis sterni.

F. W. HEADLEY.

III.

Observations on Certain Marine Animals.

I.—A New Protective Device of *Maia squinado*.
II.—Colour Assimilation among Fishes.
III.—A Fighting Stratagem of the Crawfish (*Palinurus*).
IV.—Abnormalities in *Haliclystus* (*Lucernaria*).

I.—A New Protective Device of *Maia squinado*.

I HAD occasion recently to watch closely the habits of the large Spider Crab, *Maia squinado*, while in captivity in the Aquarium tanks of the Jersey Biological Station. The first individual obtained, a female, was temporarily placed in a large bare tank, unfurnished with either boulders or sea-weeds. A thin layer of fine shingle about an inch in depth covered the bottom. *Maia* for a while was very restless, seeking, apparently, for a suitable place of concealment. Finding none, she at length took up a position in the centre of the tank, scratching a hollow in the shingle wherein to lie. After a short interval of rest she began an admirable plan of concealment. First with one chela and then with the other the crab selected and lifted carefully pebble after pebble and deliberately placed them, closely fitted together, upon the surface of the carapace, the rough spinous nature of this being admirably suited to hold them in position. Rarely a pebble would fall off, so carefully did *Maia* poise them and so steady did she hold her body. At the end of a quarter of an hour the red spiny back was converted into a little mound of pebbles, and the crab absolutely hidden.

Now *Maia*, in common with several allied species, such as *Pisa tetraodon*, *Achæus cranchii* (Spence Bate *Ann. and Mag. Nat. Hist.*, 1866), and *Stenorhynchus*, etc., has the habit of decorating and trimming the carapace and limbs with scraps of weed hooked often quite artistically on to the recurved bristles that beset these parts. This with obvious protective intention. But *Maia* at home seems never to frequent pebbly localities. Its habitat is in the sand and under the weedy, root-matted edges of the *Zostera* banks. Hence it is reasonably to be inferred that the protective device described above was independently thought out to meet the special exigencies of the occasion. Several other *Maia* subsequently placed in the same tank performed precisely the like operation.

It is noteworthy that all these were either females or immature males; the adult males are normally very large, usually at least twice the size of females, and are provided with long and powerful chelæ, which make them independent of the subterfuges adopted by the weak small females. An interesting point, too, is that the weak chelæ of these mimics are quite smooth, without the recurved bristles of the other limbs, and hence are unlike them, never having any weedy trimmings.

II.—Colour Assimilation Among Fishes.

We already know a good deal about this, especially among the flat-fishes, so the following instances but go to swell an already long list. Still, these are so striking as to be worthy of permanent record.

Two tanks were used for experiment, one with dark background and bottom, well shadowed; the other bright, with a white mottled sand bottom. Several of the marine stickleback (*Gasterosteus spinachia*) were placed in each. To sum up the result briefly, those in the dark shadowed tank remained practically unchanged in colour, but those in the light-coloured tank had in greater or less degree lost their brightness and intensity of colouring. The beautiful gold bronze lustre so characteristic of these sticklebacks was lost, and the backs were mottled black and white, contrasting strangely with the nearly unbroken yellowish black of the dorsal surface of their friends in the dark tank.

In the dark tank had also been placed a number of Wrasses (Labridæ), and these showed fading all round, most marked in the bright greens and scarlets. As these colours are usually in combination with brownish marking, the fading of the bright hues meant a close approximation to the brown appearance of the bare conglomerate forming the rockwork of the tank. One fish especially beautiful at first—of a most brilliant scarlet and brown—faded to a dirty combination of pale olive green and brown, scarcely recognisable had the fish not been marked in a distinctive manner at the beginning of the experiment. The whole of these colour changes were effected within the remarkably short period of a week.

It may be that these instances of colour assimilation carry the key to the problem of colour variation or rather mimicry in the prawn *Hippolyte* (*Virbius*) *varians*.

Plaice (*Pleuronectes platessa*) have also shown rapidity of colour-change much more marked than I was prepared for. Some that were placed in a large shallow tidal pond where the colour of the bottom varies considerably and where a portion is often in deep shadow, show change from a uniform grey to a well-marked and intensely dark blotched appearance within a few seconds. Indeed, it is quite chameleon-like, so quickly is the transformation effected. In ordinary tanks where the light and the colour of the sand are stable, the plaice soon take the exact colouring requisite, and retain it without alteration so long as they remain in the particular tank.

III.—A Fighting Stratagem of *Palinurus*.

The Common Crawfish (*Palinurus vulgaris*) has many points of interest, and not the least curious is his plan of combat when matched with a powerful antagonist. Without chelate limbs, he seems weak and defenceless. One is at first inclined to commiserate this apparent want of means alike of offence or defence, especially in comparison with his kindred, the lobsters, armed so well with powerful seizing chelæ. That he had means of defence seemed probable; but it is only within the last few days that this was satisfactorily demonstrated. Without any particular intention in view, we had dropped a medium-sized lobster into the tank containing two large *Palinurus*. At first no sign was given, but in a little while we were attracted by a loud noise as of a skirmish, and had an inimitable object-lesson in Crawfish warfare. The larger of the two Crawfish apparently resented the intrusion of the lobster, and was determined upon ejection. There was a good deal of preliminary sparring, but the fight which promised to be protracted ended suddenly in a most unexpected manner. Making a sudden twist, the Crawfish got above the lobster crosswise, and suddenly snapping his powerful tail, jammed the body of his antagonist in the fold, thus impaling him on the sharp downward spikes of the pleura that are such conspicuous objects in a side view of *Palinurus*. The lobster was put quite *hors de combat*, for his body was terribly mutilated by the sharp spines, which had pierced his armour as though it were tissue-paper. Besides this instance, cases are known where persons, incautiously handling the Crawfish, have received wounds on the arm inflicted by similar sudden flap of the tail.

IV.—Abnormalities in *Haliclystus* (*Lucernaria*) *octoradiatus*.

(Preliminary note.)

This year, having occasion to examine a very large number of the Lucernarian most common in these seas, viz., *H. octoradiatus*, I was surprised to find a very large proportion to possess more or less strongly-marked abnormal features. These were divisible into two groups, the first where the colleto-cystophores were malformed, the second where these organs or the grouped tentacles were above or below the normal number eight.

The specimens gathered were in two lots, the one taken in February and March comprising large adult individuals $\frac{3}{4}$ to 1 in. in diameter, the other collected in the beginning of June, and made up of half-grown individuals, averaging half-an-inch across, but in which the ova, etc., were well advanced.

The malformed colleto-cystophores showed several interesting points, but the only one I wish to dwell upon now was a common case, where the summit of the organ showed very obvious homology with the ordinary capitate tentacles. In many the resemblance was

but faint—a slight rounded swelling at the apex—with presence of a few of the ordinary nematocysts of the tentacles. From this stage there were all gradations up to a few rare instances where the apex of the c.-cystophore bore a perfect capitate tentacle precisely the same in structure to the normal tentacle, saving that the stalk was extremely short, just equal in length to the diameter of the head. All the examples of this variation were furnished by the lot gathered in June. The proportion was surprising. Out of 118 specimens examined but 40 were without thi abnormality in the c.-cystophores.

14	showed	one	colleto-cystophore	with	tentacle-crowned	apex.
15	,,	two	,,	,,	,,	,,
15	,,	three	,,	,,	,,	,,
9	,,	four	,,	,,	,,	,,
8	,,	five	,,	,,	,,	,,
5	,,	six	,,	,,	,,	,,
4	,,	seven	,,	,,	,,	,,
8	,,	all the	,,	,,	,,	,,

Otherwise these specimens showed but little mutability. Two colleto-cystophores were doubled and one specimen had an extra group of tentacles, while another lacked one.

In the larger specimens taken earlier in the year, the tentacle-crowned c.-cystophore phase was not observed, but there was extreme want of symmetry. Quite 33 per cent. showed variation in the number of colleto-cystophores or in the groups of tentacles. The greater number (more than half) were cases where the marginal organ was wanting in the space where normally it would be found; the others were instances of excess number of tentacle groups (accompanied usually by the presence of an equal number of excess colleto-cystophores) or of duplication—"twinning"—of one or two of the last-named bodies.

The conclusions to be drawn from the last mentioned abnormalities are obscure, but those where the colleto-cystophores bear the papillate appendix at the summit are certainly atavistic, explainable by the original derivation of these organs from ordinary papillæ.

JAMES HORNELL.

IV.

Recent Progress in Conchology.

SOME recently-published conchological treatises seem deserving of somewhat more than a mere passing notice in these pages, since they contain matter which deals with either the whole, or a large section, of the molluscan phylum.

To begin with, there is an interesting paper from the pen of M. Moynier de Villepoix (1) that furnishes an admirable summary of what is known concerning the formation and growth of the molluscan shell. It is not, however, confined to a summary, and among the new points which its author claims to have established is the mode of formation of the ligament in bivalves. This, it appears, is secreted by the epithelium of the dorsal suture, each of its constituent elements being formed by cells specialised for the purpose. With regard to his observations on the growth of the shell in *Helix*, we greatly fear that those relating to the special areas of secretion of the mantle, with the description of their cells, will have to rank as confirmatory only and not as discoveries, for if we mistake not these structures have already been dealt with some years ago by MM. Longe and Mer (2), the title of whose paper we miss from the valuable bibliography at the end of the memoir. Two other references seem also to have escaped the vigilance of the writer, namely Gray's principal and important paper on shell-structure (3), and the source of that naturalist's inspiration (albeit unacknowledged), Count Bournon's work on Calcite (4).

The next memoir to which we wish to allude is that by M. H. Fischer (5), a coming writer on molluscan anatomy and already noted for the careful nature of his work, who deals in a comprehensive manner with the morphology of the liver in the Gastropoda.

His principal results may be briefly summarised as follows:—In the Scutibranchs (*e.g.*, *Neritina*) the hepatic lobes of the embryo are equal and symmetrical. One genus only of the Prosobranchs, *Valvata*, which also resembles the Scutibranchs in possessing a bipectinate gill, displays the same regularity. In all other Gastropods the two lobes are unequal, either *ab initio* or soon after their formation, the left lobe being the larger. It sometimes happens in the adult (*Elysia*, *Arion*, *Buccinum*) that the balance is restored, but this acquired symmetry of a later stage has not the same morphological value that attaches to a like equality in the initial stages of the development of

that organ. The liver in Prosobranchs undergoes a process of evolution comparable to that of the other organs; but the sub-divisions that could be established for the Pectinibranchiata on its variations would not entirely correspond with those founded on other characters, such as the radula, nervous system, kidney, or shell. Hence the difficulty of founding a classification on the sum of the characters, and the desirability—M. Fischer considers, since the liver is not suitable for the purpose—of basing a systematic arrangement on some single feature, such, for example, as the radula.

The classification of the Mollusca, especially from a phylogenetic aspect, has received of late very much attention, and quite a forest of genealogical trees have been constructed for this sub-kingdom, or portions thereof, by Von Jhering and others, to the great bewilderment of the unlucky student who attempts to thread their maze.

The most recent complete scheme is probably (there may be a later by the time this appears in print) that by Dr. P. Pelseneer (6), and it is a matter for regret, that while the privileged few have had author's copies for some months past, so that his table has been even incorporated in Simroth's new edition of the molluscan portion of Bronn's great work (7), the volume from which it was supposed to be taken has but just been made available for the many.

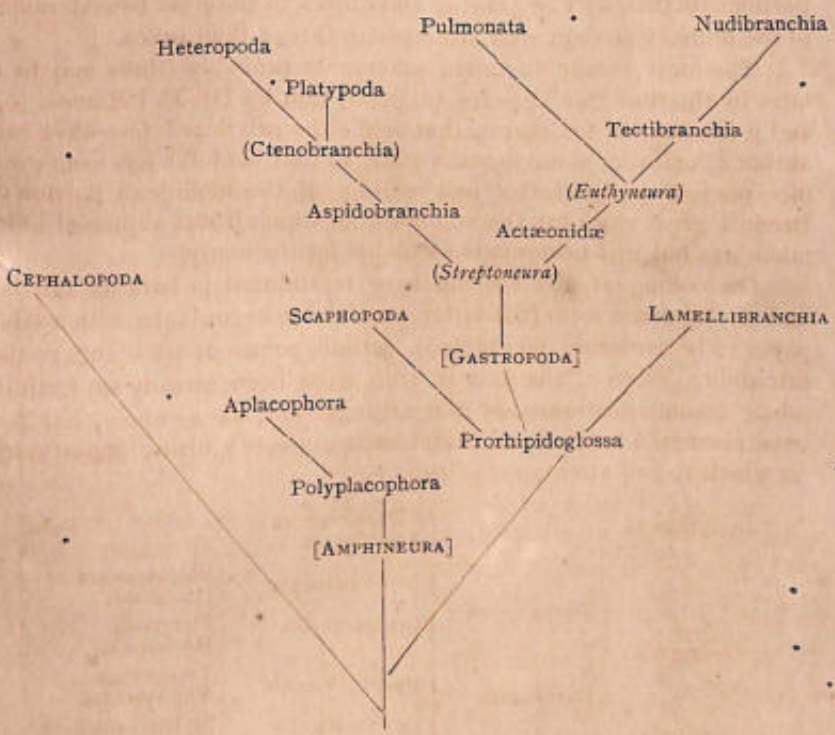
On looking at the scheme here reproduced in both its tabular and phylogenetic form (the latter modified in accordance with a later paper to be presently mentioned), certain points at once engage the attention: most of them, it is true, have been already set forth in minor communications, but the arrangement, as a whole, has not been presented before, and therefore it appears a fitting opportunity on which to call attention to them:—

Amphineura			Polyplacophora, Aplacophora
Gastropoda	Streptoneura	Aspidobranchia	Rhipidoglossa, Docoglossa
		Ctenobranchia	Platypoda, Heteropoda
	Euthyneura	Opisthobranchia	Tectibranchia, Nudibranchia
		Pulmonata	Stylommatophora, Basommatophora
Scaphopoda			Solenoconcha
Lamellibranchia			Protobranchia, Filibranchia, Pseudolamellibranchia, Eulamellibranchia, Septibranchia
Cephalopoda			Dibranchia, Tetrabranchia

In the first place, the Pteropoda as a class have disappeared, being merged with the Opisthobranchiata. They were originally classed as allies of this last-named order by De Blainville (8 and 9), who then used the term "Pteropoda" for certain of the Nucleobranchs

and *Argonauta*, whose shell in those days was not thought to be the product of the animal that occupied it. This arrangement, however, was never really adopted by conchologists, and it was not until 1886 that Boas (10) pointed out, on anatomical grounds, that the Pteropoda could not be separated from the Opisthobranchiata.

As one of the results of his researches in connection with the preparation of the "Challenger" Reports on the Pteropoda (11) Pelseneer was led to acknowledge the correctness of this conclusion, and it has been subsequently accepted by Professor Lankester (12) and adopted by Lang in his Lehrbuch (13). The latter authority, also, follows Pelseneer in his classification of the Pelecypoda (14), or Lamellibranchiata, to use a later and less preferable term, to which we note with



regret that our author reverts. These last he derives, as before, with the Gastropoda from a common stock—the hypothetical Prorhipidoglossa, but adds now as another branch from the same—the Scaphopoda. In this he is in accord with Plate (15), who, however, places their branch a little more directly on the main stem, between the off-shooting of those of the Pelecypoda and *Patella*. In a subsequent note on the genus *Actæon* (16), Pelseneer, agreeing with Bouvier (17), slightly modifies his scheme, and considers that *Actæon* is not only the common stock whence are derived the Pteropoda, Tectibranchiata, and Pulmonata, but also the link between the Streptoneura (=Prosobranchiata) and the Euthyneura (=Opisthobranchiata and Pul-

monata); for *Actæon* is streptoneurous, that is to say, possesses a nervous system in which the loop-like visceral commissure is crossed over in the form of a figure 8, and hence shows that the Euthyneura, in which the visceral commissure forms a simple loop, were in the first instance streptoneurous like the Prosobranchiata, and that their euthyneurity, if the word may be permitted, is the result of secondary displacement of the visceral hump from left to right. This retrograde displacement may possibly have been occasioned through the hermaphroditism of the animal, since the Streptoneura are all unisexual. Among the Streptoneura the Trochoid Rhipidoglossa are nearest to *Actæon*.

Another point of importance is the direct derivation of the Cephalopoda from the primitive stock before the other branches lead off. In thus opposing the Cephalopoda to the rest of the molluscan phylum, Pelseneer agrees, as he himself is careful to point out, with Giard, who advanced this view in 1876 (18). The grounds for this theory are to be found, not only in the anatomy, but also in the high antiquity of the class.

As to the source whence the Mollusca arose, this, Dr. Pelseneer thinks, must be sought in the Polychæta Errantia, or that group of the Worms to which, among others, the gaudy sea-mouse (*Aphrodite*) belongs; and, moreover, he is of opinion that the family Eunicidæ are, among the living representatives of that sub-order, the nearest, in the *ensemble* of their organisation, to the Mollusca.

Granting this conclusion, what then of the shell? We are tempted to speculate whether the power of specially secreting a calcareous covering may not be the revival of a dormant faculty (seeing that it is present in a collateral sub-order, the Tubicolæ, or Tube-building Worms), just as the individual offspring of higher creatures will at times present characters or display capabilities that have lain dormant in their immediate progenitors—an occurrence to which the term atavism has been applied.

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B. B. WOODWARD.

V.

The Museums of Public Schools.

I.—CHARTERHOUSE.

IT is an encouraging sign that the "Museum Question" has penetrated into the majority of our Public Schools, and that a desire is felt that the "old-curiosity-shop" type of museum should give place to an organised institution of real educational value. It will be granted on all sides that education is the primary object of all and particularly of School Museums.

The problem then presents itself—How ought a Public School Museum to be arranged to the best advantage of Public School teaching? To this question a cut-and-dried answer applicable to all is impossible, for, within limits, each Museum requires to be adapted to the special characters of its locality, to the class of persons for whose instruction it exists, and to the special teaching of various subjects in the school curriculum.

Probably in all cases the objects possessed by any Museum can be arranged so as to bear reference to one of the following sciences: Ethnology, Zoology, Botany, Geology, and Mineralogy. It is almost superfluous nowadays to insist that each of these subjects should be kept distinct from one another. It is best that a separate room should be devoted to each, but since this must often be impossible in schools, at any rate let the cases contain specimens illustrative of one only of these "ologies," and let the object and purpose of each case be conspicuously apparent.

In any large school there are at least two sets of individuals whose needs must be considered in the arrangement of the Museum. Firstly, the average boy who has a healthy love of Nature, but does not know or wish to know overmuch about the details of structure, principles of classification, theoretical considerations, and so on, but is content with external form and interesting anecdotes. This variety of boy is destined to become a "country gentleman," and, as such, he has a sportsman's desire to be able to identify his captures and to know something of their habits. Such persons, be they young or old, deserve all encouragement; among their number are found a few ardent collectors willing to enrich the Museum and a few—alas! a very few—careful observers keeping an amateur naturalist's diary and

amassing a quantity of facts the record of which, though known for years by the student of the "literature of the subject," is of inestimable value in the training both of mind and eye. Secondly, the "scientific variety," *i.e.*, the boy who has deliberately chosen for himself a scientific profession and desires an elementary training in those subjects to which his after life is to be devoted. It is a lamentable fact that, in my experience, with one or two exceptions, the boy field-naturalist is not also the boy scientific. Granted, then, the existence of these two types, it is evident that what is necessary, helpful, and instructive to the latter, will not be palatable to the country gentleman in embryo; technical knowledge of any subject is to him "stinks," and as such not only is abhorrent to his nostrils, but may even disgust him to so great a degree as to make him fear contamination even in the open fields! Many a boy, aye and man too, is much interested in butterflies and moths, but has an extraordinary aversion to Lepidoptera.

It becomes, therefore, inevitable that a double arrangement of the Museum should be contrived, (1) a general natural history collection, by the aid of which boys may identify their victims; this should be arranged in such a way as to elucidate the main structure of the various groups in the animal and vegetable kingdom, and to stimulate collectors to cease to be merely such, and advance to the condition of accurate observers of the special group which they have made their hobby. This may be done by the introduction of exhibits illustrating the development and life-history of some common species, so that anyone with but little apparatus can verify each phenomenon for himself when once pointed out.

(2) A small collection of types as fully described, labelled, and explained as space will permit. In this series there should be no hesitation as to the use of technical terms, but let each term be fully explained and its derivation given, in order that the convenience and necessity of its use may be apparent. It is advisable that this series should conform to the course of class or special teaching adopted in the school, and it is undoubtedly desirable that it should, to a certain extent, follow the arrangement of some good practical text-book on the subject, so that a boy with book in hand can work through the entire series, or each section of it, prior to attempting the same section with scalpel and forceps in the laboratories.

We have not hitherto considered the needs of the general public in School Museums, nor should they be considered apart from the school with regard to arrangement. Nevertheless, it is a wise and generous regulation to admit visitors freely, nor will the concession be without return, for by this means the Museum is advertised, and residents of all classes will readily present specimens of great local interest, which ought to find their resting-place in some public institution and not remain hidden in private halls or on cottage mantel-shelves.

To pass on to the limits which should be assigned to the collections. Undoubtedly, in the Botanical and Zoological sections, especial prominence should be given to the local flora and fauna, but it is not advisable to limit the scope quite so narrowly, for boys come from all parts of the kingdom, and it is, for the most part, at their homes and during the holidays that most of their unaided work as naturalists is carried on. During term there are so many rival attractions, and such a love of athletic distinction, that many good naturalists forsake woods and meadows for the cricket field. It is therefore better, in these subjects, to extend the collections to all British animals and plants, and *to draw the line unflinchingly at this point*. If a Museum is to educate at all, the faculty of observation claims its attention before

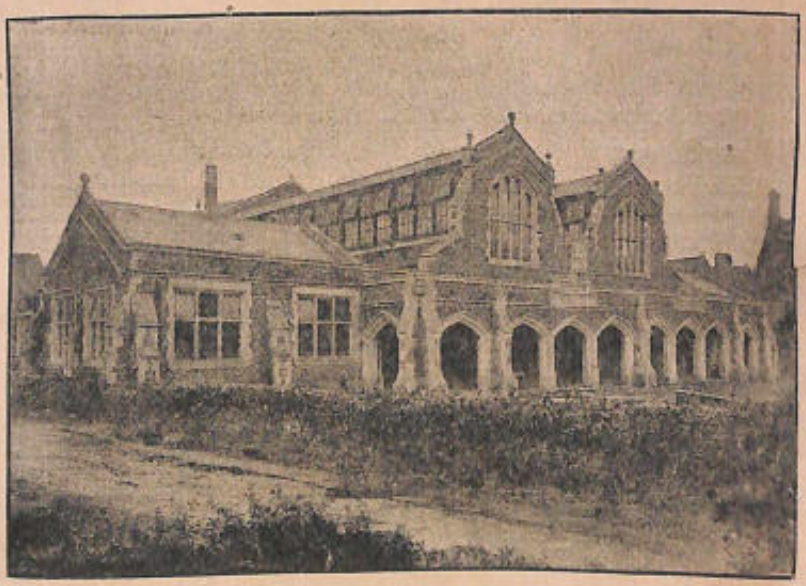

FIG. 1—EXTERNAL VIEW OF MUSEUM OF CHARTERHOUSE SCHOOL, GODALMING. *From a Photograph by* [*Messrs. W. & A. H. Fry, Brighton.*

all others, and if the boy is not taught how and what to observe in the small world immediately around him, the man assuredly will not observe Nature truly in the enlarged world of after life. It is a difficult and often an ungracious task to say "no" to some generous offer of exotic lepidoptera, foreign birds, &c., to say nothing of heads of big game, stuffed crocodiles, and other "white elephants," but the refusal is absolutely necessary if any reasonably representative collection is to be set forth in the limited space at the disposal of any school. Let it, however, be at once stated that there is no objection to receiving foreign specimens of species which do occur in Britain, but let their locality be clearly stated on the labels, and also the fact that they are of British occurrence. Exception may, perhaps, be made in the series of special types where British species are inconveniently small; but

even so, foreign species should only be exhibited in company with their British congeners and should not entirely oust them.

With regard to the Geological and Mineralogical sections, the features of the immediate neighbourhood should be clearly displayed, but for teaching purposes it is best to follow pretty closely in arrangement some good text-book, of which a copy should be kept near the

FIG. 2.—INTERIOR VIEW OF NATURAL HISTORY MUSEUM, CHARTERHOUSE SCHOOL.

cases or in the room specially set aside for these subjects. A series of well-described models of crystals ought to be placed in a conspicuous position in the collection of minerals, with actual examples, as far as possible, accompanying each model.

In addition to the actual exhibition rooms of a Museum, well-

lighted work-rooms ought to be within easy access, if possible under the same roof; one work-room, at least, should be for the use of pupils, and one should be for the private use of the Curator. The care of the Museum is most satisfactorily carried on by an official whose main work should be the arrangement, setting up, and labelling of specimens, while actual class teaching ought not to occupy any great portion of his time. Where this is impossible and one of the

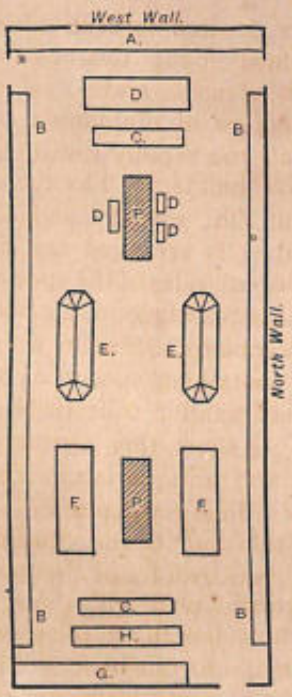

FIG. 3.—GROUND-PLAN OF NATURAL HISTORY MUSEUM, CHARTERHOUSE SCHOOL.

A.—Wall-case for Mammals.
B.B.B.B.—Side shelves for Birds.
C.C.—Central stands for ditto.
D.—Large entomological cabinet, surmounted by table case and double glass shelf.
D.D.D.—Small entomological cabinets.
E.E.—Show cases of types of Animal Kingdom.
F.F.—Table cases with Fossils.
G.—Wall case for Geological Model of Neighbourhood, etc.
H.—Botanical cabinet, surmounted by table case and double glass shelf, the two latter contain Mineralogical Collections.
P.—Stacks of hot-water pipes—tops are used for large fossils, etc.
* —point of view of photograph.

ordinary Masters of School becomes Curator, even though he be assisted by colleagues in various sections, it is most essential that there should be a thoroughly competent skilled assistant, capable of setting up specimens, making dissections, and conducting the general preparatory work. This particularly applies to the first few years after the opening of the Museum, for when once the permanent preparations are in position, the work of keeping all in good order is comparatively slight.

Turning now to the Museum at Charterhouse, the general features of the building can be seen from the accompanying figures. The

whole block consists of two centrally situated halls 75 feet by 27 feet 6 inches, running parallel to each other, these are flanked on the north and south by class rooms; to the west is a low colonnade, while the east end is formed by a large lecture hall capable of seating some 250 persons. The two central halls form the Museum proper; they are lighted by windows placed continuously on each side, immediately beneath the roof, and also by a pair of large windows in their western walls.

The south hall is devoted to Antiquities, Ethnology, Art, etc. In it are also contained many Carthusian relics, together with weapons of the chase, utensils, and so on of local interest, which furnish valuable mementoes of the simple country life of Surrey, a mode of life which is all too rapidly vanishing before the advance of railways and speculative builders. The two side walls are occupied chiefly by collections of flint weapons and of ancient pottery. The cases in which the latter is arranged are backed with continuous mirrors, so as to enable both sides of the specimens to be seen without opening the case fronts. Standing in the body of the hall are show-cases illustrating the various processes by which engravings, etchings, etc., are produced, or containing specimens obtained from the Swiss Lake Dwellings; others exhibit collections of ancient coins, Greek sculpture, and so on. In short, this portion of the Museum appeals to the anthropologist and antiquarian rather than to the student of Natural History in its ordinary acceptation.

To the north of this hall is the Museum of Natural History. The main portion of this is by force of circumstances devoted to birds. The accompanying ground plan will show at a glance the general arrangement. The whole length of the west wall is occupied by a large case with four glass doors which is to be devoted to mammals. Down both the north and south walls run two shelves at the height of about 3 ft. 6 ins. and 6 ft. 6 ins., on which are arranged the cases of birds. On the front of the lower shelf is a continuous glass-topped tray in which are placed the eggs, and where possible the nest of the species standing behind and above on the lower and upper shelf respectively. This collection of birds is of great local value; the major part of it was brought together during a period of many years by the late Mr. W. Stafford, of Godalming, by whom most of the specimens were both shot and set up. On the death of the collector a considerable sum of money was subscribed from various sources in order to prevent the collection from leaving the locality in which it had been made or being dispersed, and the whole collection, thus purchased, was handed over to the Charterhouse Museum. In addition to this, a notebook exists giving many details of the capture, locality, etc., of the specimens, and notices of other authenticated occurrences in the neighbourhood. It has thus been possible to affix to many of the cases the exact date and spot whence obtained. The wall space is insufficient to accommodate the whole collection, and

accordingly the geese and ducks are placed on brass-supported frames, running transversely to the length of the hall, near the west and east ends. The collection is strictly limited to British species according to the "Ibis" list, which has also been followed in the classification.

A few hybrids and professed "foreigners" have been removed from the collection and placed together in a separate part of the hall; eventually it is to be hoped they will be banished. The arrangement of the birds was necessarily the first thing to be attended to when the Museum was being equipped last year, thus many other sections of this half of the Museum are in a backward condition. Next to the birds, the insects, and especially Lepidoptera, attract most attention. Here the arrangements are good; there is a large entomological cabinet in which are being arranged all the British Macro-lepidoptera. It is intended that the full life-history of every species shall be shown with larvæ on artificial food-plants, pupæ both in and out of cocoon where such exists, and imagines in resting attitudes, as well as the conventional "setting." This, of course, will be a work of years; but it has begun and is progressing. Above the cabinet is a table-case, in which will be placed diagrams and described specimens to show the chief anatomical features of the various orders of insects, and above this again double glass shelves for miscellaneous objects of interest connected with the habits and economy of insects. In addition, there has been presented a fine collection of Lepidoptera (imagines only), which is kept apart. Hymenoptera have claimed a few votaries, but other orders of insects fail to attract at present. There is accommodation for a collection of Mollusca and material, chiefly foreign, where much weeding will be necessary.

Two show-cases standing in the room are devoted to special types, one including Protozoa to Mollusca, the other Chordata. These are intended for specialists in Biology, and will ultimately be arranged so as to follow, with a few additions of extinct forms, the book of Practical Zoology in use in the school. There are two table cases of fossils arranged stratigraphically. A case of Minerals arranged after an elementary text-book stands near the east end of the room, and beneath this case a collection of dried plants mounted on cards and placed on sliding trays. Portfolios of dried plants, comprising collections made in the neighbourhood of Godalming, complete the Botanical section at present. At the east end of the hall is a large wall case, destined to contain a Geological model of the neighbourhood and diagrams to illustrate sections. The fossils characteristic of the strata, and, as far as possible, the peculiarities of fauna and flora, will be pointed out. Beneath the shelves on which the birds are placed is a vacant space which eventually can be occupied by low cabinets, storecases for duplicates and specimens, for class work.

The care of the Museum is entrusted to two of the Masters of

the school, who are unfortunately not assisted by a regularly trained skilled assistant. It may seem ungracious to criticise what is in many respects excellent, but a few possible improvements may be mentioned for the warning and benefit of others who have not yet "builded new barns" for Museum purposes. (1) There are no work-rooms in connection with the Museum, nor any rooms situated conveniently near in which the work of preparation can be carried on. The inconvenience of this want to the Curators will be only too obvious to all who have ever had charge of any portion of a Museum. Moreover, the absence of a work-room for students greatly limits the usefulness of the collections. (2) The position of the windows, though giving a good light, does not give so good a light as continuous skylights would have afforded. (3) The supports of the roof descending on to the main walls of halls curtails the available wall-space to a great extent. In the present instance, had the roof been supported in some other way, a rail-gallery would have been possible round both halls, which would have doubled the wall-space. Under the present circumstances, nearly half the wall-space is too high for *instructive* exhibition purposes, though it can be made serviceable for the reception of various gifts which have no proper place in a School Museum, and can therefore without loss be "skied" into positions sufficiently remote as not to disturb the visible arrangements, and sufficiently prominent to gratify the well-meaning donors.

OSWALD H. LATTER.

VI.

The Surface of the Moon.[1]

IN the January number of NATURAL SCIENCE, attention was directed to two short articles on the subject of lunar volcanoes and lunar glaciation. In the *Bulletin of the Philosophical Society of Washington* (vol. xii., April, 1893, pp. 241–292) will be found a paper on "The Moon's Face, a Study of the Origin of its Features," being the retiring address of the President, Professor G. K. Gilbert. This is an admirable summary of the history of the study of the moon's surface, while, at the same time, it abounds with suggestive and thoughtful ideas. Professor Gilbert commences by remarking that the topography of the moon is perhaps better mapped than that of North America, and mentions the various features which have been observed and named. The "craters" are described, and figures are given, comparisons being drawn between the "wreath" of a lunar "crater" and the landslip terraces of the margin of a basaltic plateau. After this descriptive matter, the various "theories" are taken into consideration. Regarding the "volcanic theory," Professor Gilbert compares unfavourably the relative abundance of the craters on the moon with any equal area on the surface of the earth, those on the earth being as one-tenth to those on the moon. The same disparity exists with respect to size, the ten largest terrestrial craters recorded having a mean diameter of eleven miles. These are, Lake Bourbon (Luzon), 16 by 14 miles; Asosan, 15 miles; Kamschatka (Scrope, 2nd ed., p. 457), 15 miles; Mauritius (Darwin), 15 by 11 miles; Lake Bolerna (Italy), 11 by 9 miles; Lake Maninju (Sumatra), 15 by 7 miles; Pepandayan, 15 by 6 miles; Teneriffe, 7 by 8–10 miles; Deception Island, 8 by 7 miles; Monte Cavo, 7 miles; Mount Marindin (Mindanao), 9 miles; Mount Astria (Iceland) 10 miles. The mean diameter for the ten largest lunar craters visible to us is 275 miles. These are, Apennines, Serenitatis, Crisium, Humorum, Humboldtianum, Bailly, Iridium, Clavius, Otto Struve, and Grimaldi. The actual largest terrestrial crater has a diameter of about fifteen miles, the largest lunar crater, that whose rim is partially preserved in the Carpathian-Apennine-Caucasus chain, has a

[1] Précis of Professor G. K. Gilbert's address to the Philosophical Society of Washington, December 10, 1892. By C. Davies Sherborn, F.G.S.

diameter of 800 miles. There is no important discrepancy in the vertical dimensions. Lunar craters of the first rank range from 8,000 to 15,000 feet in depth; terrestrial craters, probably from 2,000 to 4,000 feet. The contrasts as to form are of greater importance, and are all fully set forth in the paper. Professor Gilbert concludes by observing that "the volcanic theory, as a whole, is therefore rejected, but a limited use may be found for the maar phase of volcanic action [no eruption of lava, but merely an explosion of steam] in case no other theory proves broad enough for all the phenomena."

Passing next to the tidal theory, Professor Gilbert puts aside the views of Rozet, Hooke, Bergeson, and Humphreys, and considers those of Faye, Ebert, and Hannay. These postulate a time when the moon was liquid, with the exception of a thin crust. The moon then rotated more rapidly than now, and great tides, excited by the earth's attraction, racked and cracked its crust, and here and there squeezed out a portion of the liquid nucleus, which flowed back again when the tidal wave had passed; but congelation caught the flood at its edges, so as to mark its limit by a solid ridge. By each successive tide the operation was repeated, with the result that the wall was given a circular form, and was gradually built up. The process was finally closed by the congelation of lava in the orifice, and while congelation was in progress the last feeble eruption sometimes produced a central hill. Professor Gilbert asks, in criticising this theory, for an explanation of the multitude of small craters overlying the larger, and why, if the crust were divided by fissures, would not the tensile strains wrought by the crest of the tidal wave cause the fissures to gape, instead of forcing out the liquid through apertures here and there? Or, if there were no fissures, would not the strains suffice to produce them? He further points out that there are numerous craters, of small or medium size, occupying slopes of the greater crater rims, and the initiation of these by tidal process seems impossible. Whatever lava escaped from an orifice on a slope would, he remarks, flow down the slope, instead of being drawn back.

The views of Ericsson and Peal, that the site of each crater was once occupied by a pool of water which, by heat from below, was vapourised, converted into snow, and from which was eventually accumulated an annular ridge, he criticises as follows:—If the rim were built up by the quiet fall of an infinitude of ice particles or snow flakes, its configuration should be smooth and regular instead of exhibiting the rugosity actually observed. The postulated heat of the central area might render the inner slope steep, and even produce the inner cliff and terraces, but the theory affords no explanation of the wreath nor of the central hill; it fails also to account for the small craters formed on the rims and slopes of the larger, for the bottoms of these are far above the assumed rock plain of the moon, through which the theory supposes the internal heat to have been communicated. Professor Gilbert admits that he is unable fully to

judge Peal's views, which have been circulated in a pamphlet, of which 100 copies were privately printed, and therefore practically unpublished and inaccessible.

All other theories which have been put forward appeal in some way or other to the collision of other bodies with the moon's surface, and Professor Gilbert groups them together under the heading "Meteoric." Proctor apparently was the first to definitely suggest a "meteoric" theory in 1873 and 1878. "Asterios" and Meydenbauer have also advanced similar views. The "meteoric" theory is thus defined in Gilbert's paper:—If a pebble be dropped into a pool of pasty mud, if a rain-drop falls upon the slimy surface of a sea marsh when the tide is low, or if any projectile be made to strike any plastic body with suitable velocity, the scar produced by the impact has the form of a crater. This crater has a raised rim, suggestive of the wreath of the lunar craters. With proper adjustment of material, size of projectile, and velocity of impact, such a crater scar may be made to have a central hill, and it is Professor Gilbert's belief that all features of the typical lunar crater and of its varieties may be explained as the result of impact. His arguments may be summed up in the following sketch. The shooting star records by its brief coruscation the collision with our atmosphere of a particle of star dust; and although they are generally very minute in size, a few survive after passing through an atmosphere and reach the earth as aerolites weighing ounces, pounds, or occasionally tons. It is also an ascertained fact that these bodies are spreading in countless myriads through space in all directions. As the moon is either without atmosphere, or has one of extreme tenuity, the impact of one of these bodies may have an important effect on the surface. As it is incredible that even the largest meteors of which we have direct knowledge should produce scars comparable in magnitude with even the smallest of the visible lunar craters, advocates of meteoric theories have assumed that at some earlier period the meteors encountered by our solar system were of greater size than now, and as no evidence has been found that the earth was subjected to a similar attack, there is assigned to the lunar bombardment an epoch more remote than are the periods of geological history, any similar scars produced on the earth having been obliterated.

Professor Gilbert does not assume that the surface of the moon was necessarily soft. Rigidity and plasticity are not absolute terms, but relative. But he suggests that the heat developed by the sudden arrest of a fragment of matter travelling at the rate of 45 miles per second might serve not only to melt the fragment itself, but also to liquefy a considerable tract of the rock mass by which its motion was arrested. The difficulty of the relation of the volume of the rim to the capacity of the hole is carefully considered, and experiment proved that when target and projectile were of uniform consistency throughout, there was no defect of rim; but when the general mass

of the target was softer than the portion at the surface, the uplift consequent on the production of the hollow was only partially localised about its periphery, the remaining part being widely distributed through the floor of the softer material below. The last difficulty is connected with the circular contours of the craters. The predominant direction of the swiftly moving meteoric bodies approaching from all directions would be about 45°, and the scars produced by collision would be predominantly oval instead of predominantly circular. Proctor suggested that immediately after the shock of the collision there might be an elastic return to a circular form ("The Moon," London, 1873, p. 346); but to account for this peculiarity of form of the lunar craters, Gilbert advances a "Moonlet Theory," assuming that before our moon came into existence the earth was surrounded by a ring of meteors similar to that possessed by Saturn, and that the small bodies constituting this ring afterward gradually coalesced, gathering first around a large number of nuclei, and finally all uniting in a single sphere, the moon. Under this hypothesis the lunar craters are the scars produced by the collision of these minor aggregations or moonlets, which last surrendered their individuality. This theory is fully explained and worked out in the address, to which the reader must be referred for details.

Professor Gilbert then discusses the "arched floors" of some of the larger craters, and the distribution and overlap and interference of the craters themselves; and then turns to the question of lunar sculpture. The sculpturing of the Mare Imbrium has particularly engaged his attention, and he points out that the trend of the sculpture lines and a peculiar softening of the minute surface configuration, as though a layer of semi-liquid matter had been overspread (which he believes to be the fact, the deposit having obliterated the smaller craters and partially filled some of the larger), indicate that a collision of exceptional importance occurred in the area of the Mare Imbrium, and that one of the results was the violent dispersion in all directions of a deluge of material—solid, pasty, and liquid. The effect of this "splash" has been traced by Gilbert 900–1,000 miles in one direction, and he estimates the volume of the projectile to have equalled a sphere 80–100 miles in diameter. A sketch-map is given, showing the trend of the lines of sculpture, and the broken and notched rim of the Mare Imbrium.

Referring to the "furrows" seen on the moon's surface, so remarkably straight in general, one of which is calculated to be 187 miles long, 10–25 miles wide, and 11,000 feet deep, it is suggested that these are the tracks left by solid moonlets, whose orbits at the instant of collision were nearly tangent to the surface of the moon. It is curious that when plotted on the chart of the moon's face, Professor Gilbert found that more than half of them accorded in direction with the trend lines of the Imbrium outrush, and he is led to suggest a connection between them and the scattered fragments dispersed by

the collision in that district. The anomalous plateau of Wargentin is considered, but no definite suggestion is made, although clues are not wanting, for the whole neighbourhood abounds in evidence of flooding.

With regard to the "white streaks," which are quite independent of any surface configuration, and which usually radiate from some crater itself lined with a similar white matter, Professor Gilbert quotes an unpublished suggestion of William Würdemann of Washington, which is as ingenious as it is simple. Würdemann writes to Dr. B. A. Gould as follows:—"The most remarkable appearance on the moon, for which nothing on earth furnishes an example, is presented by those immense radiations from a few of the larger craters—perfectly straight lines, as though marked with chalk along a ruler—starting from the centre of the crater and extending to great distances over every obstruction. My explanation is that a meteorite, striking the moon with great force, spattered some whitish matter in various directions. Since gravitation is much feebler on the moon than with us, and atmospheric obstruction of consequence does not exist, the great distance to which the matter flew is easily accounted for." Gilbert remarks that this theory accounts for the straightness of the rays, for their vanishing edges and ends, for their independence of topography, for their relation to craters, for the whiteness of the associated craters, and for the nimbus in which the rays sometimes unite close to the crater. It further explains the white crests of many grey craters, for peaks would intercept more than their share of the horizontal shower. The difficulties and suggestions of this theory are fully dealt with by Professor Gilbert, and he is inclined to believe the white matter to be a readily fusible solid.

Summing up the various theories in his retrospect, Professor Gilbert says:—the impact theory applies a single process to the formation of craters (excepting only the rill pits), correlating size variation with form variation in a rational way. Specialised by the assumption of an antecedent ring of moonlets, it accounts also for the great size of many craters. It brings to light the history of a great cataclysm, whose results include the remodelling of vast areas, the flooding of crater cups, the formation of irregular "maria," and the conversion of mere cracks to rills with flat bottoms. It explains the straight valleys and the white streaks. In fine, it unites and organises as a rational and coherent whole the varied and strange appearances whose assemblage on our neighbour's face cannot have been fortuitous.

The address concludes with a brief discussion of the growth and age of the moon. Professor Gilbert believes that during the whole period of growth the surface lost heat by radiation, but the process of growth cannot have been slow enough to permit the concurrent dissipation of all the impact heat. Liquefaction was a local and temporary surface phenomenon, and there is little evidence of the wrinkling, which, theoretically, should result from the adjust-

ment of a cold crust to a cooling nucleus. The topography is due to sculpture, not to wrinkling; the smooth floors of the maria have anticlinal and monoclinal forms, but of so gentle a slope that they are seen very near the terminator, and can represent but a minute amount of arc shortening. It is therefore probable that the final shrinkage of nucleus was small, and the antecedent storage of heat correspondingly small. During the whole period of growth, the body of the moon was cold. With regard to the age of the moon, Professor Gilbert asks two questions. (1.) Does the earth exhibit impact craters? If not, then erosion and sedimentation have destroyed them, and the Cenozoic era did not witness the building of the moon. (2.) Is any horizon of stratified rocks generally or widely characterised by molten disjecta? If not, then the moon was already a finished planet in Palæozoic times.

VII.

Antarctica: a Supposed Former Southern Continent.

IN the current number of the *Geographical Journal* there appears a paper by Mr. H. O. Forbes, on "The Chatham Islands: their relation to a former Southern Continent," read before the Royal Geographical Society on the 13th March last. He describes his discovery there in the previous year of remains of a large extinct ocydromine Rail, which he has identified with *Aphanapteryx*, a genus till then known only from Mauritius, and of a large *Fulica* closely related to *F. newtoni* of the same region. Taking these fresh facts into consideration, he marshals all the data which he considers prove a strong case for the probability of the existence of a former southern continent, and he sketches on a map of the Southern Hemisphere what he believes was the configuration of *Antarctica*, as he has named that vanished continent. He believes that it followed nearly what is the 2,000-fathom line, and extended northward from a circumpolar area, by broad extensions, one to join an old New Zealand continental island (including the Antipodes, the Maquarries, New Zealand, the Chatham, Lord Howe, Norfolk, the Kermadec, and the Fiji Islands); another to East Australia with Tasmania; another to the Mascarene and surrounding islands (the *Lemuria* of Sclater); *perhaps* one to South Africa, and, lastly, one to South America. The form of this continent would not interfere with the opinions expressed by many authorities in the permanence of the great ocean basins.

The chief evidence adduced by Mr. Forbes in support of the former existence of this continent, shortly stated, rests on the distribution of forms confined to or extending but little beyond the Southern Hemisphere.

I.—Among Aves.

1. Of the Struthious birds:—*Dinornis* and *Apteryx*, in New Zealand; *Dromornis* and *Dromæus* in Australia; *Casuarius* in the Papuan sub-region; *Æpyornis* in Madagascar; *Brontornis* in Patagonia.
2. Of the Trogonidæ in South America, Tropical and South Africa, and in the Indian region.

3. Of the Spheniscidæ, which are confined to the Southern Hemisphere, and appear fossil in New Zealand and in Patagonia.
4. Of the Chionidæ, a family of the Charadriidæ, confined to the Antarctic Islands.
5. Of the Psittacomorphæ, confined chiefly to the Southern Hemisphere, with a few stragglers in North America and a few groups in India.
6. Of the *Peristeropodes* (of the Gallinaceæ) in Australia and in South America.
7. Of the Ocydromine Rallidæ (*Aphanapteryx*), of *Fulica* and *Notornis*, of *Frigelupus* and *Heterolocha*, in the New Zealand and the Mascarene regions.
8. Of Solitaire, Dodo, and Didunculus, in the Mascarene Islands and Samoa.
9. Of the Avocets and the Stilts, in New Zealand, Australia, Africa, and South America.
10. Of the embryological affinities of the Dendrocolaptine birds of the Neotropical region with the Gymnorhine crows of Australia; of *Artamus*, with *Pitta* and *Grallaria*; of *Petræca*, with the Mniotiltidæ; of the New Zealand Xenicidæ, with the Australian and Oriental Pittidæ, the Mascarene Philepittidæ, and the Neotropical Pipridæ, Cotingidæ, Tyrannidæ—evidence of great weight.

II.—Among other Vertebrata.

1. Of Marsupialia—*Nototherium*, *Diprotodon*, *Thylacoleo*, *Thylacinus* in Australia; *Prothylacinus*, *Amphiproviverra* in Patagonia.
2. Of Edentata—In South America, South Africa (Orycteropidæ) and in India and the Malay Archipelago (Manidæ).

This distribution of the *Edentata* may, however, indicate a connection of "Antarctica" with Africa and South America when it was not in connection with Australasia, but when there was a bridge between East Africa and the Oriental Region.

3. Of Amphibia—Cystognathidæ in Australia, Tasmania, and South America; and giant tortoises in Madagascar and the Galapagos Islands.
4. Of Fresh-water Fishes—the great alliance between those of Australia, New Zealand, Chili, Patagonia, and the Falkland Islands; Haplochitonidæ and Galaxiidæ and the Dipnoan and Osteoglossid types in South Africa and in South America.

III.—Among Invertebrata.

1. Of *Peripatus* in South Africa, Australia, South America, with the West Indies.

2. Of *Cercophonius* (a genus of Scorpions) in South East Australia and South America.
3. Of Buprestidæ and *Longicornia* in South America, Australia, and New Zealand.

IV.—AMONG PLANTS.

1. Saxafrageæ—Escalonieæ (17 genera) and Cunonieæ (18 genera) in New Caledonia, Australia, Tasmania, New Zealand, Mascarene Islands, South Africa, South America. Only two of these 35 genera cross the Equator.
2. Proteaceæ (49 genera, 950 species)
3. Monimiaceæ (22 genera and 250 species)

(2 and 3) in South America, South Africa, Madagascar, Tasmania, New Zealand, New Caledonia.

4. Of the Cupressinæ (Coniferæ), *Callitris* in Africa, Madagascar, Australia; *Fitzroya* in Chili and Tasmania.
5. Of the Verbenaceæ, *Petræa* typically South American, in Java and Timor, and *Petræovitex* nearly related to *Petræa* in Buru and Amboina.
6. Of *Senecio appendiculatus*; *Fernelia buxifolia*; *Chasalia capitata*; *Pouzolzia lævigata*, *Cyperus scoparius* in Timor and in the Mascarene Islands.
7. Of *Ranunculus trullifolius*, *R. Mosleyi* (allied), *Lyallia*, in Kerguelen and in Fuegia.
8. Of *Polypodium vulgare* in Kerguelen, South Africa, South America, and Marion Islands. *Cotula plumosa*, *Uncinia compacta* in Kerguelen and Australasian region. *Pringlea* in Kerguelen and Marion Islands.
9. From the remains of buried forests, and the occurrence of lignite beds, and of enormous deposits of Peat in the Chatham Islands, in Kerguelen and in other Antarctic Islands, indicating a former forest-clad region.

The development of the Southern fauna and flora occurred, Mr. Forbes believes, during the Ice Age of the Northern Hemisphere when "Antarctica" rejoiced in a semi-tropical climate. Their dispersion occurred during the slow oncome of the partial glaciation of the Southern Hemisphere, which exists now.

The occurrence of many southern forms of life in the Miocene of France and other parts of Europe is explained, according to the author, by their having been driven north during a cold, or perhaps glacial, period in the Southern Hemisphere in that age, of which there are evidences in various parts of South Africa, Australia, and South America, corresponding to the warm epoch in the Miocene age in northern latitudes.

Mr. Forbes has independently arrived at views very similar to those expressed by Professor L. Rütimeyer in 1867 in his work, "Ueber die Herkunft unserer Thierwelt: eine zoogeographische Skizze"; and several geologists, notably Dr. W. T. Blanford, have discussed some of the more important problems involved. We have, however, compiled a concise synopsis of Mr. Forbes' latest arguments in favour of the theory of "Antarctica" (based upon the known distribution of certain animals and plants), because the sum-total of the evidence is now so much greater than it has been heretofore; and such marked progress has been made in investigating the geology of the Southern Hemisphere during recent years, that it seems profitable to re-open the question from the geologist's point of view once more. Zoologists and botanists will welcome the expression of the latest views of those who are acquainted with the physical considerations bearing upon the subject.

VIII.

On the Work of Glaciers.[1]

AN invitation from the Editor of NATURAL SCIENCE to write a short critical review of Professor Bonney's article on Glaciers in the current number of the *Geographical Journal*, is as refreshing as a whiff of cool, pure air from the "Gletcherwelt" in these sultry June days. To be brief in the present instance is not difficult, since I have nothing of importance to add to what I have already put into print, while I have nothing, on the other hand, to unsay. One has not time in these days to go on repeating, *ad nauseam*, the same arguments. All that my friend Dr. Blanford has urged now has been met and answered, as I venture to think, by anticipation in my papers, and in the criticisms which I have published of the writings of others during the last ten years, that is to say, since my papers (1) "On the Mechanics of Glaciers," (2) "On the Origin of Valley Lakes," were published side by side in the February number of the *Quarterly Journal of the Geological Society* in 1883. On the other hand, after reading the article of Professor Bonney's, which I have been asked to notice, I can only find one criticism to offer, and that is that he does not make enough of the physical impossibility of the work of excavation being done by ice, or of local earth movements, which must occur as details of the "warping" of the crust, in districts such as must be subjected to considerable squeezing between two great stable masses of the Archæan crust, as these undergo circumferential approximation towards one another by secular contraction. All that has been written in more recent years by Professor Spencer (and others) on the negative results observed by him under some of the great Norwegian glaciers, and the positive results obtained by himself and his colleagues as to the recent changes of level in the region of the great lakes of North America, co-operating with the damming-up of old Eocene lines of drainage by glacial detritus during the Quaternary period, goes to confirm the suggestion (*inter alia*) that I threw out ten years

[1] "Do Glaciers Excavate?" By Professor T. G. Bonney, D.Sc., F.R.S. With observations by Dr. W. T. Blanford, Mr. Douglas Freshfield, Sir Henry H. Howorth, and Mr. W. M. Conway. *Geographical Journal*, vol. i., pp. 481–504 (June, 1893).

ago as to the origination of the basin of the Lake of Geneva by the pinching up of the valley of the Rhone, as an incident in the comparatively recent movements that have given to the Alps and the Jura their present positions.

There are some pertinent remarks to the same effect in Heim's well-known "Mechanismus der Gebirgsbildung"; and that writer once for all disposes of the case of the Vierwaldstättersee (Lucerne Lake) by the results obtained by a series of soundings carried out by himself and one of the engineers of the Federal Government; since these show conclusively that the bed of the lake is *an alluvial plain* of an ancient valley, which has been converted into the present gem of central Alpine lakes by a partial closure of the valley at the outflow of the Limmat. The deep Achensee, again, is completely explained by the facts which I gave ten years ago, as a case of a fault-originated gorge, the drainage having been reversed as the great gorge of Jenbach was dammed by the moraine stuff from a side-valley at Maurach.[2] The Lake of Constance, again, is now well known to be a hollow formed by the damming-up of an ancient valley by glacial detritus; the present "Falls of the Rhine" at Schaffhausen having originated from a similar set of surface-changes to those which have produced the Falls of Niagara, by diversion of drainage due to glacial obstruction of the ancient valleys. Instance after instance of those quoted by Ramsay and his school (even Ramsay's pet child, the Lake of Llanberis) is being disposed of, as larger views of physiography are made to bear upon the study of lakes.

To go in detail into Dr. Blanford's criticisms would be but to attempt to combat a *scientific belief*, which has been, to my mind, rendered untenable by counter-arguments, based on known physical and mechanical laws and observed facts. Until "geologists" will take the trouble to go into the physical laboratory and master those laws, and then show that my experimental work of ten years ago in the Wellington College Laboratory upon the properties of ice, and the conclusions to which it leads, as put forth in my paper on the "Mechanics of Glaciers" (*supra cit.*); in the paper supplementary thereto on "Solar Radiation and Glacier Motion";[3] in some criticisms of a paper by another author which I published under the title of the "Motion of Land Ice";[4] and in the remarks which I made two years ago under the head of "Dr. Nansen on Glaciation,"[5] I feel that it is rather a "dealing of blows into the air" to enter into controversy on these points.

The phenomena of the recession of glaciers within the last century or two, leaving a plain strewn with rolled detritus, and the draining

[2] See my paper on the "Triassic Deposits of the Alps," *Geol. Mag.*, 1882, p. 504.

[3] *Nature*, vol. xxvii., 1883.

[4] *Geol. Mag.* for 1891, pp. 141–142.

[5] *Nature*, vol. xliii., pp. 541–542.

of a still more ancient lake by the sawing through of a moraine-dam by the valley-stream, can scarcely be better illustrated than in the case of the Rosegg-glacier. There, as in many another Alpine example, the recession of the glacier leaves no trace of excavation of the valley floor, but the contrary. The Muir Glacier of Alaska is also a very strong case. I gratefully avail myself of the paper on this which Professor Cushing, of Cleveland, Ohio, was good enough to send me.[6] His concluding remarks are—"Those who hold the power of glaciers to vigorously erode hard rocks under most circumstances, take (it seems to me) an indefensible position. At the Muir Glacier, in just the position where the greatest erosion would naturally be expected, soft gravels have been undisturbed by the ice. The key setting forth when glaciers will erode and when not, is certainly lacking at present. It is very desirable that a prolonged and detailed study of the Muir Glacier should be undertaken. It is a comparatively large glacier, rapidly dying out, and presents an admirable opportunity for studying the behaviour of a large glacier under such circumstances. Such work could not fail to be of great value."

On one minor point I cannot follow Professor Bonney. When referring to Ramsay's classical paper, he says (p. 482): "It was proved that the rock-basins of the Alpine lakes could not have been produced by any local subsidence, or by fissures in the Earth's Crust." In reasons given in my paper (*supra cit.*) on the "Origin of Valley Lakes," I have given the grounds of this dissent. I believe that, in certain instances, these have severally operated as *important factors*, though not as sole cause, and my belief of ten years ago has been confirmed by the observations I made in 1883, through the courtesy of F. Ritter von Hauer, of models of saliferous and gypsiferous strata, beneath upland valleys in the Alpine Trias, which are to be seen in the Geologische Reichsanstalt of Vienna. Mr. Jukes Browne has referred to these in his "Handbook of Physical Geology" (2nd edition, p. 627). Let underground erosion by the solution and removal of such beds or of calcareous strata proceed through a long period of time, and another great glacier filling such a valley would make, by its dead weight, short work of the hollow floor. Such a crushing-in of the pie-crust is, however, a very different thing from what Ruskin has graphically described as a "custard scooping out its own dish." I suggested this ten years ago: its *rationale* is self-evident, but it has I suppose been ignored by subsequent writers because it was not "orthodox," though it commended itself to the late Professor John Morris, when I suggested it to him. But subsidences can and do occur without the crushing weight of glaciers.

The old and trite argument based on the association of lakes with glaciated regions may be put another way. It may, I think, be fairly asked, whether it is not the association of lake-basins in great numbers

[6] *American Geologist*, October, 1891.

(as in Scandinavia, Finland, and North America) with great districts, in which the terrain is for the most part made up of Archæan crystalline rocks, in which therefore the traces of glaciation in high latitudes are better preserved, and surface changes (including differential movements) have been longest in progress, that has led to some confusion of thought on this matter.

A. IRVING.

IX.

Professor Huxley on Evolution and Ethics.[1]

PROFESSOR HUXLEY'S zoological work is great, and it has left an enduring mark on his science. He has lived in times when a new idea has been a solvent corroding the incrustations of tradition and habit, and those who knew the jewel of truth only by its setting and tarnished ornament, hated and feared him. Acute minds, orthodox or unorthodox, respected and admired him as a master in the craft of controversy, honest and resolute, apt at keen lunge and nimble recovery. But the great crowd who make reputations that iconoclastic historians of the future may have material, see in him a constructive thinker—a prophet of the new era. And so this delivery on "Evolution and Ethics" will be taken as coming "with emphatic warrant," and must be subjected to a more critical appreciation than the author himself may think fitting.

Let me state the Professor's argument as clearly as I can. Among the observed phenomena of the cosmos the most typical is cyclical change. Thus, from the inert bean there grows up the bean-plant, which, in its turn, produces other beans, and the wonder of leaves and stem fades away. Such recurring cycles are the salient feature in the shifting impermanence of the cosmos. In man, to whom the stream of life has apparently led, we find that pain, present everywhere, reaches its acutest point, and we find that his progress has been such that the old ape and tiger habits of ruthless and cruel aggression are only virtues belated in time, and become sins by the accident of advance. Thus consideration of the cosmic process of evolution brings man up against the mystery of evil.

With the philosophers of Hindustan and Ionia, centuries before our era, the changefulness of the phenomenal world, as well as the suffering in it, became apparent. When culture of the intellect and of the pleasures followed material prosperity, there came exhaustion and ennui and that "beneficent demon, doubt, whose name is legion and who dwells amongst the tombs of old faiths." Originally the conception Justice arose from some beginning like the compact of a wolf-pack, the understanding not to attack one another while on the chase. From this

[1] EVOLUTION AND ETHICS. By Thomas H. Huxley, F.R.S. The Romanes Lecture for 1893. London: Macmillan & Co., 1893. 57 pp. Price 2s. net.

came the conception of justice-in-itself, of merit as divorced from the effect of action on others, the abstract idea of goodness.

The old world philosophers, turning from this new conception to the cosmos, found the cosmos incompatible with goodness. Suffering and sorrow, sunshine and rain, were distributed independently of merit. With Greek and Semite and Indian the conscience of man revolted against the moral indifference of nature. Instead of bringing in a verdict of guilty, they attempted reconciliation in various ways. Indian speculation invented or elaborated the theory of transmigration, in which the Karma or soul-character passed from individual to individual, and in the whole chain the algebraic sums of happiness and sorrow were proportional to desert. Attempts at metaphysic were more potent in shaping ethics. Then, as now, a permanent "substance" was supposed to underlie the shifting series of phenomena of mind and matter. The Indians called the "substance" of the cosmos "Brahma," of the individual "Atman." The "Atman" had its individual existence only through its casing of phenomenal desires and passions, and the aim of wise living was not merely to destroy the body, but, by a rigorous and disciplined asceticism, to suppress all individual desires, until "Atman" in negation of self became merged in "Brahma."

Gantama accepted most of these premises, but going beyond them, he disbelieved in the substance of mind as of matter, and reduced all to a shifting series of phenomena.

Karma was handed on by a process of induction from phenomenon to phenomenon; transmigration was abolished and Nirvana, the cessation of all became the ultimate good. But the means for this was not asceticism, but culture of moral qualities, and a direct attack on the baser passions as desires.

The Stoics also were metaphysicians, and imagined an immanent, omnipotent, and infinitely beneficent cause. Evil was incompatible with this, and so they held against experience that either it did not exist, or it was inflicted for our benefit or due to our fault. These theories, although they remain on evidence, do not explain "why the immense multitude of irresponsible sentient beings, which cannot profit by such discipline, should suffer." The practical conclusion of the Stoics was to live according to nature; not nature in the sense of the crude individualism of later days, but the higher nature of moral man as opposed to the lower nature of the world in general. The ethical system of the Stoics was divorced from this theory of the cosmos; it was purely intuitional, and they came to see that the ideal wise man was incompatible with the nature of things; that the cosmos works through the lower nature of man not for righteousness, but against it. So their "perfect way" came to be "apatheia," in which desire must do nothing but execute the judgment of pure reason.

Thus Professor Huxley sees the same course in the evolution of

ethics, among Indians and Greeks; and the end, when the "malady of thought" brings the conception of good against the evil of the cosmic process, is Nirvana or Apatheia—salvation sought in absolute renunciation.

In the modern world, the scientific optimism of a few years ago is falling before a frank pessimism. Cosmic evolution is accountable for both good and evil, but knowledge of it provides no better reason than earlier speculation for choosing the good. The survival of the fittest does not mean the survival of the best, but of the best adapted to the conditions. The cosmic process is not only non-moral, but immoral; goodness does not lead to success in it, and laws and moral precepts are directed to the curbing of it. Still, Professor Huxley sees a way out of absolute pessimism. Society remains subject to the cosmic process, but the less as civilisation advances and ethical man can combat it. The history of civilisation shows that we have some hope of this, for when physiology, psychology, ethics, and political science, now befogged by crude anticipations and futile analogies, have emerged from their childhood, they may work as much change on human affairs as the earlier-ripened physical sciences wrought on material progress. And so, remembering that the evil cosmic nature in us has the foothold of millions of years, and never hoping to abandon sorrow and pain, we may yet, in the manhood of our race, accept our destiny, and, with clear and steady eyes, address ourselves to the task of living, that we and others may live better.

It would be absurd to imagine that Professor Huxley considered this eloquent address a reasoned exposition, and indeed many of his own notes supply precisely those remarks which a hostile critic would have been glad to make. The sole occasion of criticism is that the address will have a positive scientific value assigned it. The dominant note in the address is that the process of life presents the "appearance of cyclical evolution." The "value of a strong intellectual grasp of the nature of this process lies in the circumstance that what is true of the bean is true of living things in general." This is really an argument from analogy, and has no value in a constructive system. The politician addressing those who agree with him, or the preacher strengthening his hearers in the faith which they share with him, use analogies with right and reason. But they are essentially rhetorical appeals. They become arguments only when by reason of some definite identity in the subjects compared they cease to be analogies. The Professor in his first note (on the word "appearance" in the sentence just quoted) shows that he introduces the example of the bean-plant as a mere analogy. The actual process true of all living things is not a cyclical but a linear evolution. A continuous chain stretches from every living being to the distant and prodigious first appearance of life. The successive individuals are pendants of the chain, and, dropping off, leave it unbroken.

Professor Huxley makes too much of the shifting impermanence

of all things, both on his own account and in his interpretation of the old world philosophers. In the physical world impermanence means no more than constant movement; that energy rather than inert matter is the conspicuous phenomenon. In the mental world, it is true "that no man can, with exactness, affirm of anything in the sensible world that it is." But that is an ultimate metaphysical difficulty in a manner distinctive of all thinking, in a manner distinctive of none. The human mind is conditioned by its own limitations, but these limitations are conditions to be held in the background of all argument, an understood preamble in all thinking, and by no means to form an "in-and-out clause" in questions at issue.

Nor is it indisputable that Professor Huxley has assigned their proper value to pain and suffering in the world of life. Even if it were true that the consummation of pain is reached only "in man, the member of an organised polity," this were no isolated fact of far-reaching ethical import; but a fact in dependence on the increased intelligence of civilised man, an intelligence equally susceptible of increased pleasure as of increased pain. For suffering in the animal world, let Wallace and Darwin answer: "On the whole, then, we conclude that the popular idea of the struggle for existence entailing misery and pain on the animal world is the very reverse of the truth. What it really brings about is the maximum of life and of the enjoyment of life with the minimum of suffering and pain. Given the necessity of death and reproduction—and without these there could have been no progressive development of the organic world—and it is difficult even to imagine a system by which a greater balance of happiness could have been secured. And this view was evidently that of Darwin himself, who thus concludes his chapter on the struggle for existence: 'When we reflect on this struggle, we may console ourselves with the full belief that the war of nature is not incessant, that no fear is felt, that death is generally prompt, and that the vigorous, the healthy, and the happy survive and multiply.'"[1]

It is not, then, inevitable to regard the cosmic process as evil. Even when man, in various ages, had elaborated the conception of abstract goodness, and had endeavoured to make his justice a doling out of reward and punishment according to merit, it was not necessary to bring in a verdict of guilty against the cosmos. It is quite true that man has seen in all ages the sun shine on the just and the unjust. But it is an easy reflection that the world could not turn round on individual merit, and that if few are so guilty as to deserve the agonies of grief that may come to all, still fewer deserve some of the simpler and most common joys of life. Behind the "self-hypnotised catalepsy" of the devotee of Brahma, behind the aspirations towards Nirvana, behind the "apatheia" of the Stoics, there was a recognition of this worthlessness of the individual:

[1] Wallace's "Darwinism," p. 39.

an equable acceptation of oneself as part of a process: a triumph of intelligence over selfishness.

When we come to Professor Huxley's account of modern thought with its "tendency to move along the old lines to the same results" —that is, to the results of Indian and Greek philosophy—an extraordinary omission becomes obvious. Modern thought pays at least as strong a regard as did Indian or Greek thought to the position of the individual, and the inequality of individual lots strikes at least as harshly as ever against abstract conceptions of justice. But there is a new factor, the consolations of the ideals of conduct expressed in the New Testament. These bring happiness within the reach of the most unhappy, and have altered in the most fundamental way the principles of ethics. If they come with the authenticity assigned them by dogmatists, there is an end of the matter. If they are the climax of an inner spiritual law, which man himself is constantly shaping and perfecting for his own guidance, there is also an end of the matter. For then they would form as integral a part of the general cosmic process as any of the ape and tiger methods, and it would be as absurd to speak of the battle between ethical man and the immoral cosmos as is the well-worn argument of those defending miracles, that a law of nature (gravity) is interfered with when a man lifts his hand.

I can sum up my criticisms of this address very shortly:—

Evolution does not move in cycles, but in a chain.

Man's capacity for pain is a fact in dependence on other facts.

The "immorality" of the cosmic process is at least a matter of doubt.

If the events on this world are part of the plan of a divine creator, speculation on the immorality of the little we understand is futile.

On the other supposition the highest modern ideals and the brightest results to be hoped for in the future are a direct outcome of the chain of evolution, and it is rhetoric with no philosophic basis to write of the struggle between ethical and cosmic nature.

Our consciousness of the process of ethical amelioration, just as the part our consciousness plays in it, is a part of the cosmic process.

P. C. M.

SOME NEW BOOKS.

THE PURSUIT OF HAPPINESS: A Book of Studies and Strowings. By Daniel G. Brinton, A.M., M.D., D.Sc. 8vo. Pp. 280. Philadelphia: David McKay. 1893.

IN his new book Professor Brinton, philologist, ethnologist, and Americanist—gifted with "the pen of a ready writer"—discourses as a physiologist, moralist, and philosopher. He takes for his subject the pursuit of happiness—to the Pessimist a chimera, to the Puritan a crime. Happiness he defines as not pleasure but built on it; its pursuit as the incessant prompting to a higher form of existence. Biologists have discovered, he states, that the avoidance of painful and the search for pleasurable sensations are the first principles of organic animal life, and those which have developed the amœba into the man, in whom it has blossomed into that self-consciousness to which he owes all the growth of his higher nature, his essentially human powers. "The yearning for joy is a cry of Nature." "It is the secret of evolution." Happiness is the reward of effort, but the truth of the Spencerian concept that "the greatest efficiency is the greatest happiness" is denied on the ground that it is historically false, for men most famous by their enormous personal capacity have certainly not been the happiest of their kind. Nor is childhood the happiest age, nor old age the wisest; there is a rabbinical saying, "He who teacheth the old is like one who writeth on blotted paper."

The fact that women are less happy than men is attributed first to inherent physiological differences and their consequences; secondly, to the errors of a false system of education. The author then proceeds to discuss the effects that our bodily and mental constitution, the laws of heredity, environment, and climatic influences produce on our happiness in general. Dr. Brinton does not, like Ibsen, make heredity accountable for everything, but shows how its sinister conditions can be ameliorated, if not actually annulled. He attributes his individual incapacity for the appreciation of music to the fact that musical instruments were excluded from the houses of his Quaker ancestors for five generations. The author then considers how far our happiness depends on ourselves and how much on others; reviews the pleasures of the senses, the emotions, the intellect; advocates the cultivation of Individuality as opposed to Egotism, the promotion of social happiness, and, in conclusion, recapitulates the consolations of the afflicted. In fact, Dr. Brinton's *Studies* might teach us all "how to be healthy and wealthy and wise." The *Strowings* consist of interspersed "Whitmanese" in the form of aphorisms more or less original and quaintly expressed, such as "Distrust the current estimates of great men. They alone are not tried by their peers." "Self distrust is nowhere more appropriate than in discussing difficult questions, and nowhere less displayed."

Dr. Brinton's latest contributions to physiology, phychology, and

ethics, give evidence of thoughtfulness, wide reading, and that intimate acquaintance with human nature characteristic of the wise physician. He has produced a philosophical, up-to-date manual of the "Pursuit of Happiness," which, according to the American Declaration of Independence, is one of the inalienable rights of man. If there is no infallible guide-book to happiness, we may at least learn from this one how to avoid that morbid physiological condition which has been termed the disease of the century—*Ennui*.

In Dr. Brinton's opinion, "Our happiest moments are those in which we believe we can realise our ideals," but it has been well-said by his fellow-countryman, Lowell, "Woe to that man, or that nation, to whom mediocrity has become an ideal."

AGNES CRANE.

THE EVOLUTION OF DECORATIVE ART. By Henry Balfour, M.A., F.Z.S., Curator of the Ethnographical Department (Pitt Rivers Collection), University Museum, Oxford. London: Percival & Co., 1893. Price 4s. 6d.

IT is appropriate that this interesting little volume should come from the Pitt Rivers Curator, for, until General Pitt Rivers designedly studied the art of modern primitive races of mankind, any connected theory of the beginnings of design and decoration was an impossibility. Mr. Balfour has kept closely to General Pitt Rivers' original idea—that of studying the decorative art of the most primitive people now in existence, believing that the methods employed by man in all ages in reaching at conventional design from simple imitation of natural objects have been very much the same. Mr. Balfour's close study of the Pitt Rivers collection since it came to Oxford has enabled him to get much more definite and exact evidence of this process. Shortly speaking, there are two leading methods in the process. Everyone who was not born since the age of typewriters has dropped a blot of ink on his paper, and has found himself elaborating the stain into some fantastic form. So primitive man, finding a knot in the blade of his paddle, has stained and shaped it into a pattern, and has reproduced the pattern more or less rudely along the face of the blade; or, having trimmed the bamboo shaft of his spear, his eye has been caught by the regular pattern formed where the leaves are cut off at the nodes, and he has coloured the markings and the fringe-like scratches made where they cut off the blades. The transition from this to pure ornament comes when the decorative knots or node markings are reproduced upon a blade or stick where their natural origin does not exist. The second method is also familiar. We have all heard of the game called "Russian Scandal," where a simple statement or story, whispered through a chain of persons, appears in utterly unrecognisable shape when the version of the last person is compared with the original. The author has given two most interesting and amusing illustrations in which this little game was played by successive persons, each copying his predecessor's version of an original sketch. So among savage people, some unclad Raphael imitates the human figure on a weapon or a gourd, and future weapon-makers copy his representation of the natural object, until, from a possibly successful picture, a pattern apparently purely geometrical is produced. We have chosen two typical illustrations of the way in which the author deals with his materials, but the book is so short and so readable that it would be unfair, by prolonging this account, to give any reader the slightest excuse for refraining from the book itself.

ZOOLOGY OF THE INVERTEBRATA. A Text-book for Students. By Arthur E. Shipley, M.A. Pp. 458, with many illustrations. London: Adam & Charles Black, 1893. Price 18s. net.

ALTHOUGH zoological text-books are strewn thickly as the leaves in Vallambrosa, it cannot be said that there is no room for Mr. Shipley's work. The smaller text-books are elementary and insufficient; the lapse of time and a quickly advancing science have left behind the once invaluable Claus. Others, English and foreign, are too much affected by the lamentable doctrine of selected types—a method that, however it be a necessary concession to the conditions of time and space in laboratory teaching, is pernicious in a text-book. Mr. Shipley makes a wise parade of the multiplicity of Nature. He does not send the zoological world spinning round on the earth-worm because there is great store of it in the back garden, nor regard the cray-fish as a prop of creation because it can be ordered from the fishmonger.

At the same time, in each larger group a fuller account is given of some important example in order that the most interesting modifications presented by other members may be set forth. Thus, to take an example, in the chapter on Nemertea, *Tetrastemma flavida* is most fully described, but in every point its characters are compared with the diverging characters of other members, and the diagrams and illustrations given are of general application, not special representations of the anatomy of the particular species. Or, again, in the section on Ascidians, the features of *Ciona intestinalis* are made the text, but particular description is avoided by constant reference to other Ascidians.

It may be said that a clear and definite idea of the groups is given, and that a student who reads this book along with the recognised courses of higher laboratory work will have a very good knowledge of the Invertebrata.

Following a time-worn custom, the author places at the beginning of each chapter a few lines in italics entitled "characteristics." These are given in the disjointed, unfinished phrases that invariable custom supposes tenable by the memory. Possibly they are necessary, but they seem a compromise between intelligence and the examination system. Mr. Shipley, it is true, calls them "characteristics," but they will be taken as definitions. A definition of a group of animals would not exist if the personal equation were allowed for, and it is more important that the mind of the student should be fixed on the connections than on the distinctions between the groups. His concept of the group must always be his knowledge of the animals in it. Curiously enough, the author, while giving "characteristics" for the Protozoa, Metazoa, and the groups like the Coelenterata, Porifera, and so forth, has not attempted them for the Coelomata and Acoelomata, but has remained content with the much more scientific method of writing a little chapter or section on them.

In classification, Mr. Shipley is convincing and ingenious.

The book is well illustrated, well indexed, and conveniently arranged.

THE LEPIDOPTERA OF THE BRITISH ISLANDS. By Charles G. Barrett, F.E.S. Vol. I., Rhopalocera. Pp. viii., 313, pls. xl. London: L. Reeve & Co., 1893. Price £1. 10s. (with coloured plates, £2. 10s.).

THIS volume on British Butterflies is the first of a work on our native Lepidoptera, uniform with Fowler's "Coleoptera" and Saunders'

"Heraiptera." No work on the most popular of insect-orders, on such a scale as the present, has appeared for many years. The great increase of collectors and observers, while it has added vastly to the facts available for such a book, renders the author's task of sifting and arranging his material more onerous than in former times.

The introduction on the Lepidoptera generally occupies only ten pages, and anatomical statements are made very loosely. We are told that "insects of this order are never provided with jaws," that "they take food . . . by means of a flexible proboscis or trunk which is really a long, hollow, double tongue," and that they are "also provided with a pair of palpi attached to the labium or lower lip." Not a word informs the student that the "tongue" and the labium are each formed by the union of parts of a pair of jaws.

The old arrangement of the families is followed in the main, the Papilionidæ coming first and the Hesperidæ last. It seems a pity that the issue of this work was not used to familiarise British lepidopterists with Bates' order, now generally accepted, in which the Danaidæ stand at the head of the series, and the Papilionidæ are relegated to the place required by their affinities, just before the Hesperidæ.

In the systematic and faunistic portion of the work, however, which is in such publications the principal consideration, Mr. Barrett's long practical experience ensures a valuable result. After a good and original description of each butterfly, and its larva and pupa when known, we are furnished with notes on the variation shown by the insect, its localities, and habits. The attention paid to varieties is the feature of the work, and many remarkable local or aberrant forms are figured in the plates. But there is comparatively little suggestion on the vexed question of the causes of variation. Possibly this may be furnished in later volumes.

Naturalists will read with considerable interest the excellent notes on habits and localities given with each species, and will note with regret how many of our finest butterflies are on the road to extinction, if not extinct already. The generation of entomologists who saw *Chrysophanus dispar* alive is now passing away, and it seems that the present generation will see the last of *Aporia crataegi*, *Lycæna arion*, and *L. acis* as British insects. On the other hand, we have several additions recorded. *Danais archippus*, *Lycæna bætica*, *L. argiades*, and *Hesperia lineola* are now considered as entitled to a place in our fauna, though, owing to the absence of the food-plant of its larva, the first-named butterfly can never be more than a casual visitor to our shores.

The figures of the butterflies are generally good, but the caterpillars are often drawn in an attitude very suggestive of "blown" specimens.

THE HAWKS AND OWLS OF THE UNITED STATES IN THEIR RELATION TO AGRICULTURE. By A. K. Fisher, M.D., U.S. Department of Agriculture, Division of Ornithology and Mammalogy: Bulletin No. 3. Pp. 210, pls. 26. Washington, 1893.

ILLUSTRATED by well executed coloured plates of twenty-six of the species described, this excellent little monograph of the diurnal and nocturnal birds of prey inhabiting the United States should prove valuable alike to the field naturalist, the agriculturist, and the game-

preserver. It may be hoped, indeed, that its issue will do something to check the lamentable slaughter of these birds, which, if not stayed in time, appears only too likely to reduce many parts of the United States to the same condition as the British Islands in regard to birds of prey.

In the introductory chapter, the author enters into general considerations regarding the feeding-habits of hawks and owls, and comes to the conclusion that while a few of the former group are harmful to the poultry-keeper and game-preserver, in the greater number of cases the benefits conferred by the members of both groups far outweigh their depredations. A few species, moreover, like the swallow-tailed kite and the Mississippi kite, are absolutely beneficial, feeding entirely on reptiles and other noxious creatures. While the owls are chiefly instrumental in keeping in check the various species of rodents which are so harmful to the agriculturist, the smaller Accipitrines do equally good service in ridding the fields of the swarms of grasshoppers and kindred insects by which they are too often devastated. With this unmistakable verdict in their favour, we trust that we may ere long hear the last of the offer of bounties for the wholesale destruction of these interesting birds.

The total number of species of birds of prey recognised in this volume as inhabiting the United States is upwards of fifty, among which seventeen are owls, while the remainder, inclusive of the osprey, are Accipitrines. Many of these, it need scarcely be observed, are solely American, while others, like the duck-hawk, are merely varieties of Old World species, and others again, such as the golden eagle, differ in no respect from their representatives on this side of the Atlantic. The chief diagnostic features of each species are briefly but clearly given; while especial attention is directed to the areas of distribution. The especial feature of the book is, however, the care which has been bestowed in ascertaining the precise nature of the food of each species; for which purpose elaborate tables are given, describing the contents of the stomach of a large series of specimens, reaching in some cases to over a couple of hundred. Such researches must have entailed enormous labour on the part of the author and his assistants, who, with the Department, deserve the thanks of all ornithologists.

R. L.

HANDBOOK OF BRITISH GUIANA. By James Rodway. 8vo. Pp. 93. Georgetown British Guiana, 1893.

MR. RODWAY is well known to the readers of NATURAL SCIENCE; in a series of articles full of life and interest he has made us familiar with the forests of British Guiana, and now he gives us a general description of the country, its inhabitants, climate, geology, fauna flora, industries, its past history, and its resources and capabilities for future progress. Besides a map and a chart showing the steamer routes, there are five-and-twenty excellent plates reproduced from photographs, bringing vividly before us, now the splendid falls of the Masaruni or the Kaieteur, now the solitary grandeur of Roraima, towering upwards like a gigantic castle from a slope six thousand feet above sea-level, an immense sandstone rock, with an area of 32 square miles; or we are looking down on the walls and roofs of Georgetown, or New Amsterdam, standing out sharp and clear in a setting of tropical vegetation, or it is the vegetation itself which forms the

subject of the picture, the dense tangle of the forest, a clump of palms, or the *Victoria regia*, with its broad tray-like leaves quite hiding the surface of the water.

Along the coast is a fringe of alluvium so fertile that alternation of crops is unknown, sugar-canes growing year after year in the same soil with hardly any deterioration. Plantains, Indian corn, yams, sweet potatoes, and a host of fruits ripen all the year round; at present hardly a tenth of this is under cultivation. Behind lie swamps choked with tall sedges, with here and there an island of sand on which a few trees and bushes manage to exist, or wherever the land begins to rise, grand clumps of the magnificent Eta palm (*Mauritia flexuosa*). Then come the sand-reefs, the white beach of ages long ago. Miles and miles of pure sand, washed clean as driven snow, dazzle the eyes in the glare of the noonday sun. Behind the reefs the flatness of the coast gives place to an undulating country, gradually rising to hills, and then mountains, and clothed for its greater portion with primeval forest—part of the wilderness of teeming vegetation which covers most of tropical South America. This wilderness abounds with riches. There is gold in the river beds, the working of which increases every year; but of incalculably greater value is the timber in the forest. For piles, wharves, and other structures more or less immersed in water, greenheart timber, says the writer, is perhaps the very best in the world. It is also classed with teak and oak by "Lloyd's" as the most suitable for ship-building. Mora is another first-rate timber for house-frames and ship-building, and although not so lasting as greenheart, "better even than oak for all purposes to which that excellent wood is applied." Wallaba is a very useful wood, as it is easily split into shingles and paling staves, and is far more durable than oak for the large vats necessary for water storage. Among the hundreds of varieties of woods found in the colony are some of the hardest and heaviest in the world, as well as others that are light. They vary, too, in colour from nearly black through the various shades of brown to almost pure white. Where the forests are broken are the savannahs, a paradise to the botanists, especially in the mountain region. Among foaming rivulets, running through banks of ferns and mosses, grow some of the most beautiful flowering plants—orchids, utricularias, droseras, and a hundred others—in the wildest profusion.

"The climate of the colony is on the whole a very pleasant one." The variations of the thermometer are but slight, generally from 76°—86° F., while on the coast the sea breeze tempers the greatest heat, and the heavy rains cool the earth and cause so luxuriant a vegetation that an arid tropical appearance is hardly ever present. Sometimes, however, a protracted dry season brings a drought. Destructive earthquakes and hurricanes are not experienced, while accidents from lightning are said to be almost unknown. Besides large areas on the coast, thousands of square miles of fertile river bottoms and valleys in the interior await cultivation. The great difficulty is the ever-present labour question. The emancipation of the slaves caused the ruin of many estate owners, and led to the plantations being abandoned, the negroes refusing to work at all except for wages which, with the low price of sugar, the staple product, no one could afford. Labour is now obtained by immigration from East India and China, but the

great want of the colony is settlers, energetic and industrious, with some capital, and prepared to rough it a little at first.

For further information on the colony, its beauties and resources, we must refer the reader to Mr. Rodway's very readable little handbook.

MODERN METEOROLOGY. By Frank Waldo, Ph.D. [Contemporary Science Series.] London: Walter Scott, 1893.

ON taking up this book, which is one of the Contemporary Science Series, the attention is at once fixed upon the title. Why should the author have chosen the exact title of a well-known existing work? This feeling of surprise is intensified by further acquaintance with the contents of the book. It is not an account of the present state of the Science of Meteorology. It neither completely supplements nor replaces Scott's and Abercromby's work, neither is it a picturesque and popular version. What is it then? It is a series of fragmentary essays dealing with certain branches of Meteorology. There is much detail given concerning the internal arrangements and routine of observatories, and there is a long account of the theories of Von Bezold, Hann, and Ferrel upon the circulation of the atmosphere. Indeed, the value of the book lies chiefly in this latter portion, which will be very useful to those English and American meteorologists who cannot read the works of the German authors themselves. Whole sections of the science are either omitted or referred to in a most sketchy manner. From the preface it is clear that Dr. Waldo is fully aware of the true character of his work, inasmuch as he remarks: "As I have been mainly a student of what may be termed the German School of Meteorology, I probably have not brought into sufficient notice the names and work of French, English, and Italian meteorologists." In other words, such advances as have been made by English, Scottish, French, and Italian meteorologists are scarcely referred to in a work called "Modern Meteorology"! Let the author change the title to "Some Chapters in Meteorology," or "Meteorological Essays," or some similar description of his work, and there will be less reason to quarrel with him; though, even then, it will be a matter of astonishment that anyone can reprint the absurd picture on page 193. In many ways the work is good and useful, but the general result is disappointing.

OBITUARY.

KARL SEMPER.

BORN JULY 6, 1832. DIED MAY 29, 1893.

WE regret to have to record the death of the eminent Professor of Zoology in the University of Würzburg. Born at Altona, near Hamburg, sixty-one years ago, Karl Semper was the son of a distinguished architect of that city. At an early age he entered the University of Würzburg, devoting himself especially to the study of Zoology. During the years 1859-61 he made a scientific exploration of the Philippines, and in the following year pursued zoological researches in the Pelew Islands. In 1864 he proceeded again to Mindanao, in the Philippine Islands, and in 1866 returned to Würzburg as Privat-docent in the University. Two years later Semper became extraordinary Professor of Zoology in the same University, and in 1869 he succeeded to the ordinary chair. A new Zoological-Zootomical Institute was subsequently erected, and of this the Professor became director in 1872. His most important work is the series of five volumes detailing the scientific results of his travels in the Philippine Islands, published in instalments between 1867 and 1886, partly in co-operation with other zoologists in special departments. In 1877 the Professor was invited to deliver a course of lectures at the Lowell Institute, Boston, and on this was based the work by which he is best known to readers in this country, "The Natural Conditions of Existence as they affect Animal Life." Apart from his own researches, Professor Semper made great contributions to the progress of Zoology by directing the work of his pupils into profitable channels; and the series of volumes of "Arbeiten," published by the Zool.-zootomical Institute of Würzburg, is filled with scientific results of great value.

THE death is also announced of CARL FRIEDRICH NYMAN, of Stockholm, author of the well-known *Conspectus Floræ Europeæ*; of HENRY ELIASON SEATON, the young and promising Assistant Curator of the Gray Herbarium, Harvard University; and of WILLIAM REID, of Nairn, a keen observer in ornithology and ichthyology, to whom many authors own much indebtedness.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Professor Baldwin Spencer, of Melbourne, is spending his vacation this year in Europe, leave of absence having been extended until December.

Dr. W. Deecke, the well-known palæontologist of Greifswald, has been appointed extraordinary Professor of Palæontology in the University of that city.

Mr. H. Yule Oldham, Lecturer in Geography in the Owens College, Manchester, succeeds Mr. J. Y. Buchanan as Lecturer at Cambridge.

Mr. E. B. Poulton, F.R.S., has been appointed Hope Professor and Curator of Entomology in the University of Oxford, in succession to the late Professor Westwood.

Mr. W. Fisher, late of the Department of Forestry in India, has been appointed Assistant Professor of Forestry in the Royal College, Cooper's Hill.

The honour of knighthood has been conferred upon Dr. B. W. Richardson, F.R.S. The University of Durham has conferred the Honorary Degree of D.C.L. upon Sir Henry H. Howorth, F.R.S.

The University of Pennsylvania conferred, on May 10, the honorary degree of "Doctor of Science" on Daniel Garrison Brinton, M.D., LL.D., of Philadelphia, Professor of Ethnology in the Academy of Natural Sciences of that city and of American Archæology in the University of Pennsylvania, in recognition of his numerous contributions to linguistics, ethnology and American archæology.

The University of Dorpat, now Jurjeff, is beginning to issue its publications, according to the Imperial decree, in the Russian language. We have just received what appears to be an important thesis on the development of the carpus and tarsus in mammals, illustrated by three beautiful plates; but the work is not accompanied even by an abstract in French or German, and it will thus be of little service to the majority of anatomists interested in the subject.

According to *Indian Engineering*, a Committee for the establishment of a Pasteur Institute in the Punjab Himalaya has held its first meeting at Lahore, when a working scheme was decided upon, and it was determined, in the first instance, to ascertain from the Government of India the conditions under which a Pasteur Institute, supported by the public, could be amalgamated with the Imperial Bacteriological Laboratory which is to be transferred from Poona to the Punjab Himalaya.

THE Rothschild Museum, Tring, has just received from Madagascar some limb-bones of the extinct struthious bird, *Æpyornis*. In size they exceed the largest known bones of *Dinornis* from New Zealand. They seem to indicate that the tibia of the Madagascar bird was relatively shorter than that in the New Zealand genera.

WE regret to learn that the fund collected by the Owen Memorial Committee is still insufficient to carry out adequately the resolutions adopted at the meeting of subscribers held early in the year. It is to be hoped that the proposal to place a marble statue of the late Sir Richard Owen in the great hall of the Natural History Museum at South Kensington will not lapse from want of pecuniary means; and we would urge all who have not yet subscribed but who desire to honour the memory of the deceased Naturalist, to forward their donations without delay to Sir William Flower, Treasurer of the Committee.

ACCORDING to a recently-issued circular, the total amount of the contributions received is only £935, and in the list of subscribers many well-known names are conspicuous by their absence. We are surprised to notice how small a proportion of Sir Richard Owen's former colleagues in the British Museum and other scientific departments of the Government have responded to the appeal. The name even of the present Hunterian Professor at the Royal College of Surgeons does not appear in the list.

MR. R. W. ATKINSON, for six years Honorary Secretary of the Cardiff Naturalists' Society, has resigned office, and Mr. Walter Cook succeeds him.

AT the anniversary meeting of the Linnean Society on May 24, the gold medal was conferred on Professor Daniel Oliver, F.R.S. On presenting the medal, the President, Professor Stewart, referred to the wide character of Professor Oliver's work, including besides his extensive contributions to systematic and geographical botany, papers on the morphology and anatomy of plants. The "Lessons in Botany" was mentioned as the most useful elementary book we have, and the President also referred to the thirty years' tenure of the chair of botany at University College.

FROM the Annual Report of the Essex Field Club for 1892 (*Essex Naturalist*, April–May, 1893), we learn the final arrangements for the amalgamation of the Essex and Chelmsford Museum with the Essex Field Club. The collections of the two societies are now in process of arrangement in the old Museum at Chelmsford pending the erection of new buildings. The subscribers to the old Essex and Chelmsford Museum have been admitted as Members of the Essex Field Club, which now increases its ranks by nearly a hundred persons. The headquarters of the Club will shortly be removed to Chelmsford.

AT the Annual Meeting of the Haslemere Natural History Society, held on May 20, Mr. Jonathan Hutchinson, F.R.S., was elected President for the ensuing year. The Society proposes in future to publish "Records of Lectures and Addresses" delivered at its meetings. A Museum is also in course of formation, but the Society cannot hope for much success in this direction until it is able to decline the offers of those well-meaning but mistaken donors, who are likely to convert it into a "curiosity shop."

DR. R. BLANCHARD, Secretary of the International Zoological Congress, announces that the subject for the first award of the prize offered by Czarewitch of Russia is "A Study of the Fauna of one of the great Regions of the Globe and its Relations to the neighbouring Faunas." Manuscripts must be received before May 1, 1895, and they may relate either to a Fauna in general or to one particular class of animals. The award will be made at the Leyden Meeting in 1895, on the report of a Committee consisting of Messrs. Milne Edwards, Blanchard, Bogdanov, Zograf, Jentink, Studer, and Bowdler Sharpe. Detailed particulars may be obtained from the office of the Congress, 7 Rue des Grands-Augustins, Paris.

THE following letters have been circulated this month among the Fellows of the Royal Geographical Society of London:—

"Savile Row, W.
"1st June, 1893.

"Dear Sir,—At the recent Anniversary Meeting of the Royal Geographical Society, in the course of a discussion on the admission of ladies to its Fellowship it was suggested, and the suggestion met with the approval of the meeting and was accepted by the President, that the best course would be to obtain the opinion of the body of the Fellows throughout the country. General R. Strachey—acting in the absence of Mr. Clements Markham—has consequently directed me to send you the accompanying note and postcard. A general vote, thus taken, is not formally binding on the Society, but its result will, there is every reason to believe, be acquiesced in and confirmed by a General Meeting, as a conclusion to the recent controversy.

"The admission of ladies to the Society is no new idea. It was accepted in principle as a measure both of justice and expediency by the Council in 1887. Its application was deferred to a convenient occasion, and that occasion arose when last year an eminent lady traveller, Mrs. Bishop (Miss Bird), declined to furnish a paper to a Society that would not acknowledge her as a geographer. I need not catalogue the many other ladies, from our Gold Medallists, the late Mrs. Somerville and Lady Franklin to Miss Edwards and Miss North, whose names are well-known to all interested in the literature and results of travel.

"The ladies the Council particularly desire to see admitted as Fellows are, in the first place, travellers who contribute to geographical knowledge, and also ladies who have so warm an interest in geographical progress that they desire regularly to attend our meetings and to use our library. Up to the present time, such ladies, if widows or unmarried, have only been able to be present at meetings by begging a ticket on each occasion from a Fellow. The Society has refused their subscriptions, and they have been debarred from the privileges accorded to members. Is not such a state of things an anomaly at a date when ladies are already admitted as members of a large proportion of the more important Scientific Societies of the metropolis, and of, I believe, all the other Geographical Societies in the United Kingdom and the Colonies? It appears in this light, not only to the Council, but also to Fellows of such experience as Lord Northbrook, Lord Brassey, Sir John Lubbock, Sir Alfred Lyall, Sir Richard Temple, and Sir William Flower, P.Z.S., and Director of the Natural History Museum, not to mention many others. In my own twelve years' experience as one of the Honorary Secretaries of our Society, I may venture to subscribe to their opinion and to express my belief that the admission of qualified ladies as Fellows will add to the prosperity and reputation of the Society, and greatly conduce to its main object—the growth of geographical science—and to geographical knowledge.

"Some of our Fellows have recently found a legitimate subject of complaint in the fact that, owing to the extensive use by Fellows of their privilege of admitting a companion, the best seats at the most interesting lectures have sometimes been half filled by ladies. It must be pointed out that the Council have recently taken a step that will remedy this grievance by modifying a bye-law, so as to empower the officers, on occasions of exceptional interest, to seat all Fellows before any guests. It would obviously be unjust to exclude qualified ladies from the Fellowship because unqualified ladies, admitted as visitors, have occasionally occupied an unfair proportion of our benches!

"The Council, deeply impressed by the importance of geographical knowledge to the generation of the English people, and noticing every day how its absence is felt in questions of politics, of commerce, or of emigration, trust you will be able to agree with their view, and to answer the question submitted to you in the affirmative.

"I am, yours faithfully,
"DOUGLAS W. FRESHFIELD, Hon. Sec."

"Royal Geog. iety,
"1 Savi.
ngton Gardens, W.,
5th June, 1893.

"Sir,—I am desired by the Council of the Royal Ge raphical Society to inform you that Mr. Freshfield's letter of June 1st was written and issued without their authority or knowledge, and that his action in so writing has been disavowed by them. The Council as a body have no wish to influence the judgment of any Fellow in the matter.

"I have the honour to be,
"Your obedient servant,
"R. STRACHEY, Vice-President."

We need not add to our remarks last month. So far as we can judge, all the Royal Geographical Society's difficulties have arisen from the usurpation by the Council of powers exclusively belonging to the Fellows assembled in a Special General Meeting; and the time for granting the Fellowship to ladies has thus been unfortunately postponed.

DURING the absence of Mr. Theodore Hughes in England, Mr. T. D. La Touche undertakes the duties of Superintendent of the Geological Survey of India. We briefly referred last month to the new orders under which the work of the Survey will for the future be directed more especially to subjects of economic interest, and we are now able to add some further particulars. It is ordered that two-thirds of the officers shall always be primarily engaged on the explorations necessary to complete the geological survey, and the remaining third on the practical investigation of mineral fields. In the latter case, the exploration will be confined to such preliminary researches as may be necessary to supply general information regarding their character and extent to capitalists and promoters, to whom will be left the responsibility for more detailed prospecting. According to the recently-issued annual report, the most important scientific work of the past year was that carried out in the Central Himalaya by two of the survey officers in co-operation with Dr. Diener, the eminent geologist of Vienna, and their joint researches are said to be certain to prove of high scientific interest. There were experimental borings for oil in Beloochistan and on the Indus, investigations into the Nerbudda coal-fields, the coal resources of Tenasserim, jade in Burmah, coal and rubies in the regions bordering on Yunnan, and much else of a similar character. The advance in the knowledge of Indian coal-fields has been great. The centres of production, which a few years ago were almost confined to Bengal, have now been extended to Assam, Punjab, Central Provinces, the Nizam's territory, and Burma. The survey has also done much to determine the character of the oil resources of the country, and soon the question will be set at rest whether any important or permanent supply of oil can be secured in Northern India.

CORRESPONDENCE.

The Recrudescence of Leprosy and its Causation.

As the readers of Natural Science would scarcely comprehend the full scope and purport of my recently-published volume, "The Recrudescence of Leprosy and its Causation," from the brief notice in the April number, I shall be glad to be allowed space for a summary of my conclusions, the result of personal investigation in many countries where this terrible disease is endemic. I can prefer this request with the more urgency from the fact that many of the sufferers from leprous vaccination are absolutely friendless, while most of them are without political representation and are unable to plead their own cause. In the pursuit of my inquiries I have been repeatedly entreated to bring their intolerable grievances before the English-speaking public through the Press, with a view of reaching the ears of Parliament and other legislative bodies.

These conclusions are as follows:—

(1.) That leprosy has greatly increased during the last half-century, and that it is prevalent in many places where it was formerly unknown.

(2.) That whilst the opinion of medical authorities and experts varies considerably on the subject of the contagiousness of leprosy, the preponderance of authority is in favour of the theory that it is not contagious in the ordinary sense of the term, but is communicable by means of a cut, sore, or abraded surface; and this view is confirmed by my own personal investigations.

(3.) That other alleged factors such as malaria, a fish diet, syphilitic cachexia, heredity, and insanitation are admittedly unequal to explain the rapid growth of the disease in certain of our Crown colonies and dependencies, as well as in other countries.

(4.) That on one point there is much agreement, and hardly any dissent, namely, the inoculability of leprosy; and that the view of leprosy as an inoculable disease, while it is most clear to those who take the malady to be due to a bacillus, is older than the bacteriological evidence, and is not dependent thereon.

(5.) That the most frequent opportunities of inoculating the virus of leprosy are afforded in the practice of vaccine inoculation, which is the only inoculation that is habitual and imposed by law; and that the evidence here adduced is calculated to show that vaccination is a true cause of the diffusion of leprosy.

(6.) That the official information, collected by interrogatories and otherwise, has not been hitherto of a kind to show how far vaccination has determined the amount of leprosy in recent times; and that any interrogatories that may be sent out in future should not be limited to ascertaining the effects, as regards leprosy, of hypothetically "pure lymph." When, on very rare occasions, interrogatories have been submitted, they have been framed to ascertain the results of a purely hypothetical system of vaccination, which is not anywhere discoverable in practice (*i.e.*, with pure lymph, and free from hereditary taint), and the replies are therefore futile and misleading.

(7.) That with the exception of two groups of cases—those adduced by Dr. Roger S. Chew, of Calcutta, and Dr. S. P. Impey, of Robben Island—those reported in this volume have not been the result of special investigations, but have cropped up accidentally in the course of medical practice, and in some instances have been published by practitioners, with apologies to the profession for presenting such unwelcome disclosures.

(8.) That the increase of leprosy in the Sandwich Islands, the West Indies,

the United States of Columbia, British Columbia, British Guiana, South Africa, and New Caledonia has followed *pari passu* with the introduction and extension of vaccination, which in nearly all these places, without previous inquiry or demand from the inhabitants, has been made compulsory.

(9.) That as leprosy is a disease of slow incubation, often taking years to declare itself, and in its incipient stages can be detected only by practitioners of large experience, it follows that, in countries where leprosy exists, there is great danger of extending the disease by arm-to-arm vaccination.

(10.) Leprosy being one of the most loathsome diseases to which the human race is subject, and being practically incurable, it behoves all interested in the public well-being to do their best to *prevent* its diffusion, and as a means thereto, to discourage the practice of vaccination on that ground, if on no other.

Devonshire Club, St. James's, S.W. WILLIAM TEBB.
24th May, 1893.

MR. HICK AND *Calamostachys Binneyana*, SCHIMP.

IN the letter which appears in the last issue of NATURAL SCIENCE (vol. ii., p. 480), Professor Williamson announces that the "true answer" to my article on "The Fruit-Spike of Calamites" (NATURAL SCIENCE, vol. ii., pp. 354-359) will be given in a memoir which he and Dr. Scott hope to present to the Royal Society before the close of the present year. May I, on my part, refer those who are interested in the matter to the forthcoming volume of the *Proceedings of the Yorkshire Geological and Polytechnic Society*? There, a paper will be found in which I have given a full description of the beautifully-preserved specimens on which the article is based, so far as it deals with *Calamostachys Binneyana*, Schimp., as the fruit of some form of *Calamites*.

THOMAS HICK.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, *now removed to* 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.*

All ADVERTISEMENTS *to be forwarded to the sole agents,* JOHN HADDON & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

ANSWERS TO CORRESPONDENTS.

HENRY JOHNSON.—Laurence Oken's library was sold by auction on 17 May, 1853, at Zürich. It consisted of 5,881 items, 600 of which were Historical, Classical, and Theological books, the rest dealing with the Natural Sciences.

C. W. K. (Leeds).—You will find that Hagen's *Bibliotheca Entomologica* (Leipzig, 1862) gives the majority of writings on Insects previous to the date of its publication. There is a good index, and you can find out the papers on the special group you want. After Hagen, the volumes of the *Zoological Record* should be consulted.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

The Exploration of East Africa.

AMONG the latest news of interest that reaches us this month is a communication from Dr. J. W. Gregory, giving a brief account of his expedition in East Africa up to the date of writing, May 8. It appears that he left Mombasa on April 23, taking with him a small force of only forty men, thus hoping the more easily and quickly to traverse any territory where food was scarce. He passed Kilwezi, the Nyika plateau, Nzoi, Kilungu, Zuni, Machakos, the Athi river, Lanjoro, and through the Kikuyu country to Fort Smith. His present letter reports very satisfactory, and is dated from Longenot, 450 miles from the coast, and to the north of Lake Naiwasha, Masailand.

Dr. Gregory has had little trouble with his men; there have not been any desertions; and food, though not abundant, was to be had; indeed, he says that on the banks of the Malewa he lived in luxury, game and birds being abundant, and a whole sheep costing less than a 2lb. tin of corned beef. There are no vegetables, except Indian corn and dry beans, of which he bought a ton at Fort Smith. His progress has been much delayed by heavy and persistent rains.

Dr. Gregory states that he has found *Senecio johnstoni* (a remarkable composite tree which attains a height of from 20 to 25 feet, previously recorded three times from Kilima-njaro, at heights over 8,500 feet) in considerable abundance near Naiwasha. His traverse of, and occasional branch excursions into, the Ulu Mountains have yielded considerable evidence with regard to the earth-movements of that district. He has found traces of the former existence of a vast lake to the south of Naiwasha, which was probably destroyed by the east and west line of elevation to which the volcanoes of Longenot and Kenia were due.

The hostility of the Masai, who happened to be on the war-path some 9,000 strong at Naiwasha, at the time of his visit there, compelled Dr. Gregory to leave that district more quickly than he wished, and this prevented him from making any examination of the lake fauna. He reports, however, that he found the site of a factory of stone implements used by some prehistoric tribe, and has traced one of their trade routes across a pass still used by some caravans. He expected to leave Lake Barengo (his farthest point) by the end of May, and to spend June and July exploring in the Aberdare Mountains, and on and around Mount Kenia. No further news can be expected of him until he reaches Brindisi on his return journey, about the end of September, for it is more than likely that he will miss the outward mail from Mombasa at the end of August. Save for a chill, caught while crossing a river, Dr. Gregory's health since he left the coast has been excellent.

"Borderland."

Mr. W. T. Stead has sent us a circular containing information about his new Quarterly, *Borderland*, and a letter asking our views on the subject, as we are among those "whose opinion weighs much with the public," and from whom may be sought "such wise words of guidance in this new enterprise as he can collect from the most eminent of his fellow men."

It were easy to treat Mr. Stead's adventure with mere chaff or with ridicule. But Mr. Stead already has exploited the credulity of the public in the matter of red and blue electricity, and so competent a journalist doubtless will succeed in exploiting the superstitions of the class for which he caters. In this process it is "even money" that many will believe in the utility of his methods, while the odds are not long against his own belief in them. So he shall have our "wise words." He proposes to form a series of circles of students to study occult phenomena, and anyone may join a circle on payment of ten shillings per annum. The occult phenomena to be studied by this "veritable college of the occult sciences" are, we gather from the letter and circular, "the powers of the unconscious mind, multiple personality, invisible intelligences with whom we may profitably enter into communication, and the persistence of the individual after death."

These are questions of great interest, and the collection of data concerning them is of first-rate importance. But the evidence of those not trained to observe is misleading about the plainest occurrences: it will be absolutely worthless about phenomena so complicated by subjective and objective deception as these phenomena notoriously are. More than one expert in some science has been attracted to the investigation of spiritualism and has abandoned it, because of the ensnaring and degrading fraud associated with its votaries. And a college

chosen on Mr. Stead's plan will be a mixture of confiding simplicity and designing impudence. For the serious treatment of the obscurer questions of the mind there are in abundance laboratories of physiological psychology, trained specialists in the laboratories, and technical journals for the publication of their researches. The idea that science "has contemptuously relegated to superstition" such problems exists only in the minds of busybodies who have been snubbed. There are competent experts at work, and were the geologist to lay down his fossil and the anatomist his scalpel to follow Mr. Stead into the wilderness, they would aid only the wind to shake the reeds until such time as they turned to the stoning of the prophet who led them out. As for the "dangers" surrounding the study of occult science, let no fluttered neophyte be deceived. The dangers are real; though he will not be thrilled by any devil of his own raising, if the germs of imbecility lie in him they may well be scared into activity by the fraud of his associates.

Sierra Leone.

The Botany and Geology of Sierra Leone are the subject of a recently-issued Colonial report compiled by Mr. G. F. Scott Elliot, and embodying the results of his observations as botanist to the late Anglo-French Boundary Commission. Mr. Elliot has already laid before the Linnean Society the purely botanical results of the expedition, and in the present report, dealing with the economic aspect, has brought together a good deal of interesting and valuable information on the native or cultivated plants of the colony. A number of geological specimens have been named and described by Miss C. A. Raisin, of University College. The only mineral of importance is iron, of which the country apparently contains a very large amount. A specimen of titaniferous ore from a rich belt in the hills behind Sierra Leone, proved on analysis to contain 52 per cent. of the metal.

In speaking of the fertile alluvial soil along the coast and rivers, the effect of the mangroves in reclaiming land from the sea is described: "the trees seem, in fact, to have been designed by nature to change any bay or indentation of the coast-line into fertile soil." They require brackish water, and wherever a mudbank is in process of formation, there they grow, gradually advancing seawards as the silt accumulates. The trunk divides at the base into six or seven curved buttress-like roots, each of which by repeated sub-division covers a wide area with grasping supports. Moreover, from every branch long hanging roots descend vertically, dividing about the level of high tide into grasping fingers, which grow down into the water and take root so firmly in the silt that they cannot be torn up by any ordinary force of current. This meshwork of roots and rootlets catches and holds the fallen leaves of the mangroves, and all the

silt and soil in the water, and the accumulation rapidly advances. Gradually the level of the soil thus formed rises above high tide, and then the mangroves, which require a constant supply of brackish water, die off, and the whole grove advances seaward, leaving behind it a mass of rich vegetable alluvial mud, better suited for rice than probably any other soil in existence. Tobacco, sugar, and cotton would, doubtless, also do well. The part bordering the sea is adapted for the oil palm, and, presumably, also the coco-nut.

The rest of the country consists chiefly of low rolling hills or downs, covered by almost impenetrably thick bush 20 to 30 feet high, or tall grasses from 8 to 10 or even 15 feet. The upland plateaux, above 2,000 feet, would probably form excellent grazing ground.

Mr. Elliot concludes that, from a botanical point of view, the country is more suited to most valuable tropical plants than either Jamaica or Mauritius, but the climate is in most parts dangerous for Europeans, and neither energy nor physique can be kept up sufficiently to make proper use of its natural advantages.

Discovery of an American Type of Extinct Mammal in the Balkans.

As our palæontological readers are probably aware, those remarkable gigantic extinct Perissodactyle mammals known as the Titanotheriidæ (together with the Uintatheriidæ) have hitherto been regarded as confined to the Tertiaries of North America. Herr Vacek had, however, suggested, as far back as 1877, that a species from the Lower Tertiary of Transylvania, described under the neat and concise name of *Brachydiastematherium transilvanicum*, which has been generally regarded as a member of the Chalicotheriidæ, might really belong to the American family.

In a recently-published paper (*Sitzb. Akad. Wien*, 1892, pp. 608–615) Dr. F. Toula describes two mammalian teeth from Miocene beds at Kalina, near Sofia, in the Balkan Peninsula, which seem to leave no doubt that the family in question was really represented in the Tertiaries of Europe. The specimens consist of two very large lower molar teeth, which pertained, as shown by their different degrees of attrition, to as many individuals; one being the last and the other the penultimate of the series. That they belong to Perissodactyles there can be no question; and the last tooth differs from the corresponding molar of all the large Perissodactyles of Europe in possessing a third lobe, whereby it is at once distinguished from *Chalicotherium*. Dr. Toula refers the specimens provisionally to the American genus *Menodus*, under the name *M. rumelicus*,—a reference which we believe to be perfectly correct, save that the name ought to be *Titanotherium rumelicum*. He also supports the view that *Brachydiastematherium* is likewise a member of the same family.

We had been inclined to flatter ourselves that we were fairly well acquainted with the mammalian palæontology of Europe, and that there was not much hope of the discovery of any strikingly new types. However true (or false) this may be in regard to the mammals of Western Europe, it is quite certain that it will not hold good for those of Eastern Europe, where, doubtless, a large field awaits the investigator. That it is in this half of the Continent that American types would naturally be expected to occur, is self-evident, if the view that the migration from the one hemisphere to the other took place by way of Behring Strait be correct. The occurrence in Central Asia of deer closely allied to the Wapiti, and of an alligator whose nearest living ally is to be found in the Mississippi, are both strong evidences in favour of this view, which receives additional support from this new and most interesting discovery. We trust that ere long upper molars of the new fossil will be forthcoming, whereby its affinities may be more thoroughly indicated.

Plant Pitchers.

The recently-issued number of the *Annals of Botany* (June) contains two papers dealing with the pitchers of *Dischidia rafflesiana*, a twining epiphyte of the family Asclepiadaceæ, growing on trees in the forests of the Malay Archipelago. Mr. Groom has studied the live plants at Singapore, and also worked with spirit material in this country. Dr. Scott and Miss Sargant, the authors of the second paper, have had the advantage of living specimens now growing at Kew, sent two years ago from Java by Dr. Treub.

Study of the anatomical structure completely confirms Treub's view that the pitcher is a modified leaf, the upper surface of which grows more rapidly than the lower, so that the outer surface of the pitcher represents the upper surface of the leaf, and the inner surface of the pitcher the lower surface of the leaf. The mouth is small, with deeply incurved lips, and directed sometimes upwards, sometimes downwards. The stalk gives off roots which enter and ramify within the cavity, which contains a quantity of earthy particles, small stones, fragments of leaves, etc., and frequently water. Dead and decaying insects were also found in small quantity, and often ants, the latter sometimes abundantly. The first author thinks that the solids are mainly brought by ants, a view with which Mr. H. N. Ridley agrees, regarding them as the remains of ants' nests; having observed also that the earth in the pitchers resembles that at the foot of the host tree. As regards the function of these structures, Delpino's view—that the plant is carnivorous, the pitchers serving to catch insects, which are drowned, and then help to nourish the plant—meets with no support; as Treub has already objected, the roots afford a most convenient way of escape to any insects which might find their way into the cavity. There is, indeed, no provision either

for attracting or retaining insects, except, perhaps, the allurement of a shady resting-place; and Mr. Groom is inclined to lay stress on the last point, suggesting that the existence of species with merely concave leaves indicates the evolution in the pitcher of an organ more perfectly adapted to provide shelter for the ants on the one hand, and on the other to secure for the use of the plant the material collected by them. At the same time, he recognises the function of storing up rain-water and the substances washed down therewith. Dr. Scott and Miss Sargant also attach considerable importance to the detritus in the pitchers, which is proved to serve as a food-supply. They, however, find no evidence of a myrmecophilous (ant-loving) habit, and agree with Treub that the detritus is washed in by the rain, the pitchers serving to collect rain-water, and in a less degree to economise the water of transpiration. The upright pitchers, they remark—those, namely, with the mouth at the base—"can have no other function than to store up the water given off as vapour in transpiration. We have shown, on anatomical evidence, that the inner surface of the pitcher is the chief transpiring surface of the plant. The condensed water of transpiration is undoubtedly re-absorbed by the roots." It is difficult to follow this last observation. A pitcher turned topsy-turvy could only store water in the reflexed rim, and as moisture clinging round the roots, which latter would doubtless be soon re-absorbed; but why should the vapour transpired condense to any appreciable amount within the pitcher? What plays the part of condenser? Surely the economy in water of transpiration is effected merely by the restriction of the area into which it is transpired, which, by becoming a still atmosphere saturated with water vapour, will retard further transpiration.

The same authors give an interesting account of the purple colouration on the inner surface of the pitcher, which contrasts strikingly with the pale green of the outer surface. Spectroscopic examination of the purple layer, also of an alcoholic extract of the pigment, showed a considerable absorption of the yellow and green, and almost complete absorption of the blue and violet rays. Hence the green and purple layers between them cut off almost all light from the interior of the pitcher, which thus forms a dark chamber, into which the negatively heliotropic root is likely to be attracted.

Fungus Parasitic on a Beetle.

Professor Alfred Giard contributes to the *Bulletin Scientifique* (vol. xxiv.) a study of *Isaria densa*, a fungus parasitic on *Melolontha vulgaris*, the cockchafer. He published two years ago in the *Comptes Rendus* a notice of this fungus, and the present essay is partly an amplification of it and partly a continuation of his experiences. This research has been inspired with the hope of waging a successful war,

in the interests of agriculture, on the cockchafer by wholesale infection of the larvæ with this fungus. Such methods have been tried, as everyone knows, by Continental mycologists and bacteriologists in certain cases, from the Australian rabbit downwards. In this country very little has been said or done, biologists being mindful, no doubt, of the luminous idea evolved by an eminent member of their body, and gravely proposed to his French colleagues, that they should exterminate the Phylloxera by means of the yeast fungus! No wonder if there has been a dearth of ideas on the subject in this country since then. M. Giard, however, has made a truly scientific study of the fungus and its artificial culture, and of the possibility of employing it in the manner indicated. It certainly deserves to be rewarded with a practical success. The great difficulty in the way of most cases of the kind is, of course, that which has at all times prevented the proper testing of the celebrated method of catching birds by putting salt on their tails. Every mycologist knows, however, the extraordinary powers of multiplication possessed by fungi, and the extraordinary diffusion of their spores. By assisting nature with artificial cultivations of the fungus, M. Giard has obtained a gratifying success so far as his proposal has been tested, and his method has plainly numerous advantages, as a natural and otherwise harmless one, over the usually futile one of sprinkling chemical insecticides. Such methods as M. Giard's deserve not only success but further developments, and it is to be hoped that they will not be hindered by vain talk of the dangers of "letting loose the germs of disease" generally indulged in by newspaper writers, who are willing to believe that one bacillus differs from another only in degree of rapacity in attacking the human body. To assist nature in such affairs is surely better than to squirt petroleum and other chemical (patent) insecticides over our gardens and fields. Some of these have a mischievous effect on the soil, and most have no measurable effect on the parasite; all are "quack medicines." For example, it was once proposed to cure the potato disease by the application of a substance costing about £17 per acre, as if this were the difference to the farmer between a good crop and a bad one of potatoes; moreover, the substance in question was useless for the purpose. Agriculturists should, therefore, welcome the development of all such rational proposals as this one of M. Giard, the fruit of much observation and ingenuity.

Algæ.

The second part of the *Phycological Memoirs* has just been published and amply fulfils the promise of the first number—indeed this part seems to us to be the more interesting of the two, dealing, as it does, with many of the vexed questions in the study of Algæ, which can only be settled satisfactorily in a place of such vast resource as the

British Museum. It is interesting to note that a letter confirming the results of the investigation of *Splachnidium* has been received by the authors of the new natural order, published in the last part. Mr. Laing writes from New Zealand describing the escape of the zoospores from the Splachnidian sporangia, which he has himself witnessed. The first paper in Part II. is entitled "Notes on the Morphology of the Fucaceæ" (*Coccophora* and *Seirococcus* by Miss A. L. Smith, *Notheia* by Miss Mitchell, *Xiphophora* by Miss Barton and *Sarcophycus* by Miss Whitting), and goes far to clear up points left undecided in this large and important order. It is prefaced by the editor, who explains that the authors of these notes have investigated material of genera unexamined or left uncertain by Oltmanns in his account of the Fucaceæ, and the result is in some ways confirmatory of Oltmanns' expectations. We are glad to see that the two genera *Ecklonia* and *Scaberia* are also to be examined, since the fact of finding oogonia on branched hairs in the female conceptacles of *Sarcophycus* would lead us to expect interesting results from the investigation of a genus so closely allied as *Ecklonia*. *Notheia* is also to receive more attention in a future number, since we have here an example of a degraded Fucaceous alga, if not a parasite, at least suspiciously attached to its host by a wedge of tissue like a haustorium.

A contribution by Miss Whitting on a new species of *Chlorocystis* shows careful investigation. The occurrence of parasites on Algæ is becoming a somewhat favourite field of research, and the new species here described is the only one of this genus which has hitherto been found to inflict injury on its host. It is a pity that this clever worker has not cultivated a more impersonal style of expressing herself. Possibly Mr. Bracebridge Wilson, who supplied the material to the British Museum, may be able eventually to report more completely on the life-history of this new parasite.

The editor (Mr. George Murray) has three papers—one throwing light on the much-disputed question of "fasergrübchen," or, as they are called here, cryptostomata. This, taken together with Miss Mitchell's paper on *Hydroclathrus*, places the character of these bodies in a clearer light than has hitherto been the case, and we hope Mr. Murray will not allow the matter to rest here, but will pursue the investigation still further and place the question of the real nature of cryptostomata beyond all dispute. His two remaining papers are, "On Halicystis and Valonia," and "A Comparison of the Marine Floras of the Warm Atlantic, the Indian Ocean, and the Cape of Good Hope." The former treats of *Halicystis ovalis*, Aresch.—a generic type new to Britain—and compares the morphology of this Siphoneous genus with that of *Valonia*. The remarkable reproductive organs of *Valonia*, noted in the living state some years ago by the author in the West Indies, are here described for the first time. Dr. Schmitz's remarks on the position of the genus *Halicystis* are incorporated in this paper.

In the comparison of the three Marine Floras, Mr. Murray is dealing with a subject on which he has already published several papers; and this one, embodying the results of a large amount of work, is a valuable contribution to a subject which has been very little studied.

The illustrations to this part of the *Phycological Memoirs* are equal to the last, and in both cases excellent.

We look forward to welcoming before very long the third part of this interesting book.

Popular Geology.

A SLANDER action of some interest has recently been tried in the Court of Session in Edinburgh. A County Council lecturer, Mr. Alexander Johnstone, has been explaining the geology of soils to the "blameless Hyperboreans," and in the course of this, according to Sir Arthur Grant of Monymusk (defendant), he said certain wild and whirling things about the mistake of Providence in not putting more phosphates into the soil of Aberdeen, and made certain criticisms of the Biblical account of the Creation. The plaintiff denied having said such things, though he did say in reply to a question that he was not there to defend the first chapter of Genesis, and possibly he remarked that the account he would give of the creation of the world as proved by science might be more interesting than that given in the chapter referred to. Sir Arthur Grant had anyhow taken the occasion to reprove clergymen for quarrelling among themselves instead of uniting against men like plaintiff, and the Court seems to have agreed with him, and given him the verdict. We should greatly like to hear Mr. Johnstone's, or anyone else's account of the creation of the world "as proved by science." Sir Arthur Grant might then be shown as good sport by scientific men as any of the churchmen now militant in the north could exhibit. Such a match (Dr. Kinns on "Moses and Geology" barred, and Sir Henry Howorth, F.R.S., closured on the "Mammoth and the Flood") may be commended to the organising committees of sections in search of an exciting discussion for the British Association. It would be as seemly as a recent discussion on the Hygiene of Stays. So long as public lecturers regard the book of Genesis as the lamented Mr. Dick regarded King Charles I.'s head, they may expect a return innings, and it is to be deplored in any case that such matters should find their way into the Courts of Justice.

A correspondent sends us a newspaper-cutting containing a condensed report of this trial, with the remark that "if such bigotry and ignorance of geology prevails in Scotland, it is time some geological missionaries should enlighten the people." Our correspondent forgets that the countrymen of Hutton, Lyell, Murchison, the Geikies and others have already sent missionaries south in this matter, and they might do so again!

Dr. Herdman contributes to the *Conchologist* for June 24 an interesting confirmatory note as to the views of Giard with respect to the mimicry of the mollusc, *Lamellaria perspicua*. "The Mollusc," he says, "was on a colony of *Leptoclinum maculatum*, in which it had eaten a large hole. It lay in this cavity so as to be flush with the general surface; and its dorsal integument was not only whitish with small darker marks which exactly reproduced the appearance of the *Leptoclinum* surface with the ascidiozooids scattered over it, but there were also two large elliptical clear marks which looked like the large common cloacal apertures of the Ascidian colony." Professor Herdman did not notice the *Lamellaria* until he had by accident partly dislodged it in pricking the *Leptoclinum* from the stone. He points out that in this case we have the curious result that the *Leptoclinum*, while itself protected by sharp spicules against certain enemies, is thus armed to some extent for the benefit of the *Lamellaria* which preys upon its vitals.

We have received from Messrs. Hedley and Suter a copy of their "Reference List of the Land and Fresh-water Mollusca of New Zealand" (*Proc. Linn. Soc. N. S. Wales*, ser. 2, vol. vii., 1893, pp. 613–665), which enumerates no less than 184 species, as against the 15 first recorded by Gray in 1843. This jubilee list is therefore interesting in that it affords an index of the progress made in the study of an important section of Antipodean Malacology within half-a-century of its inception. The authors maintain that in proportion as the New Zealand fauna becomes known does its insularity stand out, and they enumerate no less than fourteen well-known molluscan genera which have been erroneously imposed upon it, though whether, as they say, *Helix* be one of them, very much depends upon whose classification of a very difficult group be followed.

Those interested in the microscopic details of the structure and contents of the plant-cell will find matter to their taste in the "Beiträge zur Morphologie u. Physiologie der Pflanzenzelle," edited by Dr. Zimmermann, of which the first volume has recently appeared. It includes, in 322 pages, fifteen separate communications, for twelve of which the editor is himself responsible. Among the items discussed are the internal structure of leucoplasts, which are not always homogeneous as hitherto supposed, but sometimes contain well-defined spherical bodies or *leucosomen*. Another body so far unrecorded, the *granula*, is found to be of widespread occurrence in the assimilating tissue of plants. The proteid-crystalloids, their detection, distribution, and properties, are dealt with in two of the longest papers, while the chromatophores of variegated and chlorotic leaves respectively form the subject of other two. As regards the last mentioned, the author

demonstrates the presence of small, colourless corpuscles in the leaves of plants which have had no iron salts in their food; on supplying an iron tincture the corpuscles grow and become green. There is also a short account of the properties, reactions, and distribution of the oil-plastid. Two papers refer to the growth of the cell-wall, and one, by C. Correns, is concerned with the internal structure of the cell-membrane of some Algæ. The last-named author also supplies an account of some points in the structure and life-history of a green fresh-water alga, *Apiocystis Brauniana*, Naeg. The volume is well illustrated with five coloured plates and 23 figures in the text.

In the *Journal of Botany* for July, Mr. Rendle has a note on a potato which is depicted bringing forth young potatoes from its interior through clefts in the skin. The occurrence which, though previously described, has never been satisfactorily explained, is now said to be due to the production of shoots from the base of the bud or "eyes" as a result of the continued removal of the normal shoots. The tubers were stored in a cellar as usual, and the sprouts which appeared in the spring were plucked. In the case described the potato, perhaps an exceptionally vigorous one, devised a new means for using the large quantity of soluble nutrition which was being produced to supply the growing shoots, and as it was not allowed to form aërial shoots, developed from the base of the bud where the chemical processes would be specially active, tubercle-bearing shoots penetrating the substance of the mother tuber, and providing themselves with roots and a corky protecting covering in the same way as if they were pushing through the soil.

The writer refers to a similar physiological occurrence in the Mexican *Agave*. The Indians cut off the large spike-like terminal inflorescence at an early stage and then collect the sugary liquid which the plant continues to pour into the scooped-out top of the axis till it is exhausted, withers, and dies down. In both cases the chemical processes are suddenly rendered useless by removal of the organ for which the soluble nutrition was required; in both these processes continue, in the *Agave* for the gratification of the natives, whereas in the potato in question a new means for its use is devised. Of course, as the young tubers grow the mother shrivels proportionately, having to nourish its progeny from its own tissues.

We regret to observe that the editor of the *Journal of Botany* has taken to heart, as a serious criticism, a playful, but complimentary note on the abundance of cryptogamic papers in his pages, which appeared in Natural Science for June. It has, however, given him the opportunity of boasting the proud traditions of the *Journal* in the matter of fair play—a thing well-known to all botanists, but not made more savoury by an editorial announcement.

In a supplement to the *Leeward Islands Gazette*, Mr. C. A. Barber reports on the failure of the cacao crop in Dominica in the season of 1892–93. The failure is apparently not the result of any specific disease, but partly due to a reaction after a previous very successful season, and partly to a slovenliness in cultivation which is general in the island, and tends to induce an unhealthy tone in the trees, whereby the effect of climatic or unfavourable local influences is exaggerated.

Mr. H. O. Forbes' memoir on "The Chatham Islands: their Relation to a former Southern Continent," appeared last month, not in the Geographical Society's *Journal*, but in the *Supplementary Papers* of the same Society.

We understand that a shaft is to be sunk at the Dover Boring, at an estimated cost of £50,000. It was chiefly owing to the fact that the work was entirely under the control of Mr. Brady and Sir Edward Watkin that the boring was ever completed; it will be interesting now to see whether, under a board of directors, so much will be accomplished with so steady an end in view as was the work in which only two participated. Professor Boyd Dawkins is, we believe, one of the board.

We are glad to observe that our reference to the neglected state of the geological collection in the Museum of the University of Malta has attracted some attention in that benighted island. We may assure those interested that our information was not obtained from a resident, but from a geologist who is acquainted with most of the University collections of Europe, and who tells us that that of Malta was, five months ago, in a more disgraceful condition than any other he had visited. A correspondent, who states that he writes "officially," but at the same time requests that his name may not be divulged to anyone, points out that Mr. John H. Cooke is teacher of Mathematics and English in the Lyceum, not lecturer in the University.

The Geological Committee of Russia has just marked the completion of its first ten years' work by the issue of a geological map of the empire, produced chiefly under the direction of Drs. Karpinsky, Nikitin, Tschernyschew, Sokolow, and Michalski. The map bears 45 district notations, partly in colour, partly in signs, and the scale is about 40 miles to the inch. Much of the country is monotonous, being covered with post-Tertiary deposits, but the more striking features—such as the limit of extension of erratic blocks and the transgression of the old marine deposits in the Caspian region—are all duly indicated.

THE Geological Survey of Queensland has recently issued a report on "Geological Observations in British New Guinea in 1891," by A. Gibb Maitland, with three maps and two plates of sections. The formations now known are, besides coral reefs and surface deposits, the Post-Tertiary Kevori Grits, the supposed Tertiary Port Moresby Beds, and the Boioro Limestones of undetermined age, with metamorphic and igneous rocks. The coral rocks present no features of special interest. The Boioro Limestones are doubtfully placed beneath the Port Moresby Beds, and Mr. Maitland discovered no fossils; they sometimes contain flint nodules. Useful details are given, and the report concludes with a bibliography.

PALÆONTOLOGISTS will be glad to learn that Professor K. A. von Zittel has now completed his *Handbuch der Palæontologie*, and the last two parts will shortly be issued. His general account of the distribution of the extinct mammalia has just been published in advance in the *Sitzungsberichte* of the Bavarian Academy of Sciences, and a translation will shortly appear in the *Geological Magazine*. So valuable a comparison of the extinct mammalian faunas of different parts of the globe cannot fail to arouse a considerable amount of interest among those who have specially studied the subject of distribution; and the general survey now before us is one that has long been urgently required.

COMMENCING with the view that during the Secondary period the entire globe was inhabited by Multituberculates and Polyprotodont Marsupials, Dr. von Zittel considers that early in the Tertiary three great centres of mammalian development and dispersion were established. The centres in question were (1) Australia, (2) South America, and (3) Europe, Asia, Africa, and North America. Commenting on the alleged connection of South America, South Africa, and Australia by means of an Antarctic continent, he urges that had such connection taken place during the Tertiary period we ought surely to have found South American types of Edentates and Perissodactyles in Australia; and it must be confessed that those who urge the doctrine in question will have much difficulty in attempting to disprove the contention. At the same time, the evidence of the fossil Marsupials of Argentina shows that at some time there must have been a connection between the Australian and Neotropical faunas.

Special attention is directed in the memoir to the extinct faunas of Argentina and Patagonia, in the course of which it is shown that if the earliest of these be correctly assigned to the Eocene period, its mammals had attained a degree of specialisation far and away ahead of their North American and Old World contemporaries. All the

Ungulates, for instance, had attained tall-crowned or hypsodont molar teeth, and the same was the case with the Rodents, which had likewise discarded milk-teeth. Indeed, *Homalodontotherium* (we trust we have spelt the name correctly) is the only form which can be regarded as a generalised one; most of the Ungulates having the digits reduced to three, and the carpus of the interlocking type. The list of this earliest Santa Cruz fauna, as it is termed, includes only Marsupials, Edentates, Toxodonts, Typotherioids, certain aberrant and peculiar types of Perissodactyles, Hystricomorphous Rodents, and Primates allied to those now inhabiting South America; and the same holds good for the overlying Patagonian fauna, which has been correlated with the Miocene. All the genera found in these formations may accordingly be regarded as having originated in South America. When, however, we reach the still higher beds of Monte Hermoso and the Pampas, which have been identified with the Pliocene, we meet for the first time with Tapirs, Horses, Llamas, Deer, Mastodons, Murine Rodents, and true Carnivores, which are evidently immigrants from North America, where they occur in strata devoid of the remains of any of the above-named exclusively Neotropical forms. If further confirmation be required that the connection between the two halves of the continent did not take place before the Pliocene, it is forthcoming by the evidence of an incursion of South American Edentates into North America during that epoch. It may be added that the occurrence of monkeys belonging to the Cebidæ in the earlier Argentine Tertiaries affords some confirmation of the view that the monkeys of the Old and New Worlds have had an independent origin.

Palæontologists in the Argentine Republic have little opportunity for comparing their fossils with similar remains discovered elsewhere, and certain unfortunate rivalries have led to much confusion in the nomenclature even of the best-known genera and species. We are thus glad to announce that Mr. R. Lydekker, under the auspices of the Royal Society of London, leaves England on August 24, to examine the fossil vertebrata in the museums of Buenos Ayres and La Plata. He goes in response to the special invitation of Dr. F. P. Moreno, founder and director of the great museum in the new city of La Plata; and with his great experience of the Old World fossil vertebrata, he ought to render much service to Zoology by personally revising the work of his colleagues in the remote republic.

I.

Rainfall and the Forms of Leaves.

THE following notes are taken from an admirable paper by E. Stahl in the *Annales du Jardin Botanique de Buitenzorg*, vol. xi., pp. 98–182, on leaf-forms in relation to rainfall, chiefly based on observations of tropical plants at Buitenzorg. On arriving in Java (November, 1889), he was impressed with the great humidity of the atmosphere as well as with the extraordinary rainfall, and he was led to ask how the plants met such conditions. In the tropical forests they are not only subjected to daily thunder showers during the rainy season, but the sun's rays scarcely penetrate through the thick foliage, and the air is saturated with moisture which causes a perpetual drip from the leaves. An examination of desert forms, with their arrangements for economising their scanty water-supply, has thrown so much light on the peculiarities of certain plants of our temperate climates, that plant adaptations to opposite conditions on an equally large scale promised a rich and suggestive field of study.

Leaves, as we know, differ greatly in their behaviour towards atmospheric water; from waxy leaves the drops roll off, leaving no perceptible moisture behind, while other leaves, without this provision, tend to retain the moisture on the leaf-surface, and are weighed down and often broken off, in the tropics, by the added pressure on the brittle leaf stalk. The retention of moisture on the leaf after the rain is over interferes too with the transpiration, with which the rise of water in the stem with the supply of mineral salts to the growing and assimilating organs is connected. This function is provided for, and the difficulty so far met, by the great leaf-surface, but there are also various methods by which plants with easily moistened leaves dispose as speedily as may be of their superabundant water-supply. One of these methods is the adoption of the sleeping position by leaves, such as those of the sensitive plant, that possess this power of movement. Jungner,[1] a previous observer, says: "As soon as rain falls the horizontal leaves bend upwards as in the sleeping condition, and the rain-drops easily run off by the base of the leaf."

The most frequent way of disposing of the excessive moisture, however, is by draining it off by means of long points to the leaves.

[1] "Anpassungen der Pflanzen an das Klima in den Gegenden der regnerischen Kamerungebirge." *Botanisches Centralblatt*, 1891, p. 358.

These Stahl calls "*Träufelspitzen*," or dropping-points, and the rapidity with which tropical plants shed the rain-drops by this means is very astonishing. In a comparatively moist atmosphere, after a heavy rain, such plants as *Ficus acuminata*, *Angiopteris evecta*, and many others possessing *Träufelspitzen* had been long dry, while European or Australian forms with leaves ending in blunt tips, as the Oak, Apple, *Eucalyptus*, etc., yielded on shaking them a heavy shower of drops.

It is instructive to glance over any monograph of tropical genera and note the frequent use of the terms, "*folia longe acuminata*," "*folia acuminatissima*," "*folia caudata acuminata*." These *Träufelspitzen* occur also on the lobes of divided leaves, and on the pinnæ of compound leaves, but they are, perhaps, most remarkable on entire ovate leaves, such as those of *Ficus religiosa*, where the midrib grows out into a linear point sometimes 75 mm. long, the leaf itself measuring about 100 mm. from the stalk to the base of the point. In some plants the prolonged midrib of the leaf is a fairly wide channel, but the most ordinary form is that of a tapering narrow point slightly bent back and curved at the end, a peculiarity which is found in many European species. As the water trickles down the inclined narrow points, it passes from the upper to the under surface before dropping from the leaf, and the bent tip accelerates this action very distinctly.

Stahl tried several experiments to test this theory as to the advantage of *Träufelspitzen* to the plant. He took six leaves of *Justicia picta*, one of the Acanthaceæ, with leaf points a centimetre long, and slightly bent at the tip. From three of these the points were cut, and the ends carefully rounded; they were then fixed on a board, which was placed at an angle of 30 degrees, and sprinkled with water. The rounded leaves he found retained some of the moisture for an hour, the others were quite dry in 20 minutes or less. The same results were obtained with other similar leaves.

The veins of the leaf play a very important part in the drainage of water from the surface, especially when they are deep-seated, or, as in the Melastomacean type, where the parallel veins bend in towards the apex. In plants the leaves of which slant towards the stem, the water flows in a centripetal direction, and there we find such contrivances as the rows of hairs in the Bird's Eye (*Veronica chamædrys*), which act by capillarity like blotting paper, and draw away the water from the leaf-surface. Many plants with similar rows of hairs have also deep nerves and channelled stems, as the Blind Nettle (*Lamium album*), Hop, etc. Kerner, in his *Pflanzenleben*, vol. i., p. 85, discusses the importance to the plant of the direction in which the water is conveyed down by the leaves. He considers that it is directly related to the position of the roots, and that the object is to supply moisture to the young growing rootlets. But this seems hardly necessary in the case of tropical plants. It seems more probable that the advantage gained

by the leaf is, as already noted, the lightening of the weight, the aid to transpiration, and, lastly, the continual and thorough cleansing of the surface from insects, fungi, etc. We can verify this on our home plants. After a shower, the pointed leaves of ash, willow, etc., have had the dust quite washed off, while rounded leaves like those of the oak are still dirty. This cleansing of the leaves is of immense importance to tropical plants, as all will allow who have seen how they may become overgrown with hepatics, algæ, mosses, and lichens. These epiphytes, which have many contrivances to enable them to germinate and grow on the leaf, seriously interfere with its function, and we are justified in looking for arrangements by which the leaf will make it difficult for the epiphytes to get a footing. In the end they are generally successful, however, and the leaves with longest lives have the heaviest load to bear.

Plants with shining satin-like leaves, often tinged on the underside with red, form a great group much cultivated for the sake of the beautiful foliage. *Anthurium crystallinum*, *Philodendron Lindeni*, with begonias and many others, belong to this group. The peculiar brilliancy of the leaves is due to papillose outgrowths of the epidermal cells. These exert a capillary attraction on water, and the drops left after a rainfall are thus dispersed by the papillæ into a thin sheath which evaporates quickly and secures the rapid drying up of the leaf. This capillarity was tested by dipping the tip of a leaf of *Anthurium* in water, when the moisture was drawn up on the leaf 140 millimetres.

The long points to the leaves are certainly, in countries like West Java with a heavy rainfall, the chief means by which the plant disperses the excessive water-supply. In China and Japan, where the same conditions of humidity and great heat occur, they are a striking feature of the vegetation. An interesting exception is found in plants native to the drier regions along the shore or up on the mountains which have rounded or crenate leaves. In our own woods, while we occasionally find *Träufelspitzen* on the smaller herbs, as *Melampyrum sylvaticum* which has long tapering pointed leaves, they are very frequent on plants of a larger growth, especially on trees and shrubs that grow by preference in shady woods or along the banks of rivers, such as the ash and the willow; those that seek a drier soil, different kinds of *Prunus*, roses, etc., have no such provision. The same difference in structure appears in a more pronounced form when we compare the vegetation of different climates. In the Eastern United States, where great heat alternates with heavy thunder showers, there is a great abundance of tropical forms, much more so than in Central Europe where the summer is drier and colder; but even within the limits of the same genus, the American species have distinctly longer pointed leaves than their European relatives. In Europe, Stahl says, he noticed that longer points were developed on leaves in moist regions than where the ground and atmosphere were drier.

In further support of these observations, Stahl made a careful

examination of the leaf-forms of one or two very widely distributed genera. Quoting from Pax's monograph on the genus *Acer*, he presents to us first the species belonging to Europe: *Acer platanoides*, which in a wild state inhabits damp localities, and the Sycamore (*A. pseudo-platanus*) have pointed leaves; the Maple (*A. campestre*) grows on the plains and uplands of Central and Southern Europe, and the lobes of its leaves are blunt, as are those of *A. monspessulanum*, also from Southern Europe. In the United States and in Japan, the different species have leaves with long pointed lobes, and this feature is still more marked in the Himalayan varieties. *Acer integrifolia*, from Southern China and Japan, has a simple, entire leaf which ends in a long point. Though he does not (without other evidence) refer plants without *Träufelspitzen* to a dry climate, yet he concludes that when they possess this feature so strongly marked as in *Ficus religiosa* and others, they must grow in a country with a heavy rainfall. Stahl thinks also that this peculiarity, which is so entirely one of adaptation to climate, might throw some light on the conditions of growth of fossil plants.

In plants wholly confined to the tropics, many young leaves assume a hanging position on unfolding, and remain vertical for some days after attaining their full size. This is undoubtedly a protection from the heavy rain, and saves them from the devastation that overtakes so much of the vegetation in these destructive showers. When the leaves are stronger they become horizontal, though some very large leaves, as those of *Anthurium Veitchii*, remain vertical throughout their life. It would be interesting and useful to be able to calculate the exact force of the rain-drop as it strikes the leaf. This has not been worked out, but that the blow is heavy anyone knows who has been caught in one of these showers, or who has heard the noise made by the rain falling on palm leaves. Kny, in the *Berichte der deutschen botanischen Gesellschaft*, 1885, has directed attention to the danger incurred by leaves from rain and hail, and to the leaf-tissues and leaf-forms specially adapted to meet this emergency. In simple undivided leaves we find either great elasticity and bending power, or a strong leathery structure. In other leaves we have division into laminæ; either, as in many palm leaves, this takes place regularly by the splitting of a layer of cells as the leaf unfolds, or it is effected later by a tearing apart of the tissue. The division reaches its highest development when the laminæ are quite separate leaflets, as in most ferns and in many dicotyledonous plants.

The adaptation of leaf-form to function and environment is nowhere more strikingly seen than in those plants that at different times produce different kinds of leaves. The first leaf of the stately fern *Platycerium alcicorne* is unstalked and kidney-shaped, the mantle-leaf of Goebel,[2] and lies flat on the ground, thus protecting the young

[2] "Morphologische Studien.—I. Ueber epiphytische Farne u. Muscineen." *Annales du Jardin Botanique de Buitenzorg*, 1887.

roots from being dried up. The later leaves are erect and much divided. We find the same contrivance in one of the aroids, *Pothos aurea*, the stem of which is at first creeping, and bears flat sessile undivided leaves to protect the roots; when the stem ascends the leaves become stalked and pinnate. There are numerous examples of heterophylly among our European herbs; in many of the Compositæ, Cruciferæ, etc., the upright stalk bears linear, pinnate, or lobed leaves, while those forming the rosette of radical leaves are larger, and lie flat on the ground. Still another interesting point in leaf-adaptation is found in the nervature; either the nerves are parallel as in most monocotyledons, or they anastomose to form a network as in most dicotyledons. In the former case the leaf is simple, and of much stronger tissue; the complicated net-veining is found in wide-spreading leaves, and serves as a protection against splitting. In the Fern group the simpler type of parallel nervature prevails rather than the other, but during the Mesozoic age net-veined fern leaves were very abundant up to the close of the Chalk period. Then dicotyledons with leaves of a similar structure began to appear, and seem gradually to have taken their place.

Stahl closes the paper by a short examination of the leaf-tissues as they are affected by environment. In the long, slanting or bending leaves of monocotyledons the upright palisade cells of the assimilating tissue are replaced by cells the longest axis of which is parallel to the leaf-surface. By the massing of the strengthening elements towards the centre of the tissue, between the upper and under surface, the elasticity of the leaf is secured, and there is further provision against bending strains in those leaves with parallel veins, each of them being surrounded by strands of strong fibres.

ANNIE LORRAIN SMITH.

II.

On the Zoo-Geographical Areas of the World, illustrating the Distribution of Birds.

I HAVE been asked to tabulate the different regions, sub-regions, etc., of the world, of which I spoke, and which I illustrated in my recent course of lectures on the "Geographical Distribution of Birds" at the Royal Institution. I do this with some hesitation, because, as I explained in these lectures, our want of zoological statistics for vast tracts of the Old World makes it almost impossible to give exact definitions of the natural areas of any of the Palæogœan divisions of the globe.

With America it is different, as has been shown during the last few years by the admirable work of Dr. C. Hart Merriam and Mr. J. A. Allen on the distribution of North American Mammals, by means of which these authors have presented us with excellent maps defining the natural regions of the North American fauna. Mr. Allen has also recently published ("Auk," 1893, pp. 97–150 pls. iii., iv.) a separate paper on the zoo-geographical divisions of North America based upon the class Aves, and in my lectures I have followed the main conclusions of this paper, as I think it is better for ornithologists to adopt faunal regions based upon the distribution of *birds alone*, when we have some data to go upon. Until Mr. Allen published his avi-geographical maps, I was only too content to work by means of the maps furnished by Dr. Hart Merriam and himself from a study of the mammalia of North America, but I believe that we shall ultimately arrive at sounder conclusions with regard to the zoo-geographical divisions of the earth's surface, if each section of zoologists works out the statistics of his own branch, independently of those of other zoologists, so that at some future date the results can be correlated. This I can hardly expect to see accomplished in my day, but I hope that the present paper may be found to be a small contribution to this desirable result.

The American naturalists may well be envied for, and congratulated by their European colleagues on, the success which has resulted from their patient collection of materials. This has extended over many years, and now leaves them in the proud position of having better statistics to work upon than are possessed by the ornithologists

of any other portion of the globe, with the exception, perhaps, of our own islands.

While, therefore, many of my conclusions with regard to the extent of the provinces of the Old World must be purely conjectural from want of definite knowledge, there are certain sub-regions which may be suggested, and their boundaries defined, with some confidence.

Some of Mr. Allen's conclusions ("Auk," 1893, pp. 97–150) with regard to the main divisions of the Old World are the same as those of Dr. Reichenow, and I think that they are, in both instances, too sweeping; but the recognition and definition of an Arctic Zone or "Realm," as Mr. Allen calls it, is a fact which must henceforward be admitted by all ornithologists. Its southern boundary is probably along the isothermal line of 50° (mean summer temperature), and may be coincident with the northern limit of conifers.

Mr. Allen's "North Temperate Realm" includes both hemispheres to the northern border of the palm belt. Its southern line corresponds with the isotherm of 77° mean annual temperature, but this constitutes a very roughly defined southern border for the zoological areas of the Old World.

The southern border of palm-growth follows the southern isothermal line of 77° (mean annual temperature) but by no means defines natural borders for Mr. Allen's Indo-African Realm or Dr. Reichenow's Æthiopian and Malayan Regions, and it is better, as it seems to me, to stick to our old terms at present, instead of defining the limits of the Old World regions and sub-regions, with their ill-tabulated data, by analogy with the New World sub-regions, the boundaries of which are so much better known. It is for this reason also that I have not adopted Mr. Allen's nomenclature in its entirety. I may in due time be brought to speak of "Realms," but the same conservatism which prevents my adopting the trinomial nomenclature of the American zoologists of the present day, will prevent my discarding some of the old-fashioned, and, to me, expressive zoo-geographical terms. I cannot understand why the word "Nearctic" should be discarded. It was given by Dr. Sclater not in the sense of "arctic" but "northern" region of the New World, and is, in my opinion, apart from the priority which commands respect for its retention, a most simple and expressive term. My American colleagues will understand that if I have not carried their system of nomenclature into the zoo-geographical regions of the Old World, it is not from any want of respect to their work, for I heartily agree with their conclusions as regards North America; but a little time must elapse before we can confidently apply the same reasoning to the avifauna of the Old World. When Dr. Pleske and Professor Menzbier have finished their works on the birds of the Russian Empire, so that the confusion caused by the "lumping" tendencies of the Middendorffian school, so justly deplored by Dr. Stejneger, shall have been dispelled; when France, and Spain, and

Turkey, and Greece shall have been ornithologically explored and the results described in the complete American fashion; when the first collection of birds arrives from Timbuktu; when Arabia gives up its ornithological secrets; and when the parts of Siberia and Asia, of which nothing is yet known, have been explored; to say nothing of the high mountain interiors of many of the Malayan islands; then—and scarcely till then—shall we be able to generalise with some chance of success.

This deficiency in our information, however, need not prevent us from hazarding an idea—as has, indeed, often been done before now—of some of the avi-geographical regions of the Old World, and I place before my readers the divisions which I introduced into my Royal Institution lectures, leaving it for future statistics to confirm or destroy my conclusions. I begin, therefore, with the New World, adopting, for the purposes of my lectures, the old divisions of the Nearctic and Neotropical Regions:—

A.—Nearctic Region (Northern Region of the New World).

I. Arctic Sub-Region.
II. Alaskan Arctic Sub-Region.
III. Aleutian Sub-Region.
IV. Cold Temperate Sub-Region.
V. Warm Temperate Sub-Region.
1. Humid Province.
α. Appallachian Sub-Province.
β. Austroriparian Sub-Province.
2. Arid Province.
γ. Campestrian Sub-Province.
δ. Sonoran Sub-Province.

B.—Neotropical Region (Southern Region of the New World).

I. Antillean Sub-Region, including the Greater Antilles and Southern Florida, as also the Lesser Antilles, with the exception of Trinidad, Tobago, and the islands off the coast of Venezuela.

II. Central American Sub-Region.
1. Mexican Province.
2. Isthmian Province.

Until my friends, Messrs. Salvin and Godman, have finished their monumental "Biologia," it will be impossible to define accurately the boundaries of the several provinces, especially as fresh facts are being brought to light every day. A division may be hazarded on the lines of Mr. Salvin's conclusions published in the *Proceedings* of the Zoological Society for 1867 (pp. 131, 2) and 1870 (pp. 177, 8). Thus we shall probably get two provinces, which may be called:—

1. The *Mexican* Province, including Central America from Costa Rica northwards to Mexico, leaving out the Central Plateau,

which forms part of Mr. Allen's Sonoran Sub-Province. On the northern boundaries of the Mexican Province, Mr. Salvin's remarks ("Ibis," 1889, p. 242) must be consulted. On the eastern side it seems to follow the forests as they cling to the mountains almost to the Rio Grande, and the presence of Tinamous, Motmots, and Toucans mark the limit of the Neotropical Region. On the northern side the limits of the Mexican fauna are somewhat more extended and reach to about Alamos in Sonora.

2. The *Isthmian* Province, extending from Costa Rica to Panama.

For the rest of the Neotropical Region, I find that nothing can be proposed in modification of the four sub-regions proposed by Mr. Salvin, and published by Professor Newton in his article on Birds, in the "Encyclopædia Britannica." They are delineated in the accompanying map.

III. The Patagonian Sub-Region. This includes the southern portion of South America up to Bahia Blanca on the east coast, "thence in a north-easterly direction, passing to the eastward of Mendoza, and then northward along the eastern and higher slopes of the Andes, until it crosses the Equator, and after trifurcating on either side of the valleys of the Magdalena and its confluent the Cauca, returns along the western slopes of the lofty Cordillera, until it trends seaward and reaches the Pacific Coast of South America somewhere about Truxillo, in lat. 7° S." (*Cf.* Newton, *t.c.*, p. 744.) Mr. Salvin tells me that he considers that, besides the trifurcation of this region in Colombia above referred to, there are outlying portions of the same sub-region in the mountains of Santa Marta and Merida.

IV. The Brazilian Sub-Region. This "marches with the foregoing until somewhere near Potosi in Bolivia, whence it turns to the north-east, and, avoiding the water-shed of the Amazons, strikes, perhaps, the Paranahyba, through or along which it makes its way to the Atlantic." (*Cf.* Newton, *t.c.*, p. 744.)

V. The Amazonian Sub-Region limited to the southward by the Brazilian boundary, the western frontier of the Amazonian sub-region seems to turn off before the eastern confines of the Patagonian sub-region are reached, and, leaving a space intervening, it pursues a generally northward course, at a lower level, on the western bank of the Huallaga, and crossing the great stream, whence it derives its name, in somewhere about long. 77° W., lat. 5° S., it pursues its way towards the mouth of the Orinoco." (*Cf.* Newton, *t.c.*, p. 744.)

VI. The Sub-Andean Sub-Region. "This begins in the south with a narrow strip of land, before mentioned as intervening between the comparatively low-lying Amazonian sub-region and that portion of the Patagonian which runs along the lofty Peruvian Andes, and is believed to extend from the frontiers of Bolivia to the table-land of Ecuador, rounding, on the one hand, the forked extremity of the Patagonian sub-region to the westwards until it meets the Pacific at Truxillo, stretching over five hundred miles of sea to the Gala-

pagos Islands, following the Amazonian boundary to the Atlantic, while it comprehends the islands of Trinidad and Tobago, as well as those which lie on the northern coast of South America." (*Cf.* Newton, *l.c.*, p. 744.)

C.—THE PALÆARCTIC REGION.

This may be divided roughly into three sub-regions, besides the

ARCTIC ZONE,

which corresponds with the same zone in the New World, and becomes a circumpolar province. The greater part of the northern Palæarctic Region may be denominated the

I. EURASIAN SUB-REGION,

while the southern portion forms another sub-region, which may be called the

II. MEDITERRANEO-ASIATIC SUB-REGION;

the one answering to Mr. Allen's "Cold Temperate Sub-Region," and the other to his "Warm Temperate Sub-Region."

A very good idea of the limits of the breeding areas of the Western European birds has lately been given by Mr. Henry Seebohm in his most useful introduction to his "Geographical Distribution of British Birds," and on discussing with him the natural geographical divisions of the European Avifauna, he suggests that the Scandinavian mountains must form part of the Arctic Zone which should extend as far south as the Dooresjeld, and this would correspond with the breeding range of several Arctic species, such as the Snowy Owl, &c. The Yenesei Valley, as Mr. Seebohm has before pointed out, is the natural boundary between the Eastern and Western Palæarctic birds, the Urals being by no means a strong natural boundary. Mr. Seebohm deprecates the division of the Eurasian Sub-Region into provinces, and I admit that, with the data at present in our hands, it is difficult to define the limits of natural provinces, but I expect that we shall ultimately have to recognise a Central Siberian, as well as an Eastern Siberian province. The former will be the breeding home of such representative forms as *Motacilla beema* instead of *M. flava*, *Corone sharpii* instead of *C. cornix*, *Sturnus menzbieri* instead of *S. vulgaris*, etc., while the Eastern Siberian Province will probably extend from the water-shed of the Lena to Kamtschatka. It will contain the breeding home of *Eurinorhynchus pygmæus*. Dr. Stejneger advocates the recognition of a distinct Kamtschatkan province. On this and other points connected with the distribution of Western Palæarctic birds, his admirable paper must be consulted (*Bull. U.S. Nat. Mus.*, no. 28). As in the case of the Palæarctic Region itself, the provinces will not be found to contain different families or genera, but will be characterised by representative species alone.

The following will, therefore, probably be found to be the natural areas of the Palæarctic Region:—

I. The Eurasian Sub-Region—
   1. European Province—east to the river Ob.
   2. Central Siberian Province. Between the Ob and the Lena.
   3. West Siberian Province. From the Lena to the Pacific.

II. The Mediterraneo-Asiatic Sub-Region—
   1. Mediterranean Province.
   2. Mediterraneo-Persic Province.
   3. Mongolian Province.

III. Mantchurian Sub-Region.

There is also another natural sub-region, which I propose to call:—

IV. The Himalo-Caucasian Sub-Region.

It consists of the highest Himalayas from 8,000–10,000 feet (ex. *Tetraogallus*), the mountains of Tibet and the Altai to the north, and westwards through the mountains of Persia to the Caucasus and the hill-country of Asia Minor.

The Mediterraneo-Asiatic Sub-Region has at least three distinct provinces with characteristic forms, as was shown by Mr. H. J. Elwes in his celebrated essay (*Q.Z.S.*, 1867, pp. 645–682):—

1. The Mediterranean Province.
2. The Mediterraneo-Persic Province.
3. The Mongolian Province.

The Mantchurian Sub-Region reaching from Eastern Siberia, south of the Stanovoi Mountains, west to Lake Baikal and south to the Yangtze basin, appears to me to be established by statistics. It contains a strong Indian element, with such species as *Eurystomus calonyx*, *Halcyon pileata*, *Hirundo tytleri*, *Pericrocotus*, *Suthora*, etc., and is further characterised by several eastern representative species of well-known western forms, such as *Trypanocorax pastinator* for *T. frugilegus*, *Colæus dauricus* for *C. monedula*, etc.

D.—The Ethiopian Region.

Of the Ethiopian Region it is only necessary to remark that I have greatly modified my views since I proposed zoo-geographical sub-regions of Africa in 1870. The great desert of the Sahara still remains a blank to us and may be called

I.—The Saharan Sub-Region,

but I have come to the conclusion that a second large natural sub-region can be admitted which I call

II.—The Sudanese Sub-Region.

The number of species which are common to Senegambia and to Abyssinia and Kordofan, proclaims that there must be a natural zoo-geographical sub-region stretching across between these two distant areas. Its northern and southern boundaries cannot yet be defined, but it is a somewhat important fact that a truly Sudanese form like *Melierax polyzonus* has been found in Mogador

and Southern Morocco. The Delta of Egypt must be included in the Mediterranean Region, and I am not able to define the exact boundary of my Sudanese Sub-Region in the north-east. The occurrence of a *Centropus* in the Delta shows the presence of an African element even in Lower Egypt, but the regional boundary may be about the second Cataract, above which no Sun-birds (Nectariniidæ) are found.

III.—The West African Sub-Region.

This might well be called the "Afro-Malayan" Sub-Region, as it possesses species of *Turdinus*, a *Pitta*, some Diceidæ, and other curious forms which are Malayan in their affinities. It consists of the forest districts of Southern Senegambia, and embraces the whole of the West African forest region from the above-mentioned point to the Kwanza River, its continuity being broken at Accra, where the Sudanese Sub-Region breaks through to the coast with its attendant Bustards (*Otides*) and Hemipodes, and this will probably be found to be the case at other intervals of the coast-line. The forest region may also extend a little way across the Kwanza into the interior of Benguela. Certainly it embraces the whole of the Congo basin as far as the western water-shed of the Nile, as was shown by Bohndorff's collection from the Niam Niam country and by many species obtained by Heuglin and Emin Pasha in the Equatorial Province.

IV.—The Abyssinian Sub-Region, consisting of Abyssinia and Southern Arabia.

This not only embraces all the area which I assigned to it in 1870, but must be carried north of the Zambesi, recent explorations having shown that I was mistaken in believing the latter river to be a natural boundary. It may even be found to stretch, in a broken and disjointed manner, far into Equatorial Africa, as proved by the discovery of *Chera progne* and *Pyromelana taha* by Mr. Jackson in Equatorial Africa.

V.—The East African Region.

This is not a very natural sub-region, and may have to be sunk in one of the others; but at present such a number of forms seem to be peculiar to it that it is best to recognise it till our present knowledge of the East African avifauna has been tabulated and brought up to date.

The South African Sub-Region.

It is to be noticed that in the map I have indicated the existence of a

1.—Cape Province.

By this I intend the Cape Colony south of the Karroo and reaching eastwards to the neighbourhood of Port Elizabeth and East London. Quite a number of distinct forms inhabit this province.

2.—The Natalese Province.

There is another province, which commences, probably, in British Caffraria, certainly about the Berea, near Durban, and extends along the Drakensburg mountains, including the forest regions of the Transvaal, such as the Lydenburg district, and reaches the Zambesi, whence I believe it will be found to stretch across to Benguela and Angola.

Lastly, I propose to recognise a

V.—Victorian[1] or Camaroonian Sub-Region,

consisting of the elevated mountains of Central East Africa, and extending from the highlands of the Lake Regions across to the peaks of the Camaroons. This is a natural sub-region and is characterised by a number of forms. The recent collections sent home by Mr. H. H. Johnston, C.B., show that this mountain fauna extends to the Shirè Highlands, as is evidenced by the presence of *Cryptospiza* and the inevitable green *Xenocichlæ*, which we now look for at a certain elevation, and which are known from the Shirè Mountains, Kilimanjaro, Elgon, and the Camaroons. The occurrence of many peculiar species and genera such as *Pædilorhynchus* in the Uganda Highlands, and again in the Camaroon Peaks, shows that there is a close connection between the Avifaunæ of these elevated mountains, and, doubtless, Kenia and Ruwenzori will be found to belong to the same system. The highlands of Shoa certainly belong to it, and some of the Abyssinian mountains will also, in all probability, have to be included.

VI.—Lemurian or Mascarene Sub-Region, consisting of Madagascar and Adjacent Islands.

E.—Indian Region.

This follows the usual boundaries laid down by Dr. Sclater and Dr. Wallace. The division of the Indian Peninsula into provinces will probably be determined by the mean annual distribution of rainfall, as shown by Mr. Hume in his interesting paper ("Stray Feathers," vol. vii., pp. 501, 502). This paper has not received half the attention which it deserves, and many of the problems connected with the presence of a Malayan element in the western portion of the peninsula and the mountains of Southern Ceylon become less difficult of comprehension when we find that they belong to countries where the rainfall is 70 inches and upwards. In the Himalayas we have apparently the foci of three separate sub-regions; there is a strong Palæarctic element illustrated by the number of species of *Carpodacus*, *Pyrrhula*, etc. There are also recognisable two more sub-regions.

Another Sub-Region would be

The Himalo-Chinese Sub-Region, somewhat as defined by Mr. H. J. Elwes, and illustrated by such forms as *Polyplectron*; while a third

[1] Not named directly in honour of Her Most Gracious Majesty, but intended to show that Mount Victoria in the Camaroons, and the Victoria Lake country in Equatorial Africa, belong to the same natural sub-region.

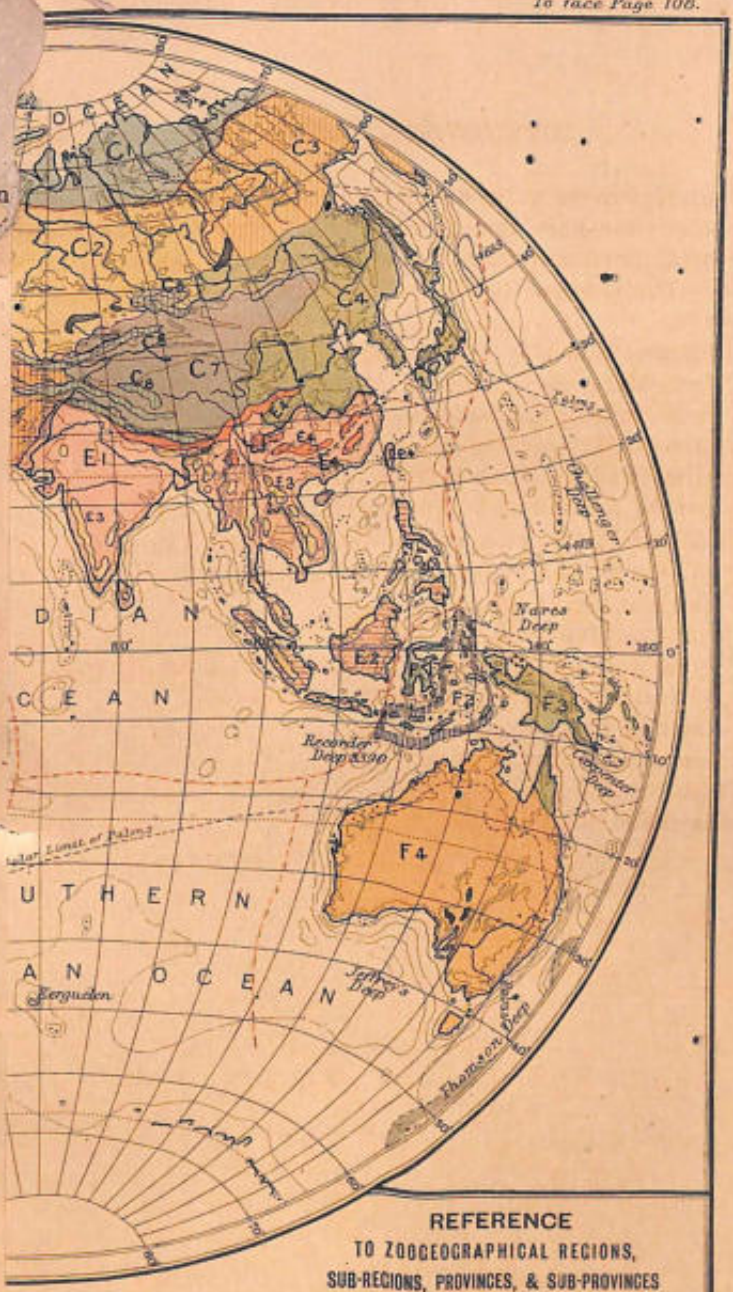
natural sub-region seems to be the Himalo-Malayan Sub-Region, which includes the high mountains of Burma, the Malayan Peninsula, Sumatra, Java, and Borneo. A representative form would be *Psarisomus*. The Indian Region would, therefore, be divided as follows :—

1. Indian Peninsular Sub-Region.
2. Indo-Malayan Sub-Region.
3. Indo-Chinese Sub-Region.
4. Himalo-Malayan Sub-Region.
5. Himalo-Chinese Sub-Region. The high mountain portion of the Indo-Chinese Sub-Region.

In the Australian Region, east of Wallace's line, I think that the following sub-regions can be fairly well recognised :—

I. Celebean Sub-Region.
II. Moluccan Sub-Region.
III. Papuan Sub-Region, which should include North Eastern Australia with its *Casuarius* and *Tanysiptera*.
IV. Australian Sub-Region.
V. New Zealand Sub-Region.
VI. Fijian Sub-Region.
VII. Hawaiian Sub-Region.

R. Bowdler Sharpe.



REFERENCE

TO ZOOGEOGRAPHICAL REGIONS, SUB-REGIONS, PROVINCES, & SUB-PROVINCES

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C. PALÆARCTIC REGION.

1. *Arctic Sub-Region*
Eurasian Sub-Region.
2. *European Province.*
3. *East Siberian Province.*
4. *Manchurian Sub-Region.*
Mediterraneo-Asiatic Sub-R.
5. *Mediterranean Province.*
6. *Mediterraneo-Persic Prov.*
7. *Mongolian Province.*
8. *Himalo-Caucasian Sub-Reg.*

D. ETHIOPIAN REGION.

1. *Saharan Sub-Region.*
2. *Sudanese Sub-Region.*
3. *West African Sub-Region.*
4. *Abyssinian Sub-Region.*
5. *East African Sub-Region.*
6. *South African Sub-Region.*
7. *Cape Province.*
8. *Natalese Province.*
9. *Camaronian Sub-Region.*
10. *Lemurian Sub-Region.*

E. INDIAN REGION.

1. *Indian Peninsular Sub-Reg.*
2. *Indo-Malayan Sub-Region.*
3. *Himalo-Malayan Sub-Reg.*
4. *Himalo-Chinese Sub-Region.*

F. AUSTRALIAN REGION.

1. *Celebean Sub-Region.*
2. *Moluccan Sub-Region.*
3. *Papuan Sub-Region.*
4. *Australian Sub-Region.*
5. *New Zealand Sub-Region.*
6. *Fijian Sub-Region.*
7. *Hawaiian Sub-Region.*

The Contour-Lines for 600 and 6000 Feet, and the 200, 1000, 2000, 3000, and 4000 Fathom-Lines, are indicated by Faint Lines.

III.

Earthworms and the Earth's History.

THERE has been recently a considerable discussion in the pages of *Nature* as to the probability of a former extension northwards of the antarctic continent. The discussion has also affected the columns of this Journal, which published last month an abstract of the recent views of Mr. H. O. Forbes on the subject. Mr. Forbes believes in this continent. We quite agree with him, though for reasons different to those which he urges.

It seems to be an obvious platitude that those animals are most important for use in arguments relating to the past connection of land masses which do not migrate of their own free will across the sea, and are incapable of being carried over by accidental modes of transit, and yet, with one exception to be mentioned presently, the group of all others which best fulfils these conditions has been completely ignored by writers on the subject of geographical distribution. The group to which we refer is that of the earthworms.

The earthworm, like the agricultural labourer, is wedded to the soil; more so, indeed, for the labourer does occasionally emigrate, but the earthworm never, and for the very good reason that sea-water is fatal to his constitution. Although earthworms do not apparently ever cross the sea by any of those modes of transit which are held to account for the range of other animals, there is sometimes (again like the agricultural labourer) "assisted emigration." Commerce between different countries is apparently responsible for the transference of earthworms from one country to another—particularly, of course, the exportation and importation of plants. That this is really the case is capable of the easiest proof; any collection of living plants from a foreign country is nearly sure to contain a few worms, besides, of course, insects and other small creatures. Indirect but still convincing proof of man's agency in affecting the distribution of earthworms is shown by the wide range of the common earthworms of Europe which belong to the genera *Lumbricus* and *Allolobophora*. In Europe and North America these two genera are the prevailing types, to the nearly complete exclusion of any others. In such distant and widely-separated regions as New Zealand and South America, the same genera are met with, but they are not the prevailing types.

Now, there is not, so far as I am aware, a single exception to

the truth of the statement that in all these cases the foreign species are identical with those found in Europe and North America. Were the forms in question really indigenous, different species, or at the very least, varieties might be fairly expected to occur; but they do not occur. Any difficulties in the existing distribution of animals are apt to be explained in one or two ways; either a continent is evoked from the vasty deep for this occasion only, or tree trunks and floating *débris* are pressed into the service. As has been already mentioned, earthworms are killed by salt water; so that we must assume a former land connection to explain their presence on both sides of a strait of whatever width. It is a most remarkable fact that works dealing with geographical distribution and the geological arguments to be derived from a study of animal distribution have not made use of this important material; the only paper known to us which does make use of this group is by Professor Spencer on the fauna of Tasmania (*Australasian Assoc. Adv. Science*, Presidential Address, Section D, 1892). Other terrestrial invertebrates are doubtless of use; but up to the present, hardly anything except insects and land shells have been studied from this point of view. But in neither of these groups are the anatomical relations between the genera and species so clear as might be wished. This partly destroys their value, as does also the fact that they possess facilities for migration, passive or active, not possessed by the earthworm. Sealed up in its shell by the temporary operculum, a snail is capable of extended travel; butterflies can, as we know, fly for long distances; the earthworm has none of these advantages; the cocoons might, it is true, be conveyed on the feet of birds, but they are often below the soil, sometimes very far down; this mode of transit has very possibly occurred in the case of the aquatic worms allied to the earthworm, which deposit their cocoons at the margins of streams and rivers frequented by birds; in this way we may possibly account for the wide prevalence of the common "red-worm" of our rivers (*Tubifex rivulorum*).

Applying these facts to the particular case in question, it seems to us that the former existence of a southern continent is rendered at least very probable. The earthworm fauna of New Zealand, which is fairly well known at present, is characterised by the presence of the genus *Acanthodrilus*, this genus and its immediate allies forming nearly the whole of the indigenous earthworm fauna of the country. Australia, on the other hand, is characterised by quite a different family, the Cryptodrilidæ, comprising three or four genera, perhaps more; *Acanthodrilus* is represented by only two species, which are found upon that side which is believed to have had, at a remote period, a connection with New Zealand. The Australian family is, on the other hand, only represented by two or three species in New Zealand. The genus *Acanthodrilus* (using the genus in its widest sense) is also characteristic of Patagonia and South Georgia (so far as is known), and the more southern parts of South America. The

only worms known from Kerguelen and from Marion Island are members of the same genus. "Ethiopian" Africa has many species of the family which differ more from those of New Zealand than the latter do from those of Patagonia—in fact, they have been placed in a separate genus, *Benhamia*; a few members of the same genus or sub-genus occur in the West Indies and one or two in India and Malaya. It is only, however, in Patagonia and New Zealand that this genus is the prevailing one; in the other countries mentioned it occurs, but there are numerous other and more abundant forms. The important point to be noticed is that, as we pass northward from the antarctic region, this particular genus thins out, until in the continents of Europe and Asia it is hardly to be found.

The former existence of a habitable antarctic continent with arms stretching to New Zealand, Africa, and Patagonia seems to be the clearest way of explaining these facts.

F. E. BEDDARD.

IV.

Some Useful Methods in Microscopy.

HAVING been occupied during the spring of 1892 in studying the early stages in the development of *Loligo* and *Sepia* at Naples, I found myself unable to make any progress in solving the problems presented by these difficult objects until I had obtained a satisfactory method of preparing and mounting blastoderms, quite freed from yolk, for study of surface-views under the microscope. After trying many methods, I at last hit upon one which, while exceedingly simple in application, not only gave the most beautiful results for these blastoderms, but has also proved applicable for other objects which are not more than a few cell-layers in thickness, such as early chick embryos, and still more so, of course, for unicellular objects, such as Protozoa. It seemed to me, therefore, desirable to describe briefly the methods employed by me, since they may prove useful to other zoologists. In so doing, however, I do not in any way wish to lay claim to any originality or precedence in inventing them. For all I know they may have been already described by many other investigators. Wherever I give any method which I have obtained from an author I shall give the reference to him; where I do not do so, my apparent oversight is to be set down to my ignorance of the now vast literature of the subject.

The method employed by me for cephalopod blastoderms is exceedingly simple in principle, though its mode of application varies, of course, with the nature of the object, and may require a high degree of manual skill, to be obtained only by much practice. It is as follows. The fresh objects are first brought into Hermann's fluid for a short time, until all the cells are fixed; this is, as a rule, effected in 2–5 minutes, but the proper length of exposure to the action of the reagent can best be determined by the eye; as soon as the cells become white and opaque they are sufficiently fixed. Too long exposure is injurious to subsequent staining.

After the Hermann's fluid, the objects are carefully washed in two or three changes of water to extract all the reagent. This also is an important point for staining. After washing, the objects come into either alum carmine or Paul Mayer's[1] newly invented stain carm-alum.

[1] "Ueber das Färben mit Carmin, Cochenille, und Hämatein-Thonerde." *Mitth. Zool. Stat. Neapel.* Bd. x., Heft. 3, p. 489.

Here they are left for an hour or two until completely stained. They are then washed with water and brought up through the alcohols—30 %, 50 %, 70 %, 90 %, and absolute—into oil of cloves, and then mounted in Canada balsam.

The above is the complete plan of procedure for this method. Before, however, proceeding to describe its practical application to the objects in question, a few words are necessary concerning the agents mentioned.

Hermann's fluid was, as its name implies, invented by Hermann[2] in his studies on nuclear division, etc., in the testis. It may be described as a paraphrase, so to speak, of the well-known Flemming's fluid (the second or strong solution) in which the 1 per cent. chromic acid is replaced by 1 per cent. platinum chloride, thus:—

Platinum chloride 1 per cent.	15 parts.
Osmic acid 2 per cent.	4 ,,
Glacial acetic	1 part.

I always make it up according to the following recipe:—Take one of the ordinary sealed glass tubes containing a gramme of osmic, as it is commonly sold, and break it in a bottle of a capacity of about 300 cc.; then pour on it 50 cc. of pure distilled water, 12·5 cc. of glacial acetic, and 200 cc. (more accurately 187·5 cc.) of a 1 per cent. solution of platinum chloride. Shake the contents of the bottle up together and put it aside. By the next morning the osmic will have dissolved and the mixture will be ready for use.

Alum carmine is too well-known a staining fluid to every zoologist to require description here; but the very valuable carmine stain recently invented by Dr. Paul Mayer,[3] to which he has given the name of Carmalum ("Carmalaun"), may not be so familiar to the majority of English zoologists. The essence of his discovery consists in using the pure colouring principle, carminic acid, instead of commercial carmine. Carminic acid can be obtained from Dr. Grubler in Leipzig at the rate of about four marks for ten grammes. Having obtained the carminic acid, the method of making up the staining solution is exceedingly simple, and can be carried out as easily in the domestic kitchen as in the well-equipped laboratory, thus: mix together in a suitable vessel one gramme of carminic acid, ten grammes of ammonia alum, and 200 cc. of distilled water. Heat the mixture up to about boiling point, until the ingredients are dissolved, and then, after cooling, carefully pour off or filter the liquid, which is now ready for use. A small crystal of thymol or some other antiseptic should be added, in order to keep it free from organisms. Carmalum is a dark-red liquid with great staining powers, having especially the great advantage that, like picrocarmine, it will give a beautiful stain to objects that have been fixed in pure osmic acid, which alum carmine will not do. It tinges the protoplasm slightly as well as the nucleus,

[2] *Arch. f. Mikroskop. Anat.* Bd. xxxiv. (1889), pp. 58–60.

[3] *Loc. cit.*

hence it is greatly to be recommended, over alum carmine, for objects which it is desired to cut into thin sections, but in the case of objects which it is desired to mount *in toto* it is liable to overstain and render them opaque, and should therefore, in my opinion, be avoided; at any rate, this is so with chick embryos.

I shall now proceed to describe the manner in which this method may be applied to each of the following classes of objects, (1) to cephalopod blastoderms; (2) to chick embryos; (3) to protozoa.

The eggs of Sepia or Loligo are probably well-known objects to the majority of zoologists, and it is not necessary for me to describe them further than by stating a few essential facts about their structure. Although the eggs of the two genera differ widely in appearance they agree in being more or less oval in form and possessing a relatively enormous mass of yolk, at one pole of which is placed the blastoderm, which is comparatively minute at an early stage, though it soon grows over the yolk. Outside the yolk comes a delicate membrane, which, for the sake of description, we may term the chorion, without wishing to prejudge its homologies. In the early stages the chorion is in close contact with the ovum, but separates more and more from it as the embryo becomes formed. Outside the chorion come several gelatinous coats. In Loligo the egg is embedded with a number of others in a common jelly, but in Sepia each egg is separate and has its own concentric coats of jelly.

In order to describe my method I shall take the egg of Loligo, which is rather more difficult to manipulate than Sepia, on account of its smaller size, though for both eggs the method is essentially the same. Moreover, my remarks apply more especially to the earlier stages, from the commencement of segmentation up to the formation of the three layers, during all of which time the blastoderm has the form of a disc or inverted saucer, its curvature being very slight. After it has commenced to grow rapidly round the yolk, it is, of course, impossible to prepare good surface views, for the simple reason that its curved form renders it physically impossible to flatten out the cup-shaped blastoderm without breaking it. Let it be supposed, therefore, that we wish to obtain a series of preparations, either to show the segmentation of the egg and the formation of the germ-layers, or to show nuclear figures.

The first step is to free the egg from its gelatinous envelopes and leave it only surrounded by the chorion. This can be done without much difficulty after a little practice. In Loligo I found it easiest to remove the jelly with a pair of mounted needles in a shallow vessel containing sea-water. The needles employed for this purpose should be as rigid as possible, very springy needles being likely to damage the structures. Now, if one needle be made to pierce the jelly, without of course touching the chorion, and then be held with its point resting on the bottom of the vessel, and slightly slanting, while with the other hand the other needle be made to cross it rapidly in such a way,

that while its point scrapes along the bottom of the vessel its edge scrapes against that of the stationary needle, an exceeding sharp and clean cut can be made through the jelly, which is snipped as cleanly as with a pair of scissors. (See accompanying Fig. 1 and explanation.) With three or four such snips, which, with practice, occupy as many seconds, the chorion is almost completely denuded of its adherent jelly, without the blastoderm suffering the least injury. In Sepia the outer coats of jelly may be removed by simply cutting them with scissors and cautiously squeezing out the egg.

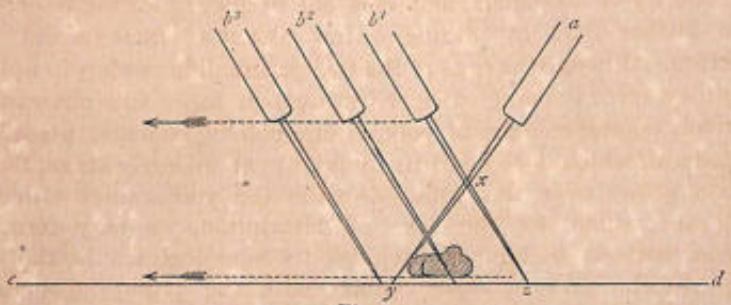

Fig. 1.

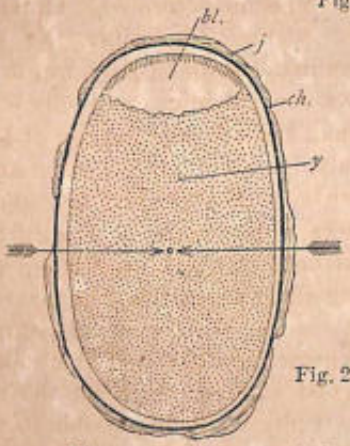

Fig. 2.

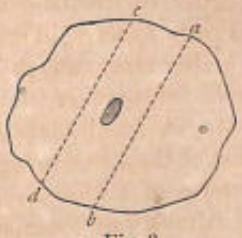

Fig. 3.

FIG. 1.—Diagram to illustrate the method of snipping an object in two wit two needles, *a* the stationary needle, b^1, b^2, b^3, three positions of the moving needle, *o*, object, *cd* bottom of the vessel. The arrows and the dotted lines indicate the direction in which *b* moves. The object is at first enclosed in a triangle, *xyz*, the plane of which should coincide with the plane in which it is to be cut. As *b* moves the triangle *xyz* is reduced to a point, and the object, if soft, is irresistibly cut in two in the plane of *xyz*.

FIG. 2.—Cephalopod Egg.—*bl*. Blastoderm. *ch*. Chorion. *y*. Yolk. *j*. Fragments of Jelly.

FIG. 3.—Diagram to explain section-cutting (p. 122).

We now have the egg enveloped only in the chorion, with the minute disc-shaped blastoderm at one end. The blastoderm is transparent and almost invisible. The egg is now removed from the sea-water and dipped into a capsule containing Hermann's fluid. After a few seconds the blastoderm shows up very plainly as an opaque white patch. It should be left for about a minute

longer and then removed to a shallow vessel containing distilled water, for the double purpose of washing out the reagent and of commencing to pick out the blastoderm from the yolk.

The process of freeing the blastoderm from the yolk is a delicate operation and more easily described than done, as it requires much skill and practice in manipulation. The proportion of failures to successes is at first very large. I proceed as follows. The egg naturally falls upon its side, as represented in the accompanying diagram, Fig. 2. The first manœuvre is to spear it right through the middle with a needle, in the spot to which the two arrows are pointing. Then, the impaling needle being held still, a snip is made with another needle in the direction of one of the arrows, after the manner described above, and, immediately after, a second snip in the direction of the other arrow, so as to cut the egg into two halves across its equator. By this method, which is very easy after a little practice, the egg with its enveloping chorion is cut in half cleanly and sharply, without injuring anything, in a way that would be impossible with scissors or knife. One half, of course, contains the blastoderm resting on a hemispherical mass of semi-fluid yolk, and enveloped in a cup-shaped chorion. With a little care the yolk and blastoderm can now be drawn right out of the chorion, which henceforth troubles us no longer.

We now have the blastoderm resting on a rather amorphous mass of yolk. We may be either intending to stain the blastoderm in alum carmine before it has been into alcohol, or we may wish to bring it into alcohol unstained, and then colour it with hæmatoxylin or aniline stains. In the former case, the further removal of yolk is best delayed till after staining. The yolk with the blastoderm still adhering to it is carefully taken up on a lifter (of course, with the blastoderm uppermost) and put into fresh distilled water in order to wash out the Hermann's fluid completely, and thence it is taken into alum carmine, where it should be left one to three hours. At the end of that time it is put again into distilled water, when the yolk very readily separates away clean from the blastoderm, the alum carmine seeming to have a slightly macerating action on the yolk. If, on the contrary, the blastoderm is to be brought into alcohol without staining, the process is exactly the same with the omission of the alum carmine, the blastoderm being left longer in water in order to macerate and separate off the yolk.

The blastoderm, stained or unstained, is now freed from yolk and in distilled water. It is not all plain sailing yet, however. When the blastoderms come into the weaker grades of alcohol, they become excessively sticky, and will adhere to the walls of the vessel, to the needle, or to anything they touch, coming away from it in pieces when detached. Moreover, they have a tendency to curl up. All these unpleasant habits, however, can be cured by a simple device, which I have also found applicable to other objects. It is as

follows. The blastoderm is placed in a watch-glass containing distilled water, in which a coverslip is submerged. The coverslip, of course, only touches the watch-glass with its four corners, and there is a space underneath it, between it and the bottom of the watch-glass. The blastoderm is now carefully floated on to the middle of the coverslip, so that its primitively external or upper surface is downwards and touching the glass. Then with a syringe or pipette the water is carefully drawn off till it falls below the level of the coverslip, so that the blastoderm is left stranded on the coverslip. As soon as this has been effected the coverslip can be lifted up with a forceps with the blastoderm upon it. The blastoderm must not be allowed to dry completely up, but all the superfluous moisture must be drawn off, so that it is pressed closely to the coverslip by capillary attraction. Its cells will then, in most cases, stick very tightly to the coverslip, which is now turned over and transferred to a second watch-glass containing 30 per cent. alcohol, in which it is placed, with the blastoderm *downwards*. It not unfrequently, however, happens that the blastoderm becomes unstuck and flies off with great rapidity when it comes into the 30 per cent. spirit. When this occurs the same performance must be gone through, in 30 per cent. alcohol, as has just been done in distilled water, and the coverslip, with the blastoderm stuck on to it again, is removed into 50 per cent. spirit, of course with the blastoderm downwards. It very seldom comes unstuck a second time. The coverslip, with the attached blastoderm, can now be transferred with the greatest ease from one liquid to another, and stained, if necessary. Finally, it is brought into absolute alcohol, and then into a watch-glass of oil of cloves, without, however, there being sufficient oil of cloves to immerse the coverslip, but only enough to wet the side to which the blastoderm is attached. The coverslip is then placed on a slide, after being supported with wax feet at its corners to prevent the blastoderm being crushed, and mounted in Canada balsam.

In this way most admirable preparations of blastoderms can be obtained, which not only show cell-structure and nuclear division to perfection, but also are all that could be desired from the morphological point of view; the radiating cells, for instance, which, at a late stage of segmentation, spread out from the edge of the blastoderm over the yolk, being most perfectly preserved. I have satisfied myself, by careful examination, that if the method is carefully carried out, no cells are left behind on the yolk. The method of sticking the blastoderm to the coverslip gives the further advantage that, while the blastoderm can be effectually protected from being crushed when mounted on the slide, it is only separated by the thickness of the coverslip from the objective, and can therefore be studied with any power that is desired, however high.

If, on the other hand, it is desired to cut into sections the blastoderm so removed, the process is just the same as above, with the difference that a thin slice of liver is substituted for the coverslip. I

use amyloid human liver kept in weak spirit for this purpose. The blastoderm can be made to stick to it, as a rule, in just the same way as to the coverslip, and then liver and blastoderm together can be cut into sections.

I have used Flemming's fluid instead of Hermann's, but not with such good results. Not only do the objects not stain so well after it, but they become very brittle, and therefore more difficult to extract from the egg. The best stain for general purposes is certainly alum carmine, and borax carmine should be avoided. I have obtained, however, a beautiful preparation for nuclear figures by bringing the blastoderm unstained into alcohol, and then staining it first in gentian violet dissolved in aniline water, and secondly in orange G., washing out both stains at the same time in absolute alcohol.

For chick embryos, the mode of applying this process, though essentially the same, is very different in practice. Here there is no difficulty in extracting, say, for instance, a chick embryo of 36 hours from the egg and freeing it from the yolk, but it is liable to curl up completely in the reagents, and then to get so brittle that it is impossible to straighten it. The method I have employed with good success is as follows. The egg is opened and the blastoderm extracted in the usual way in warm salt solution. The blastoderm is then dipped out with a flat shallow dish, such as the lid of a capsule. Here it is floated on to a small piece of black paper, in which an oval piece has been cut out of the middle, slightly larger than the area pellucida. The blastoderm is placed on the paper in such a way that the embryo and area pellucida are over the hole in the paper. By pressing down the edge of the blastoderm on to the paper with a needle all round it can be made to stick. This part of the process requires rather careful manipulation. The next thing is to carefully remove all the salt solution by means of a syringe, so that the piece of paper with the blastoderm attached is left stranded on the bottom of the vessel. Hermann's fluid is then carefully poured in, in sufficient quantity to cover the paper and the embryo. After it has been in this for a few minutes (until the area opaca is beginning to turn light brown), the paper with the attached embryo may be picked up with a pair of forceps and transferred to distilled water to wash out the reagent. It will be found that the Hermann's fluid has given the blastoderm a certain amount of rigidity, which enables it to resist the strain of being lifted about, combined, however, with a small amount of flexibility. Flemming's fluid, on the other hand, makes it very brittle and therefore more liable to break. If, moreover, it has been properly stuck down on the paper at first (which requires some practice), it will remain sticking and will not curl up. After being washed in two or three changes of distilled water it can be stained, which is best done in alum carmine if it is to be mounted whole, or in carmalum if it is to be sectioned. For mounting it whole there is no need to remove the black paper, the presence

of which makes it easier to shift the blastoderm from one liquid to another, while it does not, of course, prevent the embryo from being studied under the microscope, if it has been carefully stuck over the hole in the paper. If the embryo is to be sectioned it can either be carefully removed from the paper with a needle, after having come into strong alcohol, though it is then difficult to prevent it curling up a little; or the embryo may be embedded, paper and all, in paraffin, and the paper then carefully cut away with a sharp knife by paring down the block of paraffin until only the embryo and area pellucida are left.

Early chick embryos mounted in this way, after Hermann's fluid and alum carmine, in the first place show the general feature (medullary groove or canal, notochord, primitive streak, etc.) most excellently as low power objects, but also show the most perfect cell detail if carefully focussed with a high power. The branching mesenchyme cells are especially well-preserved, and the delicate network formed by their processes is beautifully shown, giving a wonderfully life-like amœboid appearance when focussed down. Nuclear figures are to be seen nearly everywhere. I have not seen any method which gives better results that this very simple one, which only takes about four hours from the time of opening the egg to the time of mounting the embryo in Canada balsam. The alum carmine gives a good stain, but one which is rather too delicate for thin sections, for which carmalum is preferable.

I will finally describe how this method may be applied to Protozoa, say, for example, *Paramecium*.[4] A drop of water containing the animals is placed on a slide and covered by a cover-glass supported at the four corners by wax feet. The drop should be as free as possible from *débris* of all kinds, since it hinders the free passage of liquids through the space under the coverslip. The wax feet should be high enough to keep the Paramecia from being squeezed, but low enough to prevent them being able to move very fast; that is to say, the coverslip should just not touch them. A drop of Hermann's fluid is then placed at the side of the coverslip and cautiously drawn through by blotting-paper (or better, filter paper) placed at the opposite side of the coverslip. In doing this it is necessary to be very careful that the living and freely swimming animals are not swept out from under the coverslip by the current before the reagent has reached them. This may be prevented with a little trouble by drawing the Hermann's fluid half through, and then putting another drop the other side of the cover-glass and drawing it back again. When once, however, the Hermann's fluid gets to them, it not only kills them instantly but also sticks them to the slide, so that rapid currents can be drawn over them without their moving at all. As

[4] This method is really a modification of one which I learnt in Professor Bütschli's laboratory in Heidelberg in 1888. Then I used Flemming's fluid instead of Hermann's.

soon as all the animals have in this way been killed and fixed on to the slide, all difficulty is passed. After the Hermann's fluid has been allowed to act for a few minutes it is washed out with water, drawn through in the same way by filter paper, and when well washed, the stain, alum carmine or carmalum, is run in. This should be left to stand for an hour or so in a damp chamber to prevent evaporation, and then washed with water and brought up through the alcohols into oil of cloves, which is finally succeeded by Canada balsam.

This method, apart from its simplicity and easiness, has the advantage for such forms as Ciliata of preserving cilia, undulating membranes, etc., very well indeed, besides giving good results for the nucleus and internal protoplasmic structures. I do not find it so successful, however, for Amœbæ, for which osmic acid or its vapour, followed by picrocarmine or carmalum, gives better results, though more difficult to apply, since the objects do not become fixed to the slide.* I have obtained a most beautiful and instructive series of sections through a *Pelomyxa* which was fixed with osmic acid and stained with carmalum, the sections being further coloured on the slide with various aniline stains. On the other hand, the Hermann's fluid and alum carmine method succeeds very well for *Actinosphærium* and for Gregarines.

I may mention in this connection that I obtained an excellent preparation of a plasmodium of *Badhamia*, that was creeping over a slide, in the following way: after fixing with osmic vapour and staining with picrocarmine,[5] it was floated on to a coverslip in 30 per cent. alcohol, in a watch-glass, and made to stick to it by drawing off the liquid, in the same way as has been described above for cephalopod blastoderms. Not only was the general form of the plasmodial network well preserved, but the minute structure, especially the nuclei, were beautifully shown.

I have now described ways of applying this method of a short exposure to Hermann's fluid, followed by a carmine stain, to three very different classes of objects, which may, perhaps, serve as types for yet others. I believe, for instance, that it would prove a valuable method for studying the segmentation of the egg of the fowl, or of other meroblastic ova. The fixing reagent in question was, however, invented by Hermann to be applied to larger objects for a much longer time, in order to study all detail and nuclear figures, just in the same way as Flemming's fluid is usually employed. Hermann originally employed it for the testis of the mouse and salamander, the organ being removed and hardened *in toto*. I have also obtained very good results, especially as regards nuclear figures, for the testis of the axolotl. Two were removed from an axolotl in October, and placed in Hermann's fluid, one for three days, the other for three months. In the latter the structures were a little darker, but not

[5] Weigert's picrocarmine, prepared by Dr. Grubler of Leipzig.

otherwise altered. They were then washed in running water under the tap for at least 24 hours. It is frequently necessary to wash objects in this way, and when they are small they are liable to be lost. I may recommend, therefore, the following simple and absolutely safe piece of apparatus. A piece of thick glass tubing, about nine inches long, is closed at one end by having a piece of gauze tied over it. It is then supported with the closed end downwards in a vessel about six inches in depth, which stands in the sink.[6] The object to be washed is placed in the tube on the gauze, and water is allowed to trickle from the tap into the upper open end of the tube. The water passes over the object and through the gauze into the outer vessel. When the latter is full it overflows down the sink. In this way even very minute objects can safely be left all night exposed to the current, provided they cannot pass through the gauze. At the same time they are in the direct current.

The testes after being thus washed were hardened in alcohol and cut into sections, and stained on the slide with various anilines,[7] and in this way most successful preparations of nuclear figures were obtained, giving a beautiful differential stain in combinations of different anilines. I have also obtained very good sections of *Lumbricus* in this manner, but here, unless the sections are very thin, they require to be bleached before staining, owing to granules in the cells being blackened by the osmic. Bleaching was done by placing the slide of sections in a bottle of 80 per cent. alcohol, having previously covered the bottom of the vessel with a layer of crystals of chlorate of potash.[8] A few drops of concentrated hydrochloric acid were then cautiously added, and the bottle subjected to slight warmth (by placing it on the paraffin bath). In about an hour's time the chlorine evolved has bleached the sections.

I have above briefly mentioned a method of fixing the cephalopod blastoderms on to thin slices of liver in order to section them. This method is also so useful for orientating minute objects in paraffin for

[6] It is not, of course, in any way necessary that the glass tube and the vessel should be nine inches and six inches in height respectively, but only that the tube should be considerably longer than the vessel is deep, in order that the water which runs through is obliged to pass through the gauze, and cannot overflow from the top of the tube.

[7] For all the anilines used, namely, Safranin, Gentian Violet, Eosin, Fuchsin, Fuchsin S., Dahlia and Orange G., the following recipe (recommended by Hermann, *loc. cit.*) was used for making up the solutions:—

Staining substance (dry)	1 gramme
Absolute alcohol	10 cc.
Aniline water	90 cc.

Anilines made up in this way stain very intensely and sharply, washing out being done in absolute alcohol. Safranin may, however, be washed in acid alcohol. These solutions do not, however, keep very long, especially Gentian Violet, Eosin, and Dahlia, and require to be used fresh.

[8] This method was, I believe, first described by Kühne, as a means of depigmenting retina; it can be used both for objects *in toto*, or, as here described, for sections on the slide.

cutting sections that, at the risk of stating what is already well known, I venture to give an account of how to use it.[9] Let us suppose that the object to be cut is either small and circular, with, however, a very definite axis of symmetry, like a cephalopod blastoderm, or that it is minute and with differentiated axes, like a Paramecium or a sponge larva, and, further, that it is required to cut sections through it having a definite relation to its planes of symmetry, *i.e.*, either transverse or longitudinal. Cut a thin slice by hand from the liver and bring the slice and the object into 90 per cent. alcohol. The slice of liver need not have any particular outline. Place the slice of liver on an ordinary section-lifter, without, however, removing it from the alcohol. Then, holding the lifter with the liver on it in one hand, with the other float the object on to the middle of the piece of liver. Now raise the liver, with the object on it, carefully out of the alcohol, and with the hand that is free let a drop of Mayer's glycerine and albumen solution, such as is used for sticking sections on the slide, fall right on the object, and then at once immerse liver and object again in the alcohol. If this is properly done (it requires a little practice) the albumen will at once coagulate round and over the object and stick it firmly to the liver. If it is not rapidly immersed in the alcohol after dropping the glycerine and albumen on it, the glycerine is liable to cause skrinkage in the object.

We now have the object fixed on the slice of liver, as shown in the accompanying Fig. 3 (p. 115). The next process is to put the piece of liver, still in alcohol, under a lens or low power, and with a sharp knife, carefully cut one side parallel to the object along the dotted line, *a b*; after which the other side can also be cut parallel by the eye along the line *c d*. We now have our object fixed to an oblong piece of liver in such a way that the long axis of the liver corresponds to the long axis of the object, and it is now quite easy to imbed it and cut it, liver and all, transversely or longitudinally at will.

I will finally mention a method which I have found most useful in cutting sections of eggs containing yolk, such as cephalopods, or hard chitinous objects. It was first, I believe, described by Heider.[10] Two solutions are made, one of celloidin, the other of gum mastix, in ether, to which a small quantity of absolute alcohol (about $\frac{1}{10}$ of its volume) has been added. The mastix solution should be thick and syrupy, and the celloidin also as thick as can be got.[11] Equal quantities of these two solutions are mixed together and put away for future use.

[9] I do not know who first invented this method, but I myself learnt it from my friend, Dr. Otto Maas, during my stay in Naples.

[10] "Die Embryonalentwickelung von Hydrophilus piceus, L." Erster Theil, Jena, 1889, p. 12.

[11] Ordinary collodium may be used instead of the celloidin solution.

When a difficult object has to be cut, a small quantity of the mixed mastix-celloidin solution is put into a tube or small bottle, and diluted with ether mixed with a little absolute alcohol until it is quite thin and fluid. We will suppose that, meanwhile, the block of paraffin containing the embedded object is fixed in position on a Jung microtome, or some similar kind of instrument, ready to be cut. Before cutting each section the surface of the paraffin block is painted over with the thin mastix-celloidin solution by means of an ordinary soft paint brush. It almost instantaneously sinks in a little way into the paraffin, and, in the same instant, the ether evaporates, leaving the mastix-celloidin mixture behind. The excess of the mastix-celloidin is then rubbed off with the finger, and the surface of the paraffin block should be polished with the finger by rubbing it several times. If now a section is cut, it will be found to be held together by the mastix-celloidin mixture which has penetrated into the object, and thus prevents the breaking up of the section that so often follows when yolk or chitin is cut. By this simple, if rather tedious, method, I have obtained uninjured series of sections not only through eggs containing yolk, but also through the thorax of a large mygaloid spider, in which the chitin had not been softened in any way. I have sometimes also found it necessary for things stuck on liver, as the liver is liable to become very brittle.

E. A. MINCHIN.

V.

Recent Additions to our Knowledge of the Eurypterida.

SINCE the publication of Dr. Henry Woodward's "Monograph of the British Fossil Merostomata" in 1878 (3) the number of important papers dealing with this group has been comparatively small. The additions to our knowledge of the true structure of these interesting Palæozoic forms have, however, been of great value, and their zoological affinities may be said to be fairly well established.

The classical works of Huxley and Salter (1), Hall (2), and Woodward demonstrated that the Eurypterids were Arthropods whose body consisted of a carapace followed by twelve free segments and terminating in a telson, the surface being more or less covered with scale-like markings. The under surface of the carapace bore a number of legs—five pairs, according to the above-cited authorities, and it is in connection with these that some of the most important additions to our knowledge have been made. These legs consist typically of a basal joint, the inner margin of which is armed with teeth and serves as a jaw, much as in *Limulus* or *Apus*. At the posterior end of the tooth-bearing margin is attached (6) a small oval epicoxite exactly similar to that found in the same place in *Limulus*, and at the anterior angle is attached the long, usually six-jointed ambulatory appendage.

The last pair of legs is characterised in all members of the group, except *Stylonurus*, by its greater size and usually somewhat flattened form, and ends in an oval plate. This is usually termed the swimming foot, but it seems more probable that it was used for anchoring the animal firmly in the soft mud of the sea bottom, and possibly also for shovelling up the sand and mud when the beast wished to conceal itself.

In *Pterygotus* the most anterior pair of appendages are large pincers, probably prehensile in function, and evidently attached in front of the mouth, as jaw bases are wanting. They seem to have consisted of only three long joints, though owing to the crumpling of the cuticle of the proximal joint there is often an appearance of a greater number. Behind the mouth there are—according to Schmidt (4)—four pairs of ordinary walking legs, followed by the large "swimming

feet." This would make the whole number of appendages six instead of five, as described by Huxley, who only recognised three pairs of walking legs.

In *Slimonia* and *Eurypterus* the full complement of post-oral appendages (five pairs) has long been recognised. In the former the first pair, described by Woodward (3), and by him termed "antennæ," is modified to form tactile organs, but is undoubtedly post-oral in position, as the basal joint bears teeth. Pre-oral appendages have been described in *Eurypterus fischeri* by Schmidt (4) as a pair of fine antenniform structures. In *Slimonia* and in *Eurypterus scorpioides* and *E. conicus* the pre-oral appendages have been described (6) as small stout pincers, much like the cheliceræ in *Limulus*, and corresponding generally to the big pincers in *Pterygotus*, though probably masticatory in function rather than prehensile. The difference between these appendages as described in *E. fischeri* and *E. scorpioides*, etc., is very great, but we must wait for further evidence before making the apparently logical change of dividing *Eurypterus* into two distinct genera, or even families. There would be a certain awkwardness in founding one's classification on a structure which has only been described in three species out of the forty or fifty in the genus.

The legs in the other genera of Eurypterida are less well-known. In *Stylonurus* the last two pairs are enormously elongated. The other legs are only known by fragments, but among the fragments figured in a recent paper by Hall and Clark (5) is a well-marked pair of cheliceræ, which were no doubt pre-oral in position. Limbs intermediate in form between the elongated type of *Stylonurus* and the broad "swimming feet" of *Eurypterus* have been described in *Dolichopterus* (2) and *Drepanopterus* (8).

The hard parts, other than appendages, on the ventral surface of the carapace consisted of the epistoma in front of the mouth and the metastoma behind it. The epistoma was described by Huxley and Salter in *Pterygotus*, but they were misled by the direction of the scale markings on it, and tried—as pointed out by Schmidt—to get it in hind side before. It consists in *Pterygotus*, in which it is best known, of a thin, semicircular plate, which covered the space between the anterior margin of the carapace and the mouth, the pre-oral appendages being attached close to its posterior border. The reversed position of the scale markings, which have their convex side directed forward, would seem to point to its being morphologically the inturned front border of the carapace. It probably corresponds to the hypostoma of Trilobites, which occupies the same position.

The metastoma is a heart-shaped plate attached along the middle line to the ventral wall of the body, between the bases of the last pair of legs and extending outwards and forwards so as to enclose the jaws in a kind of chamber.

The margins of the carapace are often bent over on to the ventral surface to a greater or less extent. This is particularly well-marked

in *Stylonurus*, where more than half of the under sufacee is covered in this way.

The ventral surface of the free segments of the body is much less well-known than that of the cephalothorax. The first two segments are covered by the genital operculum, which consists of a pair of plates meeting in the middle line and having a median lobe attached to them. This median lobe varies in form in the same species according to the sex, and may be considered as a copulatory organ in the male and an ovipositor in the female. In some species, *e.g.*, *Slimonia*, one form of this organ shows distinct signs of having been eversible. Underlying this genital operculum are a number of leaf-like structures, almost certainly branchial in function, which appear to have been attached to the body-wall, not to the operculum, and which, therefore, probably represent the reduced appendages of the second free segment. This segment has lost its ventral hard skeleton, and the appendage has become reduced owing to being covered by the genital operculum.

The appendages of the segments behind these first two have only recently been made out. Schmidt (4) describes them in *Eurypterus fischeri* as consisting of a series of four pairs of plates, each pair united in the middle line, and covering the whole ventral surface of the segment to which it belongs. The ventral surface of the body is, according to his description, entirely devoid of sclerites in these segments, and the plate-like appendages bear branchiæ on their posterior surfaces. He describes, though with less detail, a similar arrangement in *Pterygotus ossiliensis*. In *Slimonia*, on the other hand, the abdominal appendages have been described (6) as consisting also of four pairs of plates, which, however, with the exception of the first pair, do not meet in the middle line, but are successively smaller from before backwards. The ventral surface of the segments has, further, well-developed sclerites extending across the whole width of the body. In both cases the chief point is established that the Eurypteridæ had plate-like abdominal appendages bearing branchiæ on their posterior surfaces, and, in a general way, comparable to those of *Limulus*.

Fragments of some very interesting forms have been described from the Lower Carboniferous by Peach (7) under the name of *Glyptoscorpius*. These fragments appear to have belonged to a large animal (over 1 foot in length), the surface of whose body was covered with highly-developed scale markings. The limbs ended in a double claw similar to that of the Scorpions, and the animal was further provided with a pair of appendages closely resembling in structure the pectines of Scorpions. This form was probably aquatic in its habits, and represents not the direct ancestors of Scorpions, for they had appeared long before, but a collateral branch. True Eurypterids existed simultaneously with this form, though possibly many so-called Eurypterid fragments from the Carboniferous belong really to *Glyptoscorpius*.

As regards the zoological position of the Eurypterids, the establishment of the number of cephalothoracic appendages as six, the first pair being pre-oral and—at all events, in most forms—chelicerae, seems to considerably strengthen the argument for their relationship to the Arachnida. The presence of abdominal appendages also makes them agree much more closely than before with *Limulus*, and, if the lung-books are to be derived from such appendages, with *Scorpio*, &c. It has been pointed out, however, (6) that the great development of the genital operculum and consequent partial suppression of the second free segment is a point of some morphological importance, and contrasts strongly with the development of the same parts in *Limulus*, and even more strikingly in *Scorpio*. On the other hand, it agrees very closely with the arrangement in the Pedipalpi, in which a similar suppression of the second segment has taken place, and it seems probable that these latter are more nearly allied to the Eurypterids than are the Scorpions. This view would tend to separate *Glyptoscorpius* somewhat from the Eurypterids, but as the only evidence of relationship is the presence of scale-markings which are common to many other forms (Trilobites, Phalangidæ, etc.), there can be no serious objection to this. The relationship of these forms to the Crustacea is not so evident, as they show no special points of affinity with any one group. The absence of that special modification of three pairs of appendages to serve as mouth organs, which is characteristic of all crustacea except the Ostracoda, indicates that their point of union must have been very low down the Crustacean stem, and the very definite number of segments and arrangement of appendages in the Eurypteridæ indicates on the other hand that they are removed a considerable distance from any such primitive type. Insofar as they are somewhat more primitive forms than the recent terrestrial Arachnids, they may undoubtedly be said to be nearer to the point of union of the Crustacean and Arachnid lines of descent, but, unless great, and, it seems to me, undue importance is to be attached to their aquatic mode of life, they can hardly be termed intermediate forms.

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MALCOLM LAURIE.

VI.

Supposed Fossil Lampreys.

SOME months ago, in dealing with our knowledge of the "Forerunners of the Backboned Animals,"[1] reference was made to a singular little organism, *Palæospondylus gunni*, which seemed to indicate the existence in the early Devonian period of a highly-organised member of the group to which the modern lampreys and hag-fishes belong. This fossil, it will be remembered, was discovered by Dr.

Restoration of *Palæospondylus gunni*, somewhat enlarged, by Dr. R. H. Traquair. *c.* cirri; *t.p.* anterior part of cranium; *p.a.* posterior part of cranium; *x.* paired plate-like structure behind the head.

Marcus Gunn in the Lower Old Red Sandstone of Caithness, and our notice was based partly upon the brief original description by Dr. R. H. Traquair, partly upon some specimens obtained by Mr. Donald Calder, of Thurso. We are now glad to announce that, within the last month, Dr. Traquair[2] has published a fuller and more accurate description of the organism, based upon new specimens; and the

[1] NATURAL SCIENCE, vol. i., pp. 596-602 (1892).

[2] R. H. Traquair, "A Further Description of *Palæospondylus gunni*, Traquair," *Proc. Roy. Phys. Soc. Edinb.*, vol. xii., 1892-93, pp. 87-94, pl. i.

announcement is made with all the more satisfaction, since we are able to confirm his emendations and conclusions from a personal examination of a fine series of specimens kindly lent by Mr. Wm. Tait Kinnear, of Forss. Thus far the fossil has only been met with in a single stratum at one quarry near Thurso, and the circumstance is a striking illustration of the accidental character of palæontological discovery, inspiring hope for the future in problems which yet seem beyond solution.

Dr. Traquair appends some good figures of his new specimens of *Palæospondylus*, but we only copy his restored outline in the accompanying illustration. It is now proved that the appearance of a great ring-like lip-cartilage, indicated in our former figure in NATURAL SCIENCE, is due merely to the crushed rim of the anterior part of the skull (*t.p.*), which Dr. Traquair thinks may be equivalent to the palatine region of the lamprey's skull. There is still, however, no evidence of jaws; and it is not quite certain whether the nose was single or paired. The supposed dorsal shield behind the head curiously proves to consist of a pair of oblong plates, apparently not external, but certainly not yet capable of interpretation. Between these the ring-vertebræ are spaced, but behind they form a continuous chain. There are short and stout neural spines in the abdominal region, but no ribs; and the slender neural and hæmal spines in the short caudal region prove to be as already described. No traces of paired limbs can be detected in any specimen.

That *Palæospondylus* is one of the forerunners of the modern Marsipobranch fishes, as Dr. Traquair originally suspected, is thus rendered still more probable by the latest researches. We can only hope that, ere long, similar skeletons may be discovered in a finer and more compact matrix than that of the Caithness flagstones, where the bituminous character of the fossils usually obscures the smaller details.

A. SMITH WOODWARD.

VII.

The Origin of Monocotyledonous Plants.

IN the recently-issued number of the Linnean Society's Journal (vol. xxix., p. 485) is a paper by the Rev. George Henslow, entitled "A Theoretical Origin of Endogens from Exogens through Self-adaptation to an Aquatic Habit." Endogens are, of course, monocotyledons and exogens dicotyledons, and the author argues that the former originated from the latter as the result of adaptation to an aquatic habit. It may be of interest to see on what grounds he bases this theory of the origin of one great group of angiospermous plants from the other.

Of geological evidence there is none, and in the paragraph headed "Survivals" the calculation is not easy to follow. We read, "the belief that endogens are of very early origin is supported by the fact that so many orders of this class include very few genera. Thus, according to the 'Genera Plantarum' of Bentham and Hooker, there are 13 orders out of 166 which have only four or a less number of genera, while two orders have six. Taken together, therefore, these amount to nearly 8 per cent. of the whole class. It need hardly be added that monotypic animals and plants, as well as those orders and genera with but few members, are always regarded as survivals on the principles of evolution and represent a lost ancestry." According to the "Genera Plantarum" there are only 34 monocotyledonous orders, 15 of which, or 44 per cent., contain six genera or less. 165 orders of dicotyledons and three of gymnosperms are recognised. That there is some connection between an aquatic habit and endogenous structures is evident from the proportion of aquatic orders; for, while only 7 per cent. of exogenous British orders are aquatic, the proportion of endogenous orders is 53 per cent.; and of the 224 exogenous orders given in Le Maout and Decaisne's "Analytical Botany," nine may be regarded as aquatic, or 4 per cent.; and of 55 endogenous orders, 18, or nearly 33 per cent.

The numerous points of similarity in morphology, anatomy, and histology between the two groups are adduced as evidence of community of descent. Indications of an adaptive character are also to be seen everywhere, and the author's object "is to show that many of the characters common, more especially to the vegetative organs of endogens, and regarded as points of affinity, are just those

which are characteristic of the adaptations of exogens to an aquatic, and, in some cases, to other habits of life."

As regards the points of difference between the two groups, a most important one lies in the structure of the embryo, in the presence of one or two cotyledons. To quote again, "assuming a monocotyledon to have descended from a plant with two cotyledons through an aquatic habit, do we find any instances in support of this view among existing dicotyledons, which are now aquatic, or have presumably descended from water-plants?" *Trapa natans* is instanced. The view generally taken of the embryo of this plant is that there is one perfect cotyledon, and one rudimentary. Drs. Gibelli and Ferrero, who have lately studied the embryology, consider, on the other hand, that the mature embryo is an amorphous mass or true thallome on which is developed a single bud, the plumule; it is, in fact, a degradation resembling that of parasitic and semiparasitic plants, such as Orchids, *Orobanche*, *Balanophora*, etc., and of some aquatic genera, *e.g.*, *Zostera*, *Hydrocharis* and *Utricularia*. Hence, from the example of *Trapa natans* it is quite as legitimate, perhaps more so, to conclude that endogens originated from exogens through a parasitic as through an aquatic habit. *Ranunculus Ficaria* is given as another case. It is admitted, however, that it is not a true aquatic plant, but has, the author thinks, without doubt descended from an aquatic ancestor, one of the reasons being that it has only one cotyledon! This may be, but it is rather begging the question to use it as an example in support of the aquatic theory.

The most instructive instance, however, it is said, would seem to be that of *Carum Bulbocastanum*, which has a monocotyledonous embryo, and an embryology almost identical with that of *Sparganium ramosum*, while its final form is that of the wheat embryo. *Carum Bulbocastanum*, however, inhabits chalky fields, and, like *C. Carui*, in which also one cotyledon is frequently rudimentary, is decidedly not aquatic. Again, the difficulty is got over by supposing that these two species, as well as other umbelliferous genera with linear cotyledons and finely dissected foliage, were ancestrally aquatic; but linear cotyledons are no proof of an aquatic habit, as will be seen by turning over the pages of Sir John Lubbock's "Seedlings"; in the case of umbellifers the shape may facilitate their escape from the often tough-walled fruit. *Hydrocotyle vulgaris*, the aquatic umbellifer *par excellence*, has ovate cotyledons. Anyhow, we can hardly accept these as evidence.

Utricularia, again, has the undifferentiated embryo so characteristic of Orchids and other parasites, and cannot be classed as monocotyledonous. *Erigenia bulbosa*, an anomalous umbellifer, growing in damp and flooded places, has also a very reduced embryo.

In *Cyclamen* the cotyledons first appear as two small eminences, one of which grows no further, while the other forms a large green leaf, resembling in all respects a foliage leaf. Gressner, however,

maintains that both cotyledons become leafy, but in any case the embryo and early stages of growth of *Cyclamen*, which, whatever its ancestry, is certainly not now aquatic, bear no great resemblance to that of an ordinary monocotyledon.

Mr. Henslow admits that more than one cause may produce the same or analogous effect, and that while he attributes the total or partial arrest of one cotyledon to an aquatic habit, a similar arrest may arise from atrophy in consequence of the way in which the embryo is folded in the bud, as Sir John Lubbock has shown to be the case in *Abronia arenaria*. The fact, however, remains that not a single undisputed instance is given of the arrest of one cotyledon producing a monocotyledonous embryo in any aquatic exogen. The wholesale degeneration to a quite undifferentiated embryo, as obtains, for instance, in *Utricularia*, does not help the theory.

In the section headed "The Embryo" the author refers to an observation of Van Tieghem's "that the angular divergence between the cotyledon and the next new organ is 180° (generally in endogens), or, if lateral, 90° (as in exogens). When this latter case occurs in endogens, it implies that one cotyledon is due to an arrested condition of the other of two opposite cotyledons; so that the first leaf belongs to a second pair decussating in position with the cotyledons. If the first leaf be at 180°, then it would appear to have usurped the position of the cotyledon that is lost; but it is not strictly on the same level as the cotyledon, inasmuch as the first leaf is completely included within it, as may be seen in *Asparagus*, figured by Irmisch." Thus whether the first leaf alternates with or is opposite to the cotyledon the latter may be supposed to represent the survivor of a pair. No instance is given of the alternate position, but *Tamus communis* is quoted as a parallel instance to *Asparagus*, in which Dutrochet has shown that the first leaf is exactly opposite the cotyledon, is very rudimentary, and dies early, and he has no hesitation in calling it a second cotyledon. The appendage to the embryo opposite the single cotyledon of certain grasses, which several botanists regard as a rudimentary cotyledon, is also instanced.

We can, however, hardly accept these as proof of the origin of the endogenous embryo from an exogenous by assumption of an aquatic habit. In reflecting on the monocotyledonous embryo, we are struck with the variety of form manifested, rather than by evidence of arrest or degeneration. Of course in Orchideæ we see the degeneration, or want of differentiation, characteristic generally of a more or less parasitic habit, and the same holds in Burmanniaceæ; but in the Palms, Grasses, Scitamineæ, and Bromeliaceæ the cotyledon is a complex structure, comprising two distinct, and often clearly-separated portions; one the sucker, resident in the seed, dissolving and absorbing the store of endosperm, the other the sheath, pushing above ground, becoming green, and protecting the developing plumule. Many Irideæ have embryos built on the same plan, but

occasionally, as in *Sisyrinchium*, and sometimes in *Iris*, the apex of the cotyledon, having functioned as a sucker, follows the lower part out of the seed, the whole appearing above ground as a subulate green leaf in the characteristic liliaceous fashion. In the Hydrocharideæ and Naiadeæ, the hypocotyl and cotyledon are sometimes coherent into a more or less oval mass terminated by a small radicle, but in others, as *Stratiotes* and *Halophila*, the single cotyledon is distinct from the hypocotyl, and in the former genus large and fleshy. It is also large and distinct in *Alisma*. The absence of distinction between hypocotyl and cotyledon crops up now and again in dicotyledons, and is not necessarily connected with an aquatic habit. It has been already mentioned in *Utricularia*, but is also well shown in the Brazil nut (*Bertholletia*) and another closely-related myrtaceous plant, *Lecythis Zabucajo*.[1]

The sheathing petiole so characteristic of the leaves of monocotyledons, is next brought forward in support of the theory, as it also obtains in many aquatic exogens, *e.g.*, Ranunculaceæ, Umbelliferæ, etc. It is especially characteristic of the Umbelliferæ, but unless we assume this an aquatic order, it seems equally well adapted to land plants, especially herbaceous ones with large leaves, where it will materially strengthen their attachment to the main axis. In the enormous leaves of palms this factor must be a very important one; in fact, it seems to supply a more rational explanation than the persistence of an adaptation to an aquatic habit. If such a habit is the cause of the sheathing form of leaf, why is the form so extraordinarily well-developed in the grasses, which is not an aquatic order. The argument is, however, pushed still further, and it is suggested that the coleorhiza, the sheathing portion which encloses the root in grasses, is a development of the sheath, representing the decurrent sheaths of two united cotyledons, or, perhaps, of one only completely investing the axis.

The "pseudo-coleorhiza" of *Tropæolum* is introduced as throwing light on the origin of the root-sheath in endogens; and the author proceeds to adduce several points in favour of an ancestral aquatic habit for the genus, as nowadays it is, unfortunately, a land plant.

In the first place, however, as Mr. Henslow himself mentions, the structure in *Tropæolum* is formed from the bases of the cotyledons, which are sagittate, being produced downwards on either side as four lobes surrounding the embryo, so that if we suppose them to be completely welded together, "they would form a perfect coleorhiza over the end of the radicle"; but this is no argument in favour of the origin of the coleorhiza from a sheathing petiole.

Thus we cannot agree with the statement with which the first part of the paper concludes, viz., "that there is at least enough presumptive evidence to frame a theory that the monocotyledonous embryo has been derived from a dicotyledonous one by a suppression of one cotyledon"; the cause of the suppression being "the degene-

[1] *Cf.* Lubbock's "Seedlings," vol i., pp. 539–41.

rating effect of the aquatic medium in which the ancestral endogens originally and primarily grew."

A large part of the remainder of the paper is devoted to evidences furnished by the order Nymphæaceæ, which is remarkable for having many points of agreement in structure with endogens, and, as it is aquatic, may be looked to in support of the theory.

As regards the embryo, some abnormal cases of the yellow water-lily (*Nuphar luteum*) described by Hegelmaier are cited, where the two cotyledons were more or less united, and the two halves very unequally developed. These are quoted as "an important and independent witness to the gradual arrest of one cotyledon." It seems rather a teratological phenomenon likely to occur anywhere.

In discussing the arrest of the primary root, Mr. Henslow is, we think, clearer, and adduces what is, at first sight, a more promising piece of evidence. A very characteristic feature of monocotyledons is the disappearance of the primary root, that, namely, produced by elongation of the radicle of the embryo, and the subsequent development of adventitious roots. Now this also obtains in Nymphæaceæ, aquatic umbellifers, and other water-loving exogens. Hence the argument that its presence in endogens which to-day are not aquatic, *e.g.*, grasses, is due to inheritance from aquatic ancestors. It must, however, be remembered that fibrous roots occur in exogens which are not aquatic, and as regards the grasses, one of the largest orders of Angiosperms, when we remember their habit, and how they frequently grow in loose soil or dry exposed situations, the spreading fibrous root seems to meet the case so perfectly that the hypothesis of an aquatic ancestry appears unnecessary. Following a suggestion of Mirbel's, the author explains the arrest of the primary root in endogens as follows. When the cotyledons are raised above ground and act as leaves, they can maintain the existence of the tap-root and enable it to grow until the foliage of the plumule is sufficiently developed to carry on the work. If the cotyledons remain below ground, "the plumule *at once* develops its leaves perfectly, as in the oak, and its foliage can then nourish the tap-root instead of the cotyledons doing so." In submerged aquatic plants the first leaves are more or less rudimentary, and, "being under water, their assimilating powers are greatly impeded," so that the primary root perishes for want of nourishment.

Now, as a matter of fact, as Sir John Lubbock points out in his "Seedlings" (p. 356), when the cotyledons remain beneath the ground the plumule does not, as a rule, "*at once* develop its leaves perfectly," but the primary ones are reduced to scales, often passing gradually into the form of the adult leaf. This happens in the oak, and the tap-root is nourished, not by the plumule, but by the fleshy cotyledon, which also supplies nutriment for the growth of the stem. In this way the first green foliage leaves are borne well up in the air and raised above the humus, dead leaves, etc., which might have buried

the seed. Mr. Henslow observes that the seeds of aquatic endogens are generally exalbuminous, but as the hypocotyl or hypocotyl and cotyledon are very much enlarged, there seems no more reason for starvation of the primary root than in the case, for instance, of the oak.

In the same section it is remarked that the arrest, presumably as the result of an aquatic habit, is not confined to the primary root in the Nymphæaceæ (where, by the way, there is a good stock of albumen in the seed), but is continued into the first-formed leaves which pass through various stages of development. Now, if Mr. Henslow will refer again to the work on "Seedlings," he will find that this gradation is quite the rule where the adult form of the leaf is not a fairly simple one. See, for instance, the figure of *Clematis* on p. 82, where the mature, much-divided form is only gradually assumed.

Attention is also drawn to the agreement between these and occasionally other aquatic exogenous orders and endogens in several points of anatomy. Such are the scattered closed bundles and the large wood vessels which are probably "due to the particular degenerating effect of a watery habitat." As the author himself observes, such appearances are by no means confined to water plants among dicotyledons. Any habit which allowed a relaxation in the effort for self-support, such as that of climbing, might, and in fact does, have a very similar effect. It is pointed out that the arrangement in *Peperomia* is practically identical with that of *Commelina*. Analogous degradations occur in parasites, *Cynomorium coccineum* and *Helosis guyanensis* having the bundles "scattered through the ground-tissue, exactly like any endogenous stem."

As regards the comparison of the foliage of *Victoria regia* and *Sagittaria*, considering the close relation in form between the adult leaves, *orbicular-peltate* and *sagittate* respectively, it is not remarkable that the stages by which the normal form is reached should be similar.

From its resemblance to cotyledons "the prevailing ribbon-like form of submerged leaves of endogens," seen also in *Hippuris* and *Callitriche*, is regarded as of a more "embryonic" character than the dissected type of submerged leaves of exogens, and the conclusion is drawn "that those terrestrial endogens which still retain a linear form (*e.g.*, grasses), or ensiform (Iris), or other similar type of leaf or rather phyllode, may be regarded as representing the ancestral submerged ribbon-like form"; while those with distinct lanceolate or cordate blades, like *Convallaria* or *Maianthemum*, "represent the floating types of existing aquatic endogens." The sagittate form seen in many aroids has primarily arisen like the same forms in Nymphæaceæ and *Sagittaria* from an aquatic habit. This reasoning seems based on somewhat superficial characters. We find classed together as representing one type leaves so totally different as the linear grass

leaf and the ensiform one of the Iris. The former has a well-marked dorsi-ventral structure and a clear differentiation into distinct sheath and blade, separated by the characteristic ligule. Moreover, anatomically, the blade is that of a normal aërial leaf. The Iris, on the other hand, has equally well-marked isobilateral structure and properties. It is dangerous to draw conclusions from similarity in shape. Is the tendency towards a cordate form in the geranium leaf the result of a former floating habit, and the still more divided outline in some species evidence of a former submerged state? The hypothesis of the origin of the aloëidal type of leaf from an ancestral form like *Stratiotes aloides* seems as gratuitous.

In the section headed "Venation," the author again attempts to show that the terrestrial leaves of endogens are primarily aquatic, but have taken on aërial features, such as reticulations, stomata, etc., the idea being that a parallel or curvinerved venation is an aquatic adaptation.

The facility of tearing seen in the submerged leaves of *Nymphæa*, in those of bananas, palms, and blades of wheat, is referred to an enfeebled structure primarily due to a watery life. One can, however, see a nearer cause in the case of leaves like the banana and palm, which are not at once shed from the stem, but gradually droop. This process will be aided by the tearing, and they will occupy less space, and cause less drag on the stem, than if they remained entire. We think, too, that a better explanation of the holes in *Monstera* leaves might be found than descent from an aquatic fenestrated leaf like that of *Ouviranda*.

Finally, as regards the flower of endogens, the author suggests that the local stimulations set up by insects may equalise, if not surpass, the degrading influences of an aquatic medium. Presumably, this has happened in three important aquatic orders—Hydrocharideæ, Pontederiaceæ, and Alismaceæ—where the flowers are certainly conspicuous. As, however, a large number of genera of Naiadaceæ, Typhaceæ, Juncaceæ, Cyperaceæ, and others are greatly degraded, it is claimed that "the evidence from coincidences is accumulative, while comparative anatomy justifies the conclusion that there has been a distinct 'cause and effect' in the reduction of the floral structures."

The evidence given, however, does not seem particularly conclusive, comprising a derivation of the spike or spadix of Aroids, Typhaceæ, *Potamogeton*, and others from the branched inflorescence of Alismaceæ and Juncagineæ by suppression of the flower stalks and perianth whorls, and the reference of the quaternary arrangement of the floral organs to a reversion to a very primitive condition of an opposite and decussate arrangement. The suggestion is put forward in a foot-note that the use of the lodicules of grasses is to act as reservoirs of water; it is, however, not easy to see why water reservoirs should be wanted in a grass-flower.

Having briefly reviewed the evidence advanced in favour of the theory, we can only say that whatever has been the origin of monocotyledons, their origin from dicotyledons "by self-adaptation to an aquatic habit" does not seem to have been established in the paper before us. Mr. Henslow has instanced many points illustrating a community of descent of the two groups, and also shown that, in each, plants adapted to a watery habit have certain morphological and anatomical resemblances, as community of descent would in fact lead us to expect, but in our opinion he has not established the origin of one from the other. The study of the origin of great groups is a most fascinating one, but it is also extremely difficult, and one especially in which superficial resemblances must not be pushed too far.

A. B. RENDLE.

VIII.

The Recapitulation Theory in Biology.

I HAVE read with interest the articles on the above theory by Dr. Hurst and Mr. Bather in NATURAL SCIENCE for March, April, and May. Mr. Bather states that my work has been inspired by the Recapitulation Theory; and Dr. Hurst is pleased to retort that this is a fantastic theory accepted as a creed by myself and others. Now I am not altogether sure that Mr. Bather is absolutely correct in speaking of my work as inspired by the Recapitulation Theory; what I have upheld is a "Law of Earlier Inheritance." That this law, of necessity, leads to a theory of recapitulation is no doubt true; but it is the law of Earlier Inheritance which has guided my work. Upon this law I have set forth my views, and have shown by comparisons of the ontogeny with the (say, assumed) phylogeny in Ammonites that recapitulation is and can only be imperfect.[1] As these views are fully detailed in a paper on "Some Laws of Heredity,"[2] I need not go over that ground again. Dr. Hurst will find that I have fully taken into account the similarity of embryos of dissimilar species—which seems to be a stumbling block to his acceptance of Recapitulation.

I will assume that Dr. Hurst has not read this paper, or any notice of it; in which case it is curious that, approaching the subject from a totally different point of view, he has defined variation in nearly similar, though wider, terms to what I used some months before.[3]

So far I will suppose that Dr. Hurst and myself are practically in agreement; but it is useless to discuss a question of this kind unless the one knows exactly what the other requires. My object, therefore, is to point out where it seems necessary for Dr. Hurst to amend his statement.

In page 198 Dr. Hurst denies any *causal* relation between ontogeny and phylogeny; in page 197 he says, "the more the adult structure comes to be unlike the adult structure of the ancestors, the more do the late stages of development undergo a modification of the same kind." By "late stages of development" Dr. Hurst means, I presume, premature development—a point I will notice later. Now, as it stands, the sentence conveys to the mind the idea that the premature stages of an individual become altered in consequence of alteration to the mature stages, which are not formed till later. This

[1] Inf. Ool. Ammonites (*Pal. Soc.*, 1892), part vi., p. 288, footnote 2.

[2] Proceedings Cotteswold N. Club, vol. x., part iii., p. 258, Oct., 1892. Translated into German, forming vol. xviii., series i., of *Darwinistische Schriften*, March, 1893.

[3] *Op. cit.*, p. 261.

is clearly an impossibility—any changes in the adult man of 30 cannot affect his structure when he was a boy or child; nor do I imagine that Dr. Hurst means this, though his sentence seems to imply it. If, however, he wishes to say that the more the adult man comes to be unlike his ancestors the more do the premature stages of his descendants undergo a modification of the same "kind," he has obviously admitted the causal relation between ontogeny and phylogeny which he wishes to deny.

There is another sentence in the same page which puzzles me entirely—"in order that any structure of the adult which varies, and hence ceases to exist as an adult structure at all." The word "structure" seems to be open to objection. I presume Dr. Hurst means "character," but then I do not understand the "hence," unless Dr. Hurst wishes to say "and hence ceases to exist *solely* as an adult structure." An adult "structure" may vary in the direction of becoming greater or less, and yet may remain an adult "structure," but it will not remain solely an adult "structure." By the law of earlier inheritance illustrated in Mr. Bather's table (p. 279), it gradually becomes an adolescent "structure." Thus, if the adult "structure" be x, and it varies in subsequent generations gradually to become $2x$, the adolescent "structure" will by then be x; and as in subsequent generations adult $2x$ becomes $3x$, the adolescent x becomes $2x$. Hereby we obtain a record; yet Dr. Hurst says that in order to produce a record "as the adult structure varies in one direction, the late stages of development must vary in another direction." The question is, then, what does Dr. Hurst mean by "late stages of development"? and, also, what meaning ought to be attached to the phrase? The dictionary explains development as "the series of changes in the growth from first to last of an organised being." Obviously, growth includes not only increase as a whole, which in some organisms is continuous throughout life, but it also includes increase in any part. Thus, the growth of hair on man's chest in middle life is part of his development, and the expansion of the stomach in senility is also part of his development. I contend that the late stages of development are the stages of senility; and that to give the term any other meaning is a mistake, which, I am aware, is not confined to Dr. Hurst.

Further, I contend that loss of teeth and loss of hair in man are part of his development—though they are strictly retrogressive phases of development. Then under these circumstances I must ask Dr. Hurst what he regards as acceleration of development, because he probably uses the term in a different sense to that in which I have been accustomed to see it employed.

Lastly, in regard to development, I would remark that, in any discussion, it is most important to clearly distinguish the stages of development in ontogeny and the stages of development in phylogeny; for what Dr. Hurst has written fails to convince me that there is not a close connection between the two.

S. S. Buckman.

IX.

Professor Blake "On the Bases of the Classification of Ammonites."[1]

THE above is the title of the Presidential Address to the Geologists' Association. No wonder that the members of that association were pleased. Professor Blake is just the man for the task, he is "one of the best geological critics in England"—at least, so we have been told, and we are frequently reminded of the fact; he has studied the Cephalopoda, and he has written upon them, and treated them from the mathematical point of view; he is a member of the Council of the Geological Society, and he gave his address as president of the Geologists' Association. No wonder that the members of that association, who were so thankful "for his concise account of one of the most difficult groups of animals—the Cephalopoda"—published last year, should be glad of enlightenment concerning the classification of Ammonites.

Hitherto the members of the association as a whole have been somewhat at sea about Ammonites. It was a dangerous experiment for one of them to air any of the new (?) generic names in the hearing of his fellows. The powers that be, so he was promptly told, still clung to the good old genus *Ammonites*, with its six or seven thousand species; and new generic names were tabooed. No division of this grand old genus could be allowed, even though Professor Blake had, nearly twenty years ago, shown what could be done in the generic division of Ammonites. At last Professor Blake thought the time had come to rescue the members of the Geologists' Association from outer darkness, and accordingly he has once more bravely tackled the question of these new generic names.

According to Professor Blake the chief bases of Ammonite-classification are four—the form, the size (!), the ornaments, the sutures. He mentions other characters, for instance, the length of the body-chamber, only to dismiss it with the extraordinary remark that "if the interior part of the whorl be occupied by the previous whorl, the loss of space is apt to be made up for by the length of the last chamber; but no general rule can be laid down." We should think not, except

[1] On the Bases of the Classification of Ammonites. Presidential Address. Proc. Geologists' Association, vol. xiii., part 2, pp. 24–40, 1893.

it was to state exactly the opposite. How closely Professor Blake has studied recent literature, to find that the length of the body-chamber is considered of the least value among present-day authorities! We have not been able to do so; but we do not wonder at it, because, in our opinion, to consider the *length* of the body-chamber only is absurd. What is required is the *capacity* of the body-chamber. In an evolute, divergent-sided Ammonite the length of the body-chamber would be short, but its capacity might be exactly the same as in an evolute, compressed Ammonite with a body-chamber three times the length. The capacity of the body-chamber, in proportion to the whole shell, may become a basis for classification—its mere length never can be.

The form of the Ammonite is stated to depend on the curvature (p. 25). Alluding to the fact that he published a mathematical paper on the subject, Professor Blake proceeds to explain how to measure the curvature. He gives us three methods. He first "assumes that the rate of curvature of the inner edge is the same as that of the outer." But of what use is this method among Ammonites? The rate of curvature of the inner edge must always be greater than that of the outer edge which subtends it, otherwise Ammonites would very soon fulfil the Yankee's definition of a point—"the small end of nothing whittled down." Nevertheless, our mathematician proceeds to use this wonderful method, and finds thereby that there is considerably less curvature in an involute Ammonite than in an open-whorled *Crioceras*! Now if we take his second method and apply it to the very figures he has given us—the curvatures of the involute Ammonite and of the open-whorled *Criocéras*, work out the same within 0·01.

In the next paragraph we are introduced to the wonderful "*Ammonites transmogrificabilis*, Blake;" and the Professor sets himself to contradict what he had stated in the paragraph above. As a result of his measurements he found that an involute Ammonite had a smaller curve than an open-whorled *Crioceras*; here he tells us that "*without altering the curvature*"—the italics are ours—he can make at will an involute Ammonite or an open-whorled *Crioceras* by means of some wonderful diagrams (pl. i., figs. 3, 4, 5). When, however, we come to apply to these figures his first method of estimating the curves—the method he himself thought fit to employ for figs. 1, 2, pl. i.—we find he has altered the curvature very greatly, for the ratios are fig. 3, 1·53; fig. 4, 1·44; fig. 5, 1·25; and "the greater the ratio the less the curvature" (p. 26).

Now, we confess to little mathematical faculty ourselves. To us a sum is like a joke to the proverbial Scotchman—only to be done *weeth deefeculty*; but even our inferior faculty would have suggested that to estimate the curve of an Ammonite-shell it was necessary to consider not only the peripheral, but also the antiperipheral, or inner-marginal curve. There is no difficulty in seeing that the inner-

marginal curve in fig. 4 is greater than in fig. 5, and that in fig. 3 greater than in fig. 4. We should have supposed that to estimate the curves of these shells it was necessary to take the mean of the peripheral and anti-peripheral curves—by which method, of course, fig. 3 is curved the most.

Professor Blake takes all this trouble about the curves to prove, first, that "an involute Ammonite *(Phylloceras)* has *more curvature* than an open-whorled *Crioceras*"; secondly, that "the difference between the form of *Phylloceras*, *Arietites* and *Crioceras* has *nothing whatever* to do with their curvature,"—two statements which do Professor Blake great credit, as they do not contradict each other in the least!

In dealing with the involution, Professor Blake makes the remarkable statement that Ammonites are necessarily more evolute in youth than in later life. *Am. salisburgensis*, Hauer; *Amm. gonionotum* and *fallax*, Benecke; *Amm. polymorphus* and *dimorphus*, d'Orbigny; *Am. polymerus*, Waagen; *Am. brocchi*, Sowerby; and the majority of the *Stephanocerata* support this view, we suppose. Really Professor Blake's perception must be keen; he sees what no one else could do in the matter of these Ammonites. Then he proceeds to say that Hyatt and Buckman seek an evolute ancestor for every form, and finds fault with them in consequence. How confidently the members of the Geologists' Association may rely on Professor Blake's accuracy! Hyatt ("Genesis of the Arietidæ") gives an *involute* form like *lævigatus* as the ancestor of certain species; Buckman ("Monograph," p. 283) places an *involute* shell like *globosus* as the ancestor of the Polymorphidæ, and also (p. 289) states that the first stage of Ammonite ontogeny discloses a form like *globosus*, such, therefore, is the ancestral form he would seek.

In connection with the thickness (p. 28), Professor Blake announces a discovery which is new to us: "the whorls of an Ammonite become naturally more compressed with growth." What a number of unnatural Ammonites there must be! For instance: *Amm. bechii*, Sowerby; *spinatus*, Brug.; *sauzeanus*, d'Orbigny; *semicostatus* (Young), Hyatt; *adelæ*, d'Orbigny; *francisci* (Oppel), Vacek; *sutherlandæ*, d'Orbigny; *goliathus*, d'Orbigny; not to mention many others which change from less compressed to more compressed whorls, and again to less compressed in infancy or adolescence, and change again to more compressed in adolescence or maturity. We should have thought that the only law which could be laid down safely was that, in anagenesis, the whorls tend to become more than proportionally inflated, and, therefore, less compression is likely to arise; while in catagenesis the whorls tend to be less inflated, and greater compression may result. Unfortunately, the law is not so simple as this. Catagenesis may produce a less compressed whorl, because the reduction, in proportionate capacity, may be attained by reducing the breadth and not the thickness of the whorl.

The size of the Ammonite is given as one of the bases of classi-

fication! The size! We congratulate Professor Blake on this discovery. We believe it to be entirely his own, and entirely original, and we will not say a word to destroy the charming simplicity of the idea. Incidentally, we are treated to a most interesting piece of information about "*Ammonites planorbis*, now called *Psiloceras*"—"the fry are abundant, but the race was really gigantic, as may be seen by the specimen, over three feet in diameter, in the British Museum."

It would be strange if true; but, happily, this giant is not a specimen of "*Ammonites planorbis*, now called *Psiloceras*"; it is a senile example of *Ammonites conybearii*, now called *Vermiceras*, which not uncommonly attained the size of three feet across. It was not a contemporary of *planorbis* at all; but it lived much later, namely, in the time of *Pentacrinus tuberculatus*. That the label is incorrect in the British Museum implies no fault to anyone connected therewith. For one man to arrangè the whole of the Cephalopoda—a subject which engages the attention of many specialists—is a too gigantic task; and accuracy in all cases cannot, therefore, be expected. That the public who take all the labels in the British Museum for gospel are thereby misled, is a pity; but while an overburdened museum-curator might successfully plead excuses for an incorrect label, no such plea can be allowed to such an authority on Cephalopoda as Professor Blake.

To illustrate the confusion which arises in the mind of the student—not the confusion which arises in his own mind on a subject about which he poses as a teacher—Professor Blake compares (p. 29) the generic grouping given by Buckman and by Futtlerer (*sic*), and remarks what "most observers" would do in such cases. How useful to appeal to "most observers"! "Most observers" would not make egregious mistakes in classification in any science. Does Professor Blake wish us to believe *vox populi vox Dei*; and does he forget that it was not only Carlyle who uttered a sentiment akin to *vox populi vox stultorum*? When the immortal Mr. Pickwick went to the election and shouted "Slumkey for ever!" he was asked, "Who is Slumkey?" He replied, "I don't know; it's always best on these occasions to do what the mob do." "But suppose there are two mobs," said Mr. Snodgrass. "Shout with the largest," said Mr. Pickwick. It will always be a case of the specialists against οι πολλοι. If Professor Blake wishes to emulate the immortal Mr. Pickwick, and shout with οι πολλοι, he is quite free to do so. It is to be hoped that when asked his reasons he will be equally candid, and reply "I don't know."

"Most observers," says Professor Blake, "would place the former (species put by Buckman into two genera) not only in the same genus, but almost in the same species, and the latter (Futterer's), not only in distinct genera, but in distinct families." We don't doubt it for a minute: and thereby the observers would show their profound

knowledge of ontogenetic and septal details in both cases! If a schoolboy were to class a whale as a fish, Professor Blake would be down on him for his ignorance; but the blunder in a schoolboy would be more excusable than are such remarks about Ammonites from Professor Blake.

We have given so much time to the criticisms of these various matters, that we cannot allude to numerous statements in this extraordinary paper; but we must not pass the cream of the whole article. "By comparing the figures [in the plate] . . . we seem to see how the *Stephanocerata* (pl. i., fig. 11) may have arisen from *Hammatoceras* (pl. i., fig. 10); and by comparing this with pl. ix., fig. 1, of Vacek's work almost from *Harpoceras*. In another direction we can pass towards *Perisphinctes* (pl. i., fig. 13), and in a third towards *Aspidoceras* (pl. i., fig. 12), and there is little to distinguish them in the outlines"! What skill and study must have been expended to construct such a complicated genealogy as this! We venture to say this effort has never been surpassed, not even by a would-be populariser of Science.

The *Stephanocerata* (pl. i., fig. 11)—an *inflated* shell with a spinous centre and costate outer whorl—"may have arisen from *Hammatoceras* (pl. i., fig. 10)" which is a *compressed*, wholly costate form, and has not the ghost of a spine about it, apparently; and this, "almost from *Harpoceras*" (Vacek, ix., 1) a *less* compressed fossil with a *smaller* umbilicus—evolute in youth but more involute with age! "In another direction we can pass towards *Perisphinctes* (pl. i., fig. 13)"—by a species which has remarkable and very distinct septa, namely, a peculiarly abbreviated siphonal lobe, and a lateral lobe specialised for a very particular purpose—features altogether unknown in *Perisphinctes*. "And in a third towards *Aspidoceras* (pl. i., fig. 12)"—except for septal details.

Of ontogeny Professor Blake takes not the smallest account. He writes a paper on the bases of the classification of Ammonites, and never so much as mentions ontogeny, except to admit the similarity of ontogeny to phylogeny as a guide in genealogy. Does Professor Blake know what these words mean? and has he applied to this genealogy a principle which he has utterly ignored in his paper? According to this genealogy, the facts of evolution are not given clearly enough nowadays; and we have always applied them the wrong way about. Obviously, we have to learn that the details of embryology show whither a species is going, and the details of senility which illustrate whence a species has come!

We are indeed grateful to Professor Blake and his genealogical table for giving us such information as this, and for showing us how to use it. We shall be able to correct the misapprehensions in regard to evolution under which we have hitherto laboured. For the future we can construct a genealogy like this:—Here is a monkey who grows less hairy in his old age—he shows how monkeys may have come

from man; here is a man who is losing his teeth in old age; he shows how man may have come "almost" from a whale. A fish and a whale are very much alike, "at least, there is little to distinguish them in the outlines"—in another direction, a whale may have developed into a fish. To anyone acquainted with Ammonites, remarks like these are every bit as intelligent as those of Professor Blake; but while their value can be gauged almost by boys in the schoolroom, the pity of it is that so few people are able to properly appreciate the remarks of the ex-President of the Geologists' Association.

One last sentence we will notice in this strange address. "The modern genera of Ammonites . . . do not range much beyond the ancient idea of species" (p. 38). Certainly, the following does not represent the modern idea of species. "The family of shells allied to this species is so very variable that there is no mean between naming every minute difference . . . and throwing them into groups. I have chosen the latter method, and unite all those which may be called inflated *S. commune* under the present name, and those that stand in the same relation to *S. annulatum* under the next. They seem to develope (*sic*) tubercles indiscriminately . . . , etc."—("Geology of Yorkshire (Cephalopoda)," p. 300.)

This, of course, is the ancient idea of species—date, 1874; but the old masters in Science had the merely paltry modern idea concerning species—they actually did not "throw" forms into "groups," *vide* Sciences more ancient than Palæontology; and they took the trouble—even those who dealt with Ammonites—to observe differences in involution, costation, ornamentation, and septal details. It is a pity that they did; no doubt they would have saved themselves this trouble if they could have foreseen the "ancient idea of species."

In conclusion, then, we must express our conviction that an efficient paper on the Bases of the Classification of Ammonites yet remains to be written; and it will not be accomplished by a man who—whatever he may, at one time, have done among Cephalopods—has for some considerable time turned his attention to matters entirely different, and yet imagines that with a few weeks' study he can become master of the work of several years.

SOME NEW BOOKS.

A DICTIONARY OF BIRDS. By Alfred Newton, assisted by Hans Gadow, with Contributions from R. Lydekker, C. S. Roy, and R. W. Shufeldt. Part I. 8vo, illustrated. London: A. & C. Black, 1893. Price 7s. 6d. nett.

THE appearance of a general work on birds by an ornithologist of the long experience of Professor Newton may well be regarded as marking an epoch in the science of which it treats; and the foretaste afforded by the issue of this instalment leads us to believe that this opinion will not be belied when the whole volume is completed. Like certain

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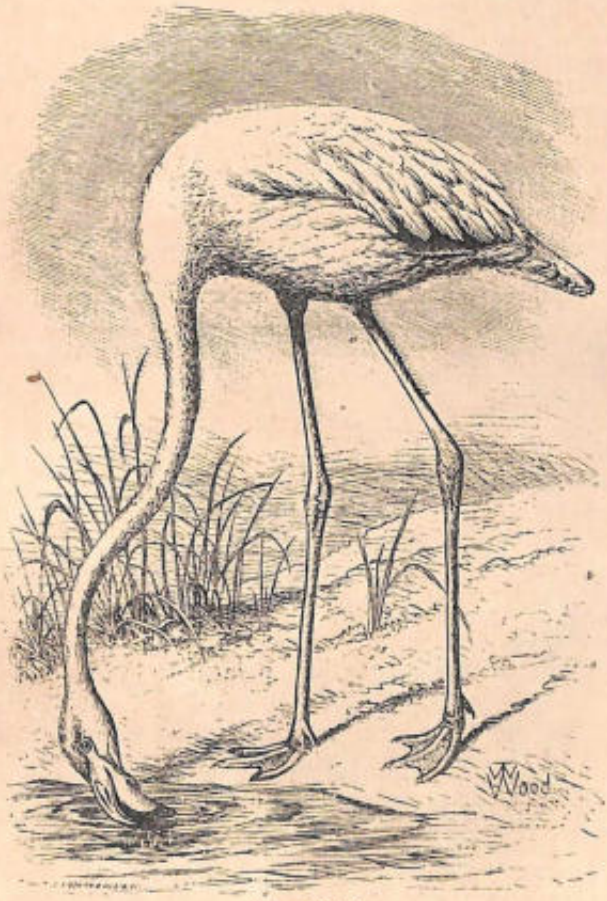
other zoological works issued by the same enterprising publishers, the manual before us is based upon a series of articles written by its author for the ninth edition of the "Encyclopædia Britannica"; but we believe, the new matter is in this instance still greater than

in the case of the other works. The present work differs, moreover, essentially from the others of the same series in that, as indicated by its title, it is in dictionary form. In deciding to treat his subject from the alphabetical rather than from the taxonomic standpoint, the learned professor, to use his own words, says that this plan has been adopted "because I entertain grave doubt of the validity of any systematic arrangement as yet put forth, some of the later attempts being in my opinion among the most fallacious, and a great deal worse than those they are intended to supersede. That in a few directions an approach to improvement has been made is not to be



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denied; but how far that approach goes is uncertain. I only see that mistakes are easily made, and I have no wish to mislead others by an assertion of knowledge which I know no one to possess." These are strong words, and we are glad to see them coming from an ornithologist of the long standing and well-deserved reputation of Professor Newton; more especially since we have had occasion elsewhere in this Journal to raise our own feeble protest against the succession of crude and ill-matured schemes of avian classification with which the ornithological world is flooded. Admitting, then, the wisdom of the author's choice in selecting the alphabetical form,

we have no hesitation in saying that the plan and execution of the work are in every way worthy of the high reputation of its author; while the excellence of the illustrations, and the careful manner in which a complicated typography has been executed, reflect credit alike on author and publishers. In calling in the aid of such a master of avian anatomy and morphology as Dr. Gadow, for articles coming within his special province, the author shows that he does not pretend to omniscience even in his particular branch of zoology, and thus affords an example which might be followed with advantage by others. The other writers mentioned on the title-page have likewise contributed articles on certain branches of ornithology with which they are more especially acquainted; and, so far as this instalment permits of forming a judgment, the work thus seems as complete and authoritative as it could well be made. That its style, although necessarily condensed, is terse and elegant, all acquainted with the author's previous writings stand in no need of being informed; while its freedom from typographical errors affords a refreshing contrast to some works that have recently come under our notice. Indeed, we have hitherto detected but a single misprint (p. 281, line 2 from bottom), and that is the fault of one of his contributors rather than of the author himself.

Of the illustrations, two of which we are enabled to give as samples, some are entirely new, others are reduced from figures previously published, while others again have been borrowed directly from earlier ornithological writers; and we are glad to see Swainson's exquisite figures of beaks and heads once more brought prominently before the public.

In our limited space it would be impossible to give a detailed notice of any of the articles; and, indeed, if such space were available, we should be puzzled to know which to select for special commendation. We may mention, however, that each bird, or genus of birds, is generally described under its vernacular name—whether this be English or foreign—although cross references will be found under the scientific titles of the genera. Among the articles that have specially interested us, are those on the cassowary and emeu, which are admirable samples of what a brief semi-popular description of a bird and its habits should be; while the article headed "Extermination" cannot fail to arouse a general, if somewhat mournful, interest. We may, however, be permitted to ask the author why he states under the head of Emeu that this "is the only form of Ratite bird which naturally takes to the water, and examples have been seen voluntarily swimming a wide river." We do not for one moment venture to say that the statement may not be perfectly correct, but at the same time we should like to know what reason the author has for discrediting the following account of the habits of the Rhea (or Ostrich as it is there called) given in the "Voyage of the 'Beagle.'" Darwin there writes that "it is not generally known that ostriches take readily to the water. Mr. King informs me that at the Bay of San Blas and at Port Valdes, in Patagonia, he saw these birds swimming several times from island to island. They ran into the water both when driven down to a point, and likewise of their own accord when not frightened; the distance crossed was about two hundred yards. When swimming, very little of their bodies appear above water; their necks are extended a little forward, and their progress is slow. On two occasions I saw some ostriches swimming across the Santa Cruz river, where its course was about four hundred yards wide, and

the stream rapid." Surely the statement of two independent observers requires refutation before it is authoritatively asserted that emeus are the only Ratites that naturally take to the water.

R. L.

Dr. H. G. Bronn's Klassungen und Ordnungen des Thier-reichs.—Aves. By Hans Gadow, Ph.D. IV. Abtheilung. 42 u. 43 lieferung.

The anatomical portion of this most important work was completed some few months since, and we have now before us the first two numbers of the systematic part. Veritably they are children of promise, and, unless we are much mistaken, the sum of Dr. Gadow's labours will be two invaluable volumes, each the perfect complement of the other, beautifully symmetrical, nicely balanced.

In these two parts Dr. Gadow presents us with a pithy sketch, which practically embraces all that is worth knowing in the history of Systematic Ornithology. Beginning with an "Historical Survey," he tells us that only those systems have been noticed which have either advanced—or hindered—our knowledge, as well, also, those which, although possessing practically no influence to-day, yet are of interest, inasmuch as they serve by reflected light to illuminate preceding systems. This is surely a wise course, inasmuch as it will enable us to appreciate much that would otherwise of necessity have remained more or less obscure.

After briefly referring to the three great epochs in the history of avian classification, so ably demonstrated by Dr. Sharpe in his "Review of Recent Attempts to Classify Birds," the author has a few words to say anent the errors of yesterday, and the dangers of employing one particular character, after the fashion of patent medicines, to remedy all evils and solve all difficulties. Next follow some thirty-four examples of the most important systems of classification, beginning with that of Linnæus in 1735, and ending with Dr. Sharpe's in 1891. In every case we have appended a brief yet all-sufficient summary, giving the chief points of interest or importance. Of these the author has selected for special mention some dozen examples, Huxley, Garrod, Sclater, and Newton being amongst the number.

The classification of Linnæus is shown to have been based upon the work of our countrymen Willoughby and Ray, of whom we are justly proud. Be it noted that Dr. Gadow here awards them full credit for their labours, considering them the founders of "Systematic Ornithology." To Merrem belongs the honour of having founded the sub-classes Ratitæ and Carinatæ, which have received recognition in every system of importance up till now. Nitzsch, with his researches in the Carotias, Osteology and Pterylography, and Müller on the organ of voice in the Passeres, have placed us under an everlasting debt. With Huxley's classification, the author tells us, began a new epoch, and, indeed, his work is too well known to need comment here. To Garrod, Dr. Gadow points out, we owe much. The Homalogonatous and Anomalogonatous arrangement of the muscles of the leg, the Holorhinal and Schizorhinal condition of the nostrils, the patagial muscles of the wing, and the syringeal muscles, are all points upon which this indefatigable worker enriched our knowledge. Garrod attacked the vexed problems of Ornithological Classification with intense zeal and a very considerable amount of success, but, as we are remined, not entirely without error. Fürbringer's work is

warmly praised, and, as the author points out, no ornithologist can proceed far without consulting his ponderous tomes. Dr. Sharpe's work, previously referred to, is, we think, deservedly considered an "important production," as all who have occasion to dabble in the mysteries of the complicated subject will allow.

As before stated, Dr. Sharpe's work is the last on the list—to our surprise, for we expected to find at least a reference to a most suggestive, if only tentative, scheme by Dr. Gadow himself, which appeared in the *Proceedings* of the Zoological Society for March, 1892; although it is more than probable that this will not be adopted in its entirety in the present work, yet we venture to think it should have been included; doubtless, however, the author has good reasons for its omission.

We come now to what might be called a diary of remarkable events in Systematic Ornithology—a sort of developmental history of classification. This retrospect (Rückblick) is arranged in a more or less tabular form, and is designed to show the more important changes of position which birds of doubtful affinity have been subjected to. To take an example at haphazard we quote the following: "Müller, 1847, *Cypelhus* removed from the Oscines, and placed with *Caprimulgus*." (First) recognition of the order *Oscines* ("Klärung der Oscines"). Some twenty-nine more or less well-known workers are quoted in this connection, beginning with Mohring in 1752, ending with Fürbringer in 1888.

Under the next heading, "Taxonomical Outlines," the author deals with the general principles of Taxonomy, and discusses in turn genealogical trees, that phenomenally arbitrary system the "Key," and the various organs used by systematists as "Characters" wherewith to differentiate various groups. Finally, we have a very valuable table of "Characters" used in classification, which are expressed chiefly by signs, for the sake of brevity and clearness. Seeing that these signs are carefully described in the text immediately preceding, there should be no difficulty in assimilating them. We would suggest, however, that eventually they should be published in one large sheet for use separately.

Dr. Gadow is to be congratulated on the decided "hit" he has made, and we are sure that we do but echo the sentiments of other workers in this field when we say that we are anxiously looking forward to the appearance of parts 44 and 45. Nevertheless, we will try to keep in view that very useful proverb, "Eile mit weile,"—a motto which Dr. Gadow wisely adopts. W. P. P.

Manual of Bacteriology for Practitioners and Students. By Dr. S. L. Schenk, Professor in the University of Vienna. Translated (with an appendix) by W. R. Dawson, B.A., M.D. (Dublin). 8vo. Pp. 310. London: Longmans and Co., 1893. Price 10s. nett.

This, the most recent text-book in English upon the new science, appears to be well translated and to merit a longer notice than is sometimes allowed to translations; for Dr. Dawson has added a valuable and timely appendix upon the mode of vaccination for cholera, a disease which has already paid us a visit this year—a visit which was, fortunately, brief; he has also included an account of Professor Marshall Ward's interesting observations on the influence of light upon bacteria, which have only just been published in the *Proceedings* of the Royal Society.

The best way of coping with many pathogenic bacteria appears to be to let daylight in upon them; it has been stated that a few minutes of direct sunlight are enough to kill the deadly bacillus of consumption, and that even diffused daylight is fatal in a longer or shorter time according to its intensity. Unfortunately, this mode of treatment is hardly applicable when the bacilli have got into the body; but they might be lessened in numbers, and the danger of infection thus diminished, by thorough ventilation of suspected localities. An open drain is neither pleasant to the eye nor agreeable to another sense which largely comes into play where bacteria abound, but it is from many points of view more healthy than a close drain, where, hidden from the sun's rays, the fellest varieties of bacteria can multiply unchecked. For practical purposes, too, any mode of dealing with typhoid is very important; it is an exceedingly common disease, and there must be few people who have not lost some friend or relative by its means; the germs of this complaint are killed by sunlight of four to seven hours' duration.

To those who have not followed recent bacteriological work, some of the figures given in this book will be surprising. One knew ever so many years ago that the large spirilla had a flagellum at each end by which they moved; now it is known that most kinds—even some of the most minute—are provided with a bunch of these vibratile processes; and this accounts, perhaps, for the rapid progress through an organ, or a system, as the case may be, of some infectious complaint; the typhoid bacilli from the great number of flagella which spring here and there from the organism are called "spider cells." As is well known, many different kinds of bacilli—alike enough on a superficial inspection—can be distinguished by their different reaction to staining fluids; and this important means of detecting a given bacillus is impressed upon the mind of the student in the work before us by the use of differently coloured figures in the text. We have, indeed, nothing but praise for the illustrations, which are not only good but abundant.

The word "Bacteriology" is used in the book before us in a sense rather wider than that usually applied to it, and the author says something about other kinds of unicellular parasites. There is a growing tendency to ascribe cancer to a Gregarine; and the important researches of Drs. Rüffer and Walker are justly quoted (by the translator) in this connection—researches which have since been confirmed upon the Continent. Gregarines are such abundant parasites in the lower animals (insects, earthworms, etc.), where their existence appears to be attended with such slight inconvenience to their hosts, that it is, perhaps, remarkable how extremely dangerous they are in the form of cancer in the human species. F. E. B.

ON ENGLISH LAGOONS: Being an Account of the Voyage of Two Amateur Wherrymen on the Norfolk and Suffolk Rivers and Broads. By. P. H. Emerson. 8vo. Pp. 298. London: David Nutt, 1893. Price 7s. 6d.

WE took up this volume with a feeling by no means favourable to the author, for of late years we have been overwhelmed with books on the Norfolk Broads. Mr. Emerson, however, has a far better claim than most to write on the subject, for he has lived the whole year round on the broads, and can describe them as they appear in a

severe winter as well as during the ordinary season for visitors. One fault only we have to find. His pictures of the broads, though striking enough to those who already know the district, will often scarcely be intelligible to strangers, for he has a habit of leaving out essential parts of the description—parts so familiar to the author and to the natives that they have ceased to be remembered.

Mr. Emerson is a good field naturalist, and his book is full of interesting notes on the habits of the birds, though there is nothing strikingly novel. Among the notes on the fish, there is a very curious one on a large take of eels at Geldeston Lock in December, after a very severe frost, and with the river full of ice and snow. As eels usually hybernate in winter, this was most unexpected, and we are glad that a naturalist like Mr. Emerson was on the spot and can vouch for the fact. Can this sudden appearance of eels have been connected with the formation of anchor ice, which disturbed the eels in the muddy bottom in which they had been lying? The formation of anchor ice is not a common phenomenon in Norfolk; but the severe frost with clear sky which the author recorded a few days before ought to have caused its appearance. Mr. Emerson makes also a good many remarks on the manners and customs of the natives. He seems, however, to be much more able to appreciate the good points of a fish or of a bird than of a human being; our own experience has not led us to consider the Norfolk people so utterly boorish as he pictures them.

Manuel de Géographie Botanique. Par Oscar Drude, traduit par Georges Poirault et revu et augmenté par l'auteur. Livraison I. Paris: P. Klincksieck, 1893. Price fr. 1.25.

There are so many who find French easier reading than German that a translation in the former language of Drude's important book will be very generally welcomed. The value of the present work is, moreover, enhanced by the fact that it is reviewed and augmented by the author so that it should be in every way up to date, a matter of importance when dealing with geographical distribution, a subject in which new series of observations on plant biology, or the better understanding of the flora of a region and its relations to neighbouring ones, may necessitate a reconsideration of hitherto accepted theories.

According to the advertisement, the complete work will form a volume of about 500 pages (8vo), and will include 4 coloured maps and 3 figures in the text. It will be published in 12 or 13 parts, each containing two or three sheets of text. We are glad to note that the parts will follow each other rapidly. The price of each part is 1 fr. 25, the subscription to the complete work, payable in advance, 15 francs.

Part I. contains 48 pages, and is clearly printed on good paper. It comprises an introductory definition of the subject, with a brief account of its relation to geology and physical geography, and also a portion of the first part of the book dealing with the influence of external agents on the plant organism. Under geographical agents are considered the influence of duration and intensity of light, of heat, cold and dryness; acclimatisation is briefly disposed of, and a section on phenology follows. Next, under topographical agents, the influence of the soil is discussed. We shall await with interest the succeeding parts of what promises to be a valuable work.

THE GENUS MASDEVALLIA. Issued by the Marquess of Lothian. Plates and descriptions by Miss Florence H. Woolward. Part IV. London: R. H. Porter, 1893. Price £1 10s.

MASDEVALLIAS form a favourite genus with orchid growers, and are, besides, of great interest from a botanical point of view. They are confined to tropical America, inhabiting chiefly the damp higher regions of the Andes, but ranging up into Central America and southwards as far as Rio Janeiro. Though, speaking systematically, a good genus, it includes species widely different in appearance. Contrast, for instance, the brilliant *Masdevallia Harryana*, one of the most common in cultivation, with its broad-spreading perianth limbs, with the more sombre-coloured *M. coriacea* and it allies, where the perianth takes the form of an open fleshy cup; or, on the other hand, with the gorgeous *M. bella*, where the lip, no longer inconspicuous or hid within the tube, forms in its shell-like beauty a striking characteristic of the flower. The range of colour is as wide as that of form. Now a plain scarlet or orange, or, alas! even magenta, an almost pure yellow, or, but rarely, white; now, on the other hand, a rich blending of citron and crimson lake as a background to the pure white lip. To all of these Miss Woolward does ample justice in the beautiful plates which accompany and interpret the descriptions of every species.

Much of the material for the work is from Lord Lothian's own collection of orchids at Newbattle Abbey; but Miss Woolward has also had the advantage of specimens, both living and dried, from orchid growers and in herbaria, for purposes of comparison and description. The carefully-drawn floral dissections on each plate will be invaluable to the botanist. Consul Lehmann (German Consul in the Republic of Colombia) has supplied interesting notes on the habitat of those species with which he is acquainted in their native homes. The present number also includes a species from Brazil, which has not yet reached Europe, but drawings and descriptions of which have been supplied by Professor Rodriguez, the Director of the Gardens at Rio Janeiro. In the same issue a new species is described, a striking plant near *M. elephanticeps*, and apparently imported by Mr. Bull mixed with the latter plant, as it was purchased from him under that name.

Part IV., like its three predecessors, contains ten species, bringing the whole number up to forty, and we understand that Part V. is well on the way.

ILLUSTRATIONS OF THE TYPICAL SPECIMENS OF LEPIDOPTERA HETEROCERA IN THE COLLECTION OF THE BRITISH MUSEUM. Part IX. The Macrolepidoptera Heterocera of Ceylon. By George Francis Hampson. 4to. Pp. vi., 182, pls. clvii.–clxxvi. London: Published by the Trustees of the British Museum, 1893.

THIS ninth part of the British Museum Catalogue is devoted to the Macrolepidoptera Heterocera of Ceylon, and consists of a "General Systematic List," followed by "descriptions of species figured," containing a formidable number of new genera. There is an interesting illustration of protective mimicry shown by the caterpillar of *Cornibœna biplagiata*, in which the sides of each somite are produced into fleshy processes, upon which the larva fastens small pieces of withered leaves and sticks as a disguise. It may be as well to mention that this information occurs on p. 145, for as is usual in this series of volumes there is no index, and one has to hunt through page after page of text to find the details required.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Miss Maria M. Ogilvie, B.Sc., well-known for her researches in the geology of the Tyrolese Alps, has been promoted to the degree of D.Sc. in the University of London.

Dr. William H. Howell, of the Harvard Medical School, has been appointed Professor of Physiology in Johns Hopkins University; and the vacant chair of Anatomy in the same University has been accepted by Dr. Franklin P. Mall, late of Chicago.

Dr. Spyridon Miliarakis, curator at the Athens Botanical Museum, has been appointed Professor of Botany at the University; and Dr. Ed. Fischer has been appointed extraordinary Professor of Botany at the University of Berne.

Dr. U. Dammer and Dr. P. Hennings have been appointed assistant-curator and curator respectively in the Berlin Botanic Garden, and Dr. M. Gurke a curator in the Botanic Museum.

Mr. F. W. Gamble, B.Sc., has been appointed Assistant Lecturer and Demonstrator of Zoology in the Owens College, Manchester. Mr. H. B. Pollard, M.A. (Oxon), has been elected to a Berkeley Research Fellowship in Zoology in the same College.

Dr. Giessler, assistant at the Botanical Institute of the Jena University, has been appointed assistant at the Botanical Museum and Garden of the Göttingen University in the place of Dr. Hallier, who, as noticed in a former number of Natural Science, has become an assistant at the Buitenzorg Garden in Java. At the same University (Göttingen), Dr. Dreyer, from St. Galle, takes the place of Dr. Alfred Koch as assistant in the Institute of Plant Physiology.

We regret to announce the retirement, on account of failing health, of Sir J. William Dawson from the Principalship of McGill College, Montreal, an office he has held for nearly forty years. While relieved of administrative duties, Sir William hopes to be able to pursue with greater success his long-continued researches in geology and palæontology.

The Royal Saxon Mining Academy in Freiberg has had attached to it since 1767 a "Mineralien-Niederlage," or commercial branch for the supply of specimens. This is situated in the main building of the academy, and as a public office has been under the supervision of the director. We have received what is to be the first price list issued, written in remarkably good English, and showing that the mineral productions of Saxony can be supplied at most reasonable terms. Both A. G. Werner and A. Breithaupt have been the managers of this concern. Special stress is laid on the necessity of addressing letters Freiberg, Saxony.

The Essex County Council Technical Instruction Committee has issued a prospectus of lectures and classes in Chemistry and Biology. The course of instruction is graduated in an interesting manner, there being first "pioneer lectures" on subjects of every-day life, then short introductory series of a popular character, and subsequently systematic and practical courses. There are also "County Normal Classes" for the instruction of advanced students as teachers. Mr. David Houston is Staff-Instructor of Biology, with Mr. Wilfred Mark Webb as Demonstrator. Mr. J. T. Cunningham, of the Marine Biological Laboratory, Plymouth, has also arranged to commence lectures in October on the Natural History of marketable sea-fishes and oysters.

We have received the first part of vol. iii. (new series) of the *Journal of the Marine Biological Association of the United Kingdom*. In the report of the Director, Mr. Edward G. Bles, reference is made to a change in the regulations by which the shortest term for the renting of a table at the Plymouth Laboratory is reduced from one month to one week. This should induce investigators to take any opportunity which may occur at any time of the year to collect seasonable material, or to study living animals and plants available for the time being. Among the papers there are two important contributions to the fauna of the English Channel, one on Nemertines, by T. H. Riches, and another on Turbellarians, by F. W. Gamble. Mr. J. T. Cunningham discusses the "Immature Fish Question," and the colouration of the skin in flat-fishes. Dr. W. B. Benham gives a beautifully illustrated description of the post-larval stage of *Arenicola marina*; and there are two reports of economic interest by E. W. L. Holt and W. L. Calderwood.

The annual report of the American Museum of Natural History, New York, for 1892, has lately been issued. The opening of the Museum on Sunday afternoons appears to have proved a success; and the popular lectures, arranged with the aid of the professors of Columbia College, are highly appreciated by the public.

The Department of Botany of the British Museum has purchased the great collection of Diatoms made by Mr. Julien Deby, estimated to consist of about 30,000 slides. Although the Museum previously possessed a magnificent series, including Greville's, William Smith's, Gregory's, O'Meara's, Ralf's collections, etc., there is singularly little duplicate material in the Deby Collection, which was, before acquisition, the only rival of the Museum collection in importance. It has been handed over in excellent order, fully indexed and accessible to students.

The annual meeting of the Palæontographical Society was held on June 23, when a satisfactory report of progress was presented. Only two non-members of Council, however, attended. The Right Hon. Professor Huxley was elected President in succession to the late Sir Richard Owen.

The question of the admission of Women as Fellows was decided in the negative by the Royal Geographical Society, at a special general meeting held on July 3. The voting was 172 against, 158 for. The result is disappointing, but is what might have been expected under the circumstances.

Next year the Geological Society of London publishes the fiftieth volume of its Quarterly Journal. It has been decided to mark the event by preparing an index to the whole series, which will probably be published early in 1895.

The First Report of the Scientific Society of University College of Wales, for session 1892-93, has just appeared. It is a modest little pamphlet of 12 pp. and contains besides business matters a tentative list of the Flora and Fauna of the district around Aberystwith. Since the formation of the Society, Oct. 20, 1892, the members have made eight excursions, chiefly to places of geological interest, their enthusiasm even taking them afield in November and December.

The thirteenth report of the Manchester Microscopical Society is prefaced by an excellent portrait of the President, Professor Milnes Marshall. There are two readable presidential addresses, the one on "Death," the other on "Recent Embryological Investigations"; and Professor Weiss briefly discusses Symbiosis in plants. Mr. P. Cameron contributes part ii. of his description of the galls of Mid-Cheshire, with two plates; and Messrs. W. Moss and F. Paulden give an illustrated account of the reproductive organs of *Bulimus acutus*.

The Guernsey Society of Natural Science and Local Research has completed the first decade of its existence, and the new part of its *Report and Transactions* contains several valuable papers. Mr. E. D. Marquand supplements his list of the flowering plants of Guernsey by an account of the mosses, hepaticæ, and lichens, and it is noteworthy how many species common in Devon and Cornwall appear to be absent in this island. Mr. W. A. Luff publishes a list of the Hemiptera-Homoptera, the first of the kind relating to any of the Channel Islands. Mr. G. Derrick describes the Guernsey clays, and in another geological paper Mr. A. Collenette considers that the raised beaches and associated phenomena prove Guernsey to have been completely submerged at a recent period. A brief account of the rocks of Alderney is also given by Mr. C. G. De La Mare.

Circulars dated June 10 inform us that "The World's Congress of Geologists will meet in Chicago" immediately after the meeting of the American Association in Madison. The Geological Society of America will also hold its fifth summer meeting on the 15th and 16th of August at Madison. The Excursions proposed for the Geological Society of America will comprise the Lake Superior Region, the Dells of the Wisconsin, and the driftless area in the vicinity of Madison. Messrs. Wadsworth, Van Hise, Chamberlin, and Salisbury will conduct the Excursions.

The Geologists' Association of London called a Special General Meeting on July 7 and passed some important alterations in the rules. It was decided that the names of those proposed as members should, after being read out from the chair, be declared duly elected unless a ballot was demanded. It is hoped by this means to avoid much unpleasantness and expenditure of time, while opportunity is afforded of an appeal to the ballot, if any one of those proposed for election is considered ineligible. Another important change is the increase of the commutation fee or life subscription to seven pounds ten shillings, being fifteen years' purchase. This is in accordance with the practice of other Societies, it having been found that the low commutation fee has been a loss to the Association for several years past. The third important change is an expulsion clause. We trust the Association has not been compelled to adopt this in self-defence, but rather as a terror to possible evil-doers. The clause appears to us to be very cautiously worded, and will no doubt prove valuable if required. Ugly as these clauses are, they are necessary for the general welfare of the members.

The 42nd meeting of the American Association for the Advancement of Science will be held in August at Madison, Wisconsin, under the presidency of William Harkness. The president of the Geological Section will be Mr. C. D. Walcott,

who discourses on "Geologic time as indicated by the Sedimentary Rocks of North America"; of the Zoological Section, Professor H. F. Osborn, whose address is to be devoted to "The Rise of the Mammalia"; of Botany, Professor C. E. Bessey, who speaks on "Evolution and Classification." Dr. J. O. Dorsey, the president of the Anthropological Section, will read a paper on "The Biloxi Indians of Louisiana."

THE next Congress of the French Association for the Advancement of Science will be held at Besançon from August 3 to 10. M. A. Magnin, professor at the Besançon Faculté des Sciences, will preside over the botanical section, and he suggests as a subject for discussion the question, already touched on, but in an incomplete manner, of the formation and conservation of botanical collections, also the relations of the Jura flora to the vegetation of the Alps.

THE Selborne Society celebrated the centenary of the death of Gilbert White, June 26, by a visit to the Hampshire village, immortalised as the scene of the unassuming naturalist's labours. Gilbert's father was squire of Selborne at the time of his son's birth in 1720. Gilbert, the eldest of several sons, went to Oxford, where he graduated in 1744, gaining a Fellowship at Oriel. He lived some time at the University, serving for a year as Senior Proctor. In 1753 he took a curacy, but two years later we find him at his father's house at Selborne, where he stayed till 1758, when his father died. He then did duty in Faringdon, a neighbouring parish, returning in 1784 to Selborne, where he remained as curate till his death in 1793. The "Natural History of Selborne" appeared in 1789, only four years before his death.

WE have received some circulars from Mr. Joseph Coe, of 33 All Saints' Street, Hull, from which we learn that an excursion was conducted on June 12, 1893, to Mablethorpe, by some of the more energetic naturalists of Lincolnshire, for the double purpose of investigating the natural history and geology of the district, and considering the question of founding a "Lincolnshire Naturalists' Union." Professor Miall, Rev. Canon Fowler, Messrs. F. M. Burton, Denison Roebuck, John Cordeaux, and others were present, Professor Miall taking the chair; a favourable decision was obtained, and those present at the excursion were formed into a temporary committee to draw up rules and elect officers at Lincoln on July 1. At the meeting of this committee the necessary formalities were proceeded with, and the first officers elected. Mr. Cordeaux is to be the first President; Messrs. W. F. Baber and Joseph Coe, Honorary Secretaries; while the sectional presidents are Mr. G. H. Caton-Haigh (Vertebrate Zoology); Rev. C. W. Whistler (Conchology); Rev. Canon Fowler (Entomology); Rev. W. Fowler (Botany); Mr. F. M. Burton (Geology) Mr. R. J. Ward (Micro-zoology and Micro-botany). Field meetings will be held twice a year, once in the northern and once in the southern division of the county; the subscription is 5s., but those subscribing 10s. 6d. will receive the proposed publications of the society.

THE Museums' Association, which formed the subject of our first comment last month, met, by permission of the Zoological Society, at 3 Hanover Square on July 3. The retiring President, Professor Boyd Dawkins, was unavoidably absent, and the chair was taken by the new President, Professor Sir William Flower, who delivered an opening address. On Tuesday, July 4, Dr. Jonathan Hutchinson opened the proceedings by a paper on "Educational Museums," which was followed by an interesting discussion. Mr. Ruskin's views on Museums were expounded by Mr. W. White, and Mr. Newstead read some notes "On the use of Boric Acid as a preservative for Bird-skins." Visits were paid in the afternoon to the Natural History Museum, and the South Kensington Museum. On the Wednesday, Dr. Sclater discoursed "On the Typical Forms of Vertebrated Animals suitable for Exhibition,"

Mr. Flatnauer "On the Arrangement of Insect Collections in Museums," and Mr. Newstead "On the Preparation of Coccidæ for Museums." The day concluded with a visit to the British Museum, and a Conversazione at the Royal College of Surgeons. The Royal Wedding on Thursday fortunately provided an excuse for open-air work, and the members visited the Zoological Gardens and Sir John Soane's Museum. Thursday and Friday were devoted to exhibitions of museum appliances, or visits to various smaller private collections, while Saturday was agreeably spent by a visit to Dr. Hutchinson's private museum at Haslemere, and a ramble over the Hind Head district.

The papers read at the meetings will be printed later on, but Sir William Flower's address is already to be found in *Nature* (July 6 and 13). He again emphasised the absurdity of the crowding of specimens, pointed out the value of good labelling, isolation of objects, suitable backgrounds, and necessity of space, and referred to the insignificant amount expended by Government on the development of museums; he also referred to the contemptible offers which had lately appeared in the newspapers for curators of local museums—a subject mentioned in our comments last month. After speaking on the general theory of museum arrangement, Sir William Flower gave a sketch of the museums at the Jardin des Plantes, Paris, at Vienna, and at Berlin, and noticed "the most original and theoretically perfect plan for a museum," explained to the British Association by General Pitt-Rivers in 1888, finally presenting a plan of his own, which contemplates a one-storied, top-lighted building, in which the front galleries would be devoted to the public exhibitions, a second series of galleries would contain the reserve collections, while a third set of rooms would be occupied by the officers and students. In such a place as this the absurdity of having to walk a quarter of a mile and climb several staircases to compare a fossil plant with a recent plant, or a mammoth with an elephant, as one has now to do in the British Museum (Natural History), could not exist, and the officer in charge would be in direct communication with his reserve and exhibited series, instead of being, as in many cases, far removed. Another point brought out in this plan is the necessity for a lecture theatre, and an arrangement in construction of the officers' workrooms by which isolation from the main building is effected so as to permit those who desire to do so to remain for work instead of having to leave at an appointed hour. We hope to hear more of this plan, for many students would heartily welcome some radical changes in the present arrangement and working facilities of most public museums.

OBITUARY.

FRANCIS POLKINGHORNE PASCOE.

BORN SEPTEMBER 1, 1813 — DIED JUNE 20, 1893.

A VETERAN naturalist, well-known for his researches in Entomology, has passed away in the person of Mr. F. P. Pascoe. Born at Penzance eighty years ago, Mr. Pascoe was trained for the medical profession, and was always imbued with a taste for Natural History and travel. In 1835 he became M.R.C.S., and shortly afterwards entered the Navy as assistant surgeon, visiting Australia, New Zealand, the West Indies and the Mediterranean. In 1843 he retired from the service, married, and resided in Cornwall until the death of his wife in 1851. Subsequently Mr. Pascoe removed to London, when he gradually formed the great entomological collection now being acquired by the trustees of the British Museum. His first original paper related to a botanical subject, but all subsequent contributions dealt with his chosen field of research in Entomology, certain groups of Coleoptera. The results of his studies are to be found in the publications of the Linnean and Entomological Societies, and in numerous scientific journals, during the last thirty or forty years. His monographs of the Malayan and Australian longicorn beetles, in which the collections of Dr. A. R. Wallace are described, are among the most important of these. Although a believer in evolution, he was, with so many of the older systematists, opposed to the theory of Natural Selection. Two books, the latter of which appeared only three years ago, were written by him attacking the Darwinian position.

HENRY HUGH HIGGINS.

BORN JANUARY 28, 1815.—DIED JULY 2, 1893.

THE REV. H. H. HIGGINS, whose death is chronicled by the daily Press as having occurred while writing a paper for the Museums Association Meeting, recently held in London, was born at Turvey Abbey, Bedfordshire. Entering Corpus Christi College, Cambridge, in 1833, he obtained his B.A. in 1836, was admitted to Priest's Orders in 1839, and became a curate at Wolverhampton and at Shrewsbury. In 1842 Mr. Higgins went to Liverpool, and it is in connection with the Liverpool Free Museum that his name has

become famous. He was chairman of the Museum sub-committee of management for many years, and devoted much of his time to the arrangement of the collection. Mr. Higgins was the author of "Notes of a Field Naturalist in the Western Tropics," 1877, a "Synopsis of the Invertebrata," "Museums Memorandum Book," "Museum Talk," and several other pamphlets of a practical value on similar subjects. In 1880 he rendered special service to the Liverpool Museum by securing the duplicate Cirripedes collected by Darwin during the voyage of the "Beagle." He founded the Liverpool Naturalists' Field Club in 1860, and had been president of the Literary and Philosophical and the Microscopical Societies of Liverpool. The fine series of Coal-measure fossils from the railway cutting at Ravenhead near St. Helen's, now in the Liverpool Museum, was collected by himself. An excellent portrait of Mr. Higgins appeared in *Research* for July 1, 1888, and to the article which accompanied it, by Mr. O. W. Jeffs, we are indebted for most of this information.

THE death is announced of Dr. Henri Viallanes, Director of the Zoological Station of Arcachon. Dr. Viallanes, who was only thirty-six years of age, had paid considerable attention to the nervous system of the Arthropoda, and his early loss is much to be deplored.

WE also regret to record the death of Dr. John Rae, the Arctic explorer, and of Mr. James W. Davis, the geologist and archæologist, one of our valued contributors. Obituary notices will appear next month.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 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.*

All Advertisements *to be forwarded to the sole agents,* John Haddon & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

ADDENDUM.

On p. 59, line 38, after the words "Dr. Nansen on glaciation" add the words "are unsound."

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 19. Vol III. SEPTEMBER. 1893.

NOTES AND COMMENTS.

The British Association.

BRITISH Science is again preparing for its annual festival, which will be inaugurated by Professor Burdon Sanderson, at Nottingham, on the evening of September 13. There are, as usual, many malcontents who disparagingly remark that the association has "had its day"; and we know a number of eminent biologists and geologists who have declared that this year their vacation shall not be interrupted by attendance at committees and in scientific meeting rooms. There is, however, every prospect that the forthcoming meeting will be no less a success than most of its predecessors; and the organising committees have done well in arranging a goodly number of interesting discussions. We note, moreover, that the President of the section for Geology (Mr. Teall) announces his Address to be not a mere general summary of recent progress, but to deal with no less fundamental a subject than the Doctrine of Uniformitarianism, as viewed from his own special domain of research.

If these discussions of wide themes were more generally encouraged than they are, and if the innumerable, desultory, technical papers, often only of local interest, were absolutely refused, the Sectional Meetings would be much more appreciated than is the case at present, and the Report would be relieved of a burden of brief "abstracts" that are of little use to anyone. However much the enthusiast may sneer at the "picnic element" in the gathering, it can never be reduced until narrow specialists are prevented from inflicting the details of their work upon the Sections. The British Association Meeting is a great reunion of amateurs and patrons of Science rather than of professed devotees, and it forms one of the most important sources of revenue upon which the latter can depend for assistance in their investigations. The programme ought, therefore, to be adapted,

as far as possible, to a general scientific assembly, and there is then no fear that this great meeting will lose any of its long-sustained interest and importance in the intellectual life of the nation.

TRILOBITES.

CONSIDERING the abundance of the familiar Palæozoic fossils named Trilobites, it is singular how little is known of their appendages. Twelve years ago the researches of Mr. C. D. Walcott showed that the ventral side of the body was covered with a hardened arch over each segment bearing a pair of limbs, while the head was provided with four pairs of appendages, acting as jaws at the base, as in the modern King Crabs.

More than 20 years ago, Dr. Henry Woodward also discovered a kind of palpus attached to an inferior head-plate; but conditions of fossilisation more satisfactory than any of those under which trilobites have hitherto been met with can alone permit much advance is our knowledge of the subject.

A stratum in the Hudsor River shales, near Rome, New York State, it is interesting to note, has yielded materials for the first step in this advance. The discovery of a number of specimens of

HEAD OF TRILOBITE SHOWING ANTENNÆ.

Triarthrus becki by Mr. W. S. Valiant, enables Mr. W. D. Matthew to contribute an important paper on the subject to the current issue of the *Amer. Journ. Sci.* (ser. 3, vol. xlvi., pp. 121–125, pl. i.); and it now appears that not only had the trilobites the appendages already observed, but also a well-developed pair of antennæ. We reproduce a figure of the best specimen above. The antennæ are composed of a great number of joints, each of which is conical, about half as long as wide, and smallest at the base. They seem to have been of a structure less firm and thick than the substance of the carapace; and just over the spot where they come out, the anterior margin of the

head-shield is arched slightly upwards, apparently to afford space for their play to and fro. On the side of the same head there are also some impressions that may indicate the gills, though it is equally probable they are merely hairs on a narrow limb, serving as mouth-organs.

Trilobites are also discussed in the same journal (pp. 142–147, pl. ii.) by Dr. C. E. Beecher, who follows the work of Barrande in attempting to interpret the larvæ. Here, again, the fine character of the sediments in which the fossils are embedded (Lower Helderberg, near Albany, N.Y.) favours the investigation. Moreover, the facts are of especial interest in this case, because all the trilobites belong to highly ornamented and specialised genera. Some beautiful figures are given with detailed descriptions, and Dr. Beecher concludes by trying to generalise in reference to the normal life-history of a trilobite. The results are too technical for recapitulation here, but they show that before long zoologists will be able much more satisfactorily than hitherto to discuss the reference of these characteristic Palæozoic organisms to Crustacea or Arachnids.

Fishes at High Temperatures.

Dr. Lawrence Hamilton forwards some interesting statistics he has collected in reference to the existence of fishes in water of a high temperature. Some of the cases are very striking. Spallanzani, it appears, observed river carp living at a temperature of 106° F., and exhibiting no signs of uneasiness, though at 109° they began to struggle, and died at 116° F. Dr. John Davy (1835) showed that the Bonito had a temperature of 99°, while the water of the Mediterranean, in which it was, had only a temperature of 80°. Saussure stated that he found eels in the hot springs of Aise, in Savoy, at a temperature of 113° F. In 1882, Dr. Davy found that water at 85° F. killed trout by convulsions. A trout and a minnow were put in water at 70° at night, which by the next morning had sunk to 67°, when the trout was dead, though the minnow had not suffered. A salmon parr at 80° became convulsed and torpid, dying at 84°. Several fishes were deposited in water at 53° F.; the temperature of the water was gradually raised, and none showed signs of failing vitality till the thermometer rose to 82°, when the perch became prostrated, roach succumbed at 82½°, salmon at 83°, minnow at 85°, gudgeon at 85½°, dace at 86°, tench at 88°, and carp at 91°. Brandy restored all the fishes except the dace, which died.

In India, fishes at noon-day in their natural water remain in health at 92°; at 4 p.m., 86°; and at 6 p.m., 82°. Günther states that Cyprinodonts live in briny springs even at a temperature of 91° F.

Sir Emerson Tennent collected the following observations, which seem to require further proof or verification:—

In the hot springs of Ceylon, living carp, *Nuria thermoicus*, at

114° F.; members of the perch family, the *Apogon thermalis*, and the *Ambassis thermalis*, in water at 115° F.; a roach, *Leuciscus thermalis*, at 112° F.

In a hot spring at Pooree (in the province of Bengal), with the thermometer in the water standing at 112° F., carnivorous fishes have been discovered, which would indicate that these must have found and fed on living prey at the same high temperature. Further accounts, moreover, declare that in hot springs in Barbary, in North Africa, living fishes have been taken in water at 172°, while in Manila (one of the Philippine Islands) in water marking 187° F. While travelling in South America, Humboldt and Koupland stated that they saw fishes thrown up alive from a volcano in water at 210° F., but this is, of course, an absurdity which nowadays, it is to be hoped, no one will believe.

The Dwindling of Limestones.

In an article on the Dwindling and Disappearance of Limestones (*Quart. Journ. Geol. Soc.*, vol. xlix., pp. 372–384, pl. xviii., Aug., 1893), Mr. Frank Rutley discusses the question, which he has himself raised, of the possible dissolution of limestones that were formed during the earlier periods of the earth's history. That limestone is dissolved by the action of acidulated waters, and that caverns, "pipes," and other features are formed in the rocks, are facts familiar enough. The author, however, contends that if unlimited time be conceded, there seems no reason why very thick beds of limestone should not wholly disappear; at all events, he thinks that thin ones may easily do so, and thereby certain pages in the life-history of particular localities may be lost for ever. Unfortunately for his argument, the author is unable to bring forward any special evidence in support of it. The Durness Limestone of Cambrian age must be of considerable thickness, and yet it has survived many vicissitudes. The Carboniferous Limestone over large areas, though in places riddled with caverns, presents in mass its full thickness; it passes by alternations of limestone and shale into the beds above and below, while the masses of the *insoluble* strata above and below are more extensively wasted away. Statistics of the amount of solid matter carried away by rivers and streams prove how large an amount of limestone is dissolved, but, except in the case of thermal springs, the loss of material is not very deep-seated.

Attention is drawn to the nodular character of some limestones, and the author very properly points out that in some cases a bed of jointed limestone may become weathered into isolated nodules. He proposes to call these nodules *residual*, as distinct from concretions or segregation-nodules. These residual nodules occur, so far as we know, only where the strata come near the surface; but they serve to confirm one of the author's conclusions, namely, "That bands of limestone-nodules may, in certain cases, represent what were

originally *beds* of limestone." Under similar conditions, that is to say, near the surface, "bands or nodules of chert may represent all that remains of what were once beds of cherty limestone."

Whether the comparatively thin limestones in some of the older formations were originally much thicker, is a subject to which attention may profitably be directed.

Bibliography of Botany.

Mr. J. C. Bay, of the Missouri Botanical Garden, submits a scheme in the July issue of the *Botanical Gazette* for the production of a yearly bibliography of American botany. It will be on the lines of Just's well-known *Jahresbericht* which, Mr. Bay complains, pays little attention to American literature, and should give, (*a*) a list of papers and works in every department of botany, *absolutely* complete; (*b*) a review of each of these papers and works, short and perfectly *objective*. A third point is that, as the importance of such a work would be international, it should be published and sold separately and not assimilated by any report or periodical, as in the latter case it would be inaccessible to many botanists. The Smithsonian publications are referred to as so very expensive as hardly to be ever seen in private libraries in other countries. The publishing should therefore, as far as business management goes, be in the hands of a publisher.

The work should be undertaken by a committee among the members of which the editor should distribute the reprints of the papers received from botanical authors. A classification under fifteen heads is suggested, and Mr. Bay is willing to look after subjects falling under three of these. The whole work "could appear in July," *i.e.*, only six months after date; truly an admirable feature. We wish the scheme success. If a publisher can be found to do the thing well and satisfy himself on the score of finance, perhaps some English brother will be courageous enough to follow the example in our own country. At present, we have only Just, and of this the concluding part, *i.e.*, *the part containing the index* of the volume for 1890 bears the date 1893. The *Annals of Botany* started in 1887 with a record of current literature, but this ceased with the fourth volume, under the plea that space was too valuable! The Linnean Society might do it, but it would have to wake up.

The *Kew Bulletin of Miscellaneous Information* is beginning to justify its title in a novel and remarkable way. In the July number we have a list of the principal guests at a garden party given by the First Commissioner of Works on the lawn and reserve part of the Royal Gardens, adjoining Kew Palace. We learn that "more than a thousand people thronged the grounds," and that the "host and hostess, accompanied by about 200 guests, arrived at Kew by special

steamers from the House of Commons, Pier." Surely such details and records might be left to the *Morning Post* and the Society journals, which are not published by "Her Majesty's Stationery Office." The "miscellaneous information" is expected to have reference to economic botany, and not to the festivities of "Society." The Director of Kew is commonly supposed to have exhibited a courageous independence of the wishes of exalted personages, who have presumed too much in their visits to the Royal Gardens, and one would have expected him not to tolerate such doings at all, rather than to chronicle their occurrence in the *Bulletin*. Even if this account be followed by a more extended list of less eminent visitors on the August Bank Holiday, with a report of their innocent recreations, it will only show the Director's wide view of the limits of "Society." The one thing would be as unjustifiable as the other, in an official publication, which may, or may not, by its sale repay the Treasury for the cost of its production.

IT might perhaps conduce to promptness if the editor of the *Kew Bulletin* read NATURAL SCIENCE. In last year's July number (vol. i., p. 327) we gave an abstract of a Colonial Report on the Aldabra Islands, a small group to the north-east of Madagascar; while the *Kew Bulletin* for July of this year supplies "some recent information about this little-known group," in great part consisting of a letter from the Administrator of the Seychelles, dated June 13, 1892, which merely repeats some of the facts noticed by us a year ago. About a third of the article is occupied with a letter from Dr. W. L. Abbott, an American, who visited the islands at the end of last year and collected specimens of most of the native plants, which were sent to Kew, but have, unfortunately, miscarried. His short note adds but little to our knowledge of the vegetation already gained from the account given by the lessee, Mr. Spurs, in the above-mentioned Colonial report. Dr. Abbott says: "There do not seem to be more than 35 indigenous species," while Mr. Spurs mentions 30. The only trees now in Aldabra are Casuarinas and Mangroves, but decaying stumps of "Porché" and "Rose-wood" show that these formerly grew to a considerable size, though now only small ones exist. Mr. Spurs referred to the destruction of the large trees, which he considers partly due to the hurricane of 1889.

Apropos of the *Bulletin*, why is it necessary to insert so many official letters spread out into numbered paragraphs like a child's lesson-book? If the information, which is often of interest, were boiled down and the numerous salutations, etc., omitted, the publication would be more readable, and the recent doubling of the price rendered unnecessary.

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I. Four illegalities in its organisation and methods of procedure.

II. Three absurdities, by which the resolutions which they affect become illegal and impracticable.

III. *Violatio juris quæsiti.*

Is *Erythea* on the wane? The last three numbers (May to July) feel very thin, containing only 16, 20, and 16 pages respectively, whereas the journal started with 28 last January, and contained 24 in February and March, while the number rose to 32 in April.

Two alpine anemones, *Anemone alpina* and *A. sulphurea*, are the subject of a notice by M. F. Prévost Ritter, in the *Bulletin de l'Herbier Boissier*, no. 6. The second of these two is generally admitted to be only a colour form of the first, depending on the chemical composition of the soil. H. Christ, in his work on the origin of the Swiss flora, cites them as a most remarkable instance of the influence of the soil on the distribution of plants. In the Alps, he says, the white-flowered form (*A. alpina*) occurs exclusively on the chalk, while the one with a sulphur-yellow blossom is only found on clayey and sandy soil. So sharp is the limitation, that where the two soils meet the two varieties faithfully follow the line of division; while where the transition is gradual, the flower passes by a series of intermediate shades from white to yellow. In order to find out, if possible, the cause of this phenomenon, M. Ritter has, since 1886, been cultivating the seedlings of the two forms in sandy and chalky soils. He finds that, while *Anemone alpina* prefers the chalk, but will grow in sand, *A. sulphurea* will only grow in the latter. He also concludes from differences in the shape of the cotyledons, that the yellow-flowered plant must be considered not as a mere form or variety, but as a distinct species. He says, and his figures bear him out, that the seed-leaves of *A. sulphurea* are larger, and have a shorter and more rounded tip, while those of the *A. alpina* are narrower, oblong, and pointed.

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Japanese *Helwingia* of the Araliaceæ, the saxifragaceous *Phyllonoma* from the New World, and *Polycardia* (Celastrineæ) from Madagascar. The common lime recalls, in a small degree, the same phenomenon, the stalk of the inflorescence adhering to the lower part of the bract and appearing to spring from the middle of that organ. Tropical West Africa boasted already two genera with epiphyllous flowers both belonging to the family Bixineæ, and *Mocquerysia*, the new one established by M. Hua (named after its discoverer, M. Mocquerys) resembles these in some points, and is placed by its author in the same natural order.

IF ordinary placental mammals have evolved from pouched animals like the modern marsupials, rudiments of the pouch ought certainly to be recognisable in some of them. Dr. H. Klaatsch (*Morphol. Jahrb.*, vol. xx., pp. 276–288, 1893), has just made the interesting announcement that such rudiments can actually be observed in most placentals. Something of the kind has already been found in the lemurs, and one author has supposed that rudiments of the pouch can also be detected in the sheep. The detailed account of Dr. Klaatsch's extension of the evidence will be awaited with interest.

M. ADRIEN DOLLFUS provides a first supplement to his Catalogue of the Terrestrial Isopoda of Spain in the xxii. vol. of *Anal. Soc. Espan. Hist. Nat.*, 1893. The same mail brings to us a second paper of his on this group of animals from the Canaries, which adds to our previous knowledge obtained through him in a paper dealing with specimens from the Azores, Madeira, and the Canaries (*Bull. Soc. Zool. France*, 1889, p. 12). Seven new forms are described, and the essential portions figured, in *Mém. Soc. Zool. France*, vol. vi., 1893, pp. 46–56. It is quite possible that diligent search in England might reveal many interesting points in distribution of the wood-lice, and even result in the discovery of new forms.

APROPOS of the "Plague of Wasps" in the South of England, a correspondent suggests our calling in the assistance of the toad. "A toad will," he writes, "sit for some time quite comfortably by a wasps' nest (in the evening, of course) and pick up the belated wanderers as they return home. The sting is absolutely disregarded, and not the faintest attention is paid to the warning colours of the wasp. An evening or two since I persuaded a large fat toad to take up a position actually blocking up the entrance to a nest. He waited there for a few minutes, during which he caught several wasps and made unsuccessful shots with his long tongue at others." One might, perhaps, instal a circle of toads round the orifice of the nest, and then, after stirring it up with a long stick, depart to give free play to the toads and wasps to fight it out between them.

A CORRESPONDENT remarks that in our review of Professor Alfred Newton's "Dictionary of Birds" last month (p. 146), we overlooked the fact that the author had descended to a particularly low level in his criticisms on others. "The remarks on p. 133 regarding the Timeliidæ can only be described as the language of the gutter, and should hardly emanate from a member of one of our old Universities, a person of culture, and, we had hoped, a gentleman. But they impress upon us more than ever the old adage, 'Manners makyth man.'" We are not concerned here with the author's convenient neglect of many well-known publications—that will reflect upon himself in due season.

DR. J. W. GREGORY arrived safely at Mombasa, East Africa, on August 19, and he may be expected in England before the end of September.

ACCORDING to the London *Daily Chronicle*, two members of the Geological Survey of Canada have left this summer for the exploration of Labrador. The exploring party will be absent from civilisation for two years; they will traverse the interior of Labrador from south to north, as well as from east to west, especially visiting the great Lake Mastassini, and the cataract of the Hamilton River, concerning the size and nature of which such fabulous tales have been told. They will winter with the Eskimos on Ungava Bay, where only four of the twenty-four hours of the winter day are daylight. They expect to return to Canada from Hamilton inlet, *viâ* a Hudson's Bay Company's steamer trading with England.

M. MATHIEU MEIG contributes to the *Feuille des Jeunes Naturalistes*, a paper of Excursions in the district of the Lower Carboniferous of Alsace. This is apparently one of a series, and deals with Bourbach-le-Haut, Massevaux, Rothhütel, Rossberg, Stauffen, Steinby and Hundsrücken. M. Meig commences with a useful bibliography of papers on the district, gives notes on the general divisions and composition of the Carboniferous beds of Alsace, and discusses the details in the form of an itinerary.

THE August number of the *Quarterly Journal of the Geological Society* contains an important contribution to the Highland question. Mr. George Barrow has now traced a clear succession in Forfarshire, the comparatively thin strata being repeated by sharp folds, till they form a mass of enormous thickness. The various zones are all highly metamorphosed; but Mr. Barrow shows by analyses and the occurrence of particular minerals, that a certain zone is an altered quartzite, another is an altered limestone, and a third is probably a highly metamorphosed clay. The greater part of this change he

considers to be due to the intrusion of huge sills or laccolites of muscovite-biotite gneiss.

ATTENTION was directed to the Underground Waste of the Land in a former number of NATURAL SCIENCE (vol. ii., p. 124). The subject has also attracted the notice of Professor Lebour. In an article on certain surface features of the Glacial deposits of the Tyne Valley, near Corbridge (*Nat. Hist. Trans. of Northumberland*, etc., vol. xi., part 2, 1893), he states that a notable amount of sand and very fine gravel is continually being discharged from underground, by springs which issue at the junction of the Glacial sands and gravels with the underlying Boulder Clay. He thinks that the bowl-shaped depressions of the surface of the drift gravel and sand of the Tyne Valley—depressions that have the characters of "kettle-holes"—may be due to subterranean erosion. Similarly, features resembling the arched structure of Kames or Eskers may be caused by slipping, due to the eroding action of water below ground.

ANOTHER new British monthly devoted to the science of geology has appeared! We have before us *The Glacialists' Magazine*, vol. i., no. 1, August, 1893. Price 6d. It is edited by Percy F. Kendall, assisted by Warren Upham, C. E. De Rance, and J. Lomas, and its pages are crowded with misprints. Mr. De Rance, who is President of the Glacialists' Association, communicates his address, which was delivered last year, and discusses the geology of the northern Polar regions, concluding that no evidence of former Glacial periods is to be found among the strata. He is, however, scarcely just to the naturalists who accompanied Sir George Nares, when he says it fell to his lot to work up the geological results obtained. The observations were made by Colonel Feilden and others (very competent geologists), while the fossils were identified by Mr. Etheridge. An intrusive mass of Boulder Clay at Bidston, Cheshire, is described by Mr. A. R. Dwerryhouse; and there is a review of Sir Henry Howorth's "Glacial Nightmare" by the Editor. There are also short notes on various subjects, and an account of "Current Glacial Bibliography." The Bibliography, if rendered complete, will no doubt form a valuable record, but otherwise we are inclined to question the advantage of issuing a separate journal on the subject of Glacial Geology. Moreover, we fear that a modest monthly of 29 pages will hardly suffice for the verbosity peculiar to most writers on Glacial Geology.

STILL another American journal relating to geology—the *Bulletin of the Department of Geology, University of California*—has made its appearance. As far as we can judge by the first part, which is all that has reached us, it would, perhaps, be more correctly entitled "Memoirs" than "Bulletin," for it consists of one paper of 59

pages, "The Geology of Carmels Bay," by Andrew C. Lawson, assisted by Juan de la C. Posada. As with most of the American scientific journals, this one is extremely well got up, printing and illustrations both being excellent; but we would like to suggest to our American cousins that the large size, thick paper, and enormously wide margins of all these new journals is a mistake. It is all very well to have one or two large handsome volumes, but when the series mounts to fifty or one hundred, everyone will grumble at the amount of space wasted. This is a matter in which we in the old country are best able to form an opinion, for we have many journals that have reached their fiftieth volume, and all true workers would contemplate with dismay the idea of having to allow double the space for each set.

We regret to learn that the *Midland Naturalist* receives so little support that its publication will be discontinued at the end of the present year.

Two numbers of an "Occasional Paper for Nyasaland" have been printed and published at the Universities Mission Press, at Likoma, an island in Lake Nyasa. They contain notes on Nyasaland Timber, by Mr. William Murray; and memoranda on the occurrence of Garnets and Beryls, by Archdeacon Maples. In future the paper is to be published four times a year, under the name of *The Nyasa News*.

London will shortly rejoice in another club. On and about August 17, a perfect rain of circulars announced to the scientific world that there was no club available "for social intercourse and réunion" of the fellows of the various royal and learned societies. We are glad to observe that so careful a distinction is marked out by the promoters between royal and learned societies. On the front of the circular is a list of "Gentlemen supporting the founding of the Club," whose names can, with patience, be dug out of the perfectly bewildering conglomerate of F.R.S.—F.R.S.G.S.—R.Z.S.I.—Brit. Ass. [*sic*],—etc., etc., in which they are embedded. Then follows the ground for establishing such an arcadia, all for six guineas a year, and lastly, on the back of the circular, comes a series of newspaper-like extracts from letters of the learned, in which the "positive want" is stated with consentaineity, while numerous wailings escape from extra-athenæum clubites. The whole is surmounted by a badge consisting of the American eagle, rather out of drawing, with wings resembling the star-spangled banner, and surrounded by an aureole, in which is hidden "Omnes artes inter se continentur." Colonel W. P. Hodnett, late of the 2nd Dorset (54th) Regiment, as hon. secretary, 63 St. James's Street, will attend to the despairing. *Sic iter ad astra.*

I.

On Epiphytes.

PLANTS are much more directly dependent on the nature of their physical surroundings than are animals, and display a correspondingly greater plasticity of form under varying external conditions. Hence among plants examples of structural harmony with the environment are both striking and numerous; but this malleability in the nature of a plant renders it more difficult to decide when we are dealing with a true instance of adaptation. A leaf exposed to direct sunlight transpires at a greater rate than it would in a damp shaded habitat; and we find that leaves which develop in the sunlight have a structure different to that of leaves of the same species formed in the shade. The leaves developed in the light are structurally so constituted that they transpire less than those formed in the shade, provided that both sets of leaves be exposed to the same conditions; but these peculiarities of structure are not transmitted to the offspring of the respective plants. Here we are not dealing with a case of adaptation—in the narrow sense—but with an instance of the direct effect of the environment on the individual plant. Now this is obviously very different to the case of desert plants, in which the leaves have structural characteristics (succulence, hairs, thick cuticle, sunken stomata, etc.), which are handed down to the offspring g.own under any conditions. Here we can see that the leaves are constituted so as to avoid excessive loss of water—they are suited to the dry habitat. But can we at once pronounce this a case of adaptation to the peculiar mode of life on the desert? Decidedly not. Either the ancestor of the desert plant possessed another form, and only changed it gradually as it became a desert plant; or the ancestor possessed these structural peculiarities even before it became a desert plant, and, in fact, only assumed its present mode of life in virtue of these characteristics. In the latter case, correctly speaking, the desert plant is not adapted, though it is suited, to its peculiar mode of life. The environment has not impressed itself on the nature of the plant. That such cases are possible is shown by the fact that plants occupying widely different positions in nature often betray remarkable structural similarities. For example, many Epiphytes, desert-plants, littoral forms, neighbours of mineral springs, alpine plants, and temperate evergreens possess leaves which agree in being

constituted so as to avoid too rapid transpiration (succulence or leathery or hairy nature, strongly developed epidermis with thick cuticle, water-storing tissue) (1). A plant evolved so as to suit one of the above-mentioned environments would incidentally become suited to one of the other modes of life; and it might desert its old habitat and take up the new one for which it had casually become suited. This process has taken place; even now we find plants of which some individuals are epiphytic in jungles, and others are terrestrial alpine plants. In other species, some of the individuals are littoral plants, which do not reappear inland until we reach the mountain tops; and what is true of these individuals is true of certain species of genera. The genus *Casuarina* is a littoral form and a Malayan alpine form, but does not occur in the jungles between these two extremes. So it becomes very difficult to say what characters in a plant are truly adaptive, *i.e.*, acquired with special reference to the present mode of life. And we can only solve the mystery in each case by, on the one hand, studying the physiological nature of the plant; and, on the other, tracing out its past history by means of morphological comparisons with allied forms, and observations on the geographical distribution of it and its relatives. The first person to recognise this was A. F. W. Schimper, whose work on tropical American Epiphytes (2) revolutionised this branch of botany. He introduced a new method of treating biological problems, which should serve as a model for subsequent investigators.

Epiphytes are subjects peculiarly suitable for the study of adaptation; for among them occur all stages from unmodified terrestrial plants right up to forms highly metamorphosed to suit their aërial mode of existence.

For our knowledge on Epiphytes we are almost solely indebted to Schimper, and the present article is almost entirely based upon his work. Goebel, however, who has followed—far behind—in Schimper's footsteps the study of the biological problems, has been at the trouble to collect facts observed by other investigators of Epiphytes, and has added a few examples illustrating the principles advanced by Schimper and others (3).

Compared with temperate forests, the moist tropical jungles are overwhelmingly rich in Epiphytes. There they vary in size from minute epiphyllous lichens up to the gigantic herbs or huge shrubs. Often an Epiphyte is itself loaded by countless others, on which, in turn, are crowds of smaller ones.

Most of these are confined to a few families:—Lycopodiaceæ, Ferns, Bromeliaceæ, Araceæ, Orchidaceæ, Urticaceæ, Piperaceæ, Melastomaceæ, Ericaceæ, Asclepiadaceæ, Solanaceæ, Gesneraceæ, and Rubiaceæ (as well as our temperate Epiphytes, Mosses, and Lichens). Although these families are few in number, they contain many epiphytic genera; and, with the exception of the purely American family, the Bromeliaceæ, they occur in the tropics of both

the New and Old World. These families must possess some characters which confer upon them the power of assuming an epiphytic mode of existence.

The first indispensable feature is that the seed shall be able to reach the tree on which it is to grow. The only two practicable methods of seed-dissemination are by the wind or by the agency of animals. This fact has been well illustrated by the observations of Loew (4) on the means of distribution of Epiphytes found on willow-tops in Germany. A walk along the banks of an English river, shaded by pollard-willows, reveals the fact that there is a rich epiphytic flora on these trees. Among them are not only algæ, and mosses, but also numerous herbs, and large rose and blackberry bushes. Willis and Burkill (5) found that, near Cambridge, 44·6 per cent. of these Epiphytes had fleshy fruits; 16·4 per cent. had burrs; 25·1 per cent. had winged or feathery fruits; 10·6 per cent. had very light seeds; while in only 2·9 per cent. was the means of distribution feeble or obscure.

So, in the tropics, the seeds of Epiphytes may be all ranged under three main classes:—

(i.) Those enveloped in a fleshy covering (many bromeliads);

(ii.) Seeds (or spores) very light and easily carried by the wind (*Dendrobium attenuatum*, the seed of which weighs ·00000565 grammes);

(iii.) Seeds light and with a mechanism for floating in the air. ("Wings" in *Rhododendron pendulum*, in which the seed weighs ·000028 grammes: tufts of hairs in Asclepiadaceæ: air-containing spaces in the envelope, *Æschynanthus pulchra*.)

In all cases of these regular tropical Epiphytes the seeds are small, so that they can easily become lodged in minute crevices on the surface of the host. Those characters are not adaptive. The seeds must have possessed them to a large extent when the plants were terrestrial; otherwise the latter could have never become Epiphytes. The structure of their fruits and seeds explains why certain families should not have Epiphytes among them—thus the heavy-seeded Leguminosæ. On the other hand, in certain families only those types which possessed appropriate seeds have become Epiphytes. For example, the Loganiaceæ frequently possess fleshy fruits, but the seeds are large excepting in *Fagræa*, which accordingly has epiphytic species.

Even when the seed has reached the surface it must rapidly make arrangements for the firm attachment of the embryo. Only a few observations exist on the germination of Epiphytes (*Clusia*, *Myrmecodia*, *Æschynanthus*, *Tæniophyllum*). In all these the primary root soon ceases to grow, and there is a precocious development of an extensive system of adventitious roots. In *Æschynanthus pulchra* (3) the radicle remains short, the inferior end of the embryo becomes flattened, and

its periphery swollen. In this manner a species of adhesive disk is formed and applied closely to the uneven surface of the host. The attachment is rendered all the firmer by the formation of numerous root-hairs radiating from the peripheral swelling.

Thus the whole root-system of an Epiphyte—except at a very early stage of germination—is adventitious. The same is the case with shrubs growing in our own climate on rocks or walls. In both cases this is obviously the direct effect of the mode of life.

The flowers of Epiphytes display no adaptive characters.

Concerning the modes of reproduction, vegetative multiplication is frequent. The case of *Tillandsia usnevides* is interesting. Its pendent shoots are either whisked away by the wind or carried away by birds for the purpose of constructing nests. The transferred fragments send out new shoots, which twine round the branches of the new host. This *Tillandsia* only rarely produces flowers.

It is in the structural characters of the vegetative organs that Epiphytes display the greatest amount of adaptation.

Epiphytes grow in situations where the supply of nutritive solutions is meagre and fluctuating, and where, usually, only diffused light reaches them. Moreover, they live in positions in which firm support is essential. For fixation and the absorption of nutritive solutions, a strong development of surface of the root-system is indispensable. Owing to the poverty of their water-supply, measures must be taken to avoid excessive transpiration. A desert plant accomplishes this by diminishing its transpiring surface; but an Epiphyte cannot afford to act similarly, because it requires a large assimilatory surface in order to make up for the weakness of its illumination. The Cactaceæ illustrate this point. The desert forms often have swollen, rounded shoots which are sparsely branched; but the epiphytic forms possess flat, leaf-like shoots (*Phyllocactus*, *Epiphyllum*), or have numerous thin branches (*Rhipsalis*, *Cassytha*). It only remains, then, for the Epiphytes to have leaves constructed so as to retain their supply of water. Hence the leaves are often leathery or fleshy, and frequently have a strong development of cuticle.

The mode in which Epiphytes absorb nutritive solutions serves as a basis for their classification. Appended is a brief table to show the grouping:—

CLASS I.—*Epiphytes which absorb the nutritive solutions by means of their roots.*

Group (i.) Those which content themselves with utilising the water and humus occurring on the surface of the host-plant.

Group (ii.) Those which have attaching roots and also absorptive roots which descend to the ground.

Group (iii.) Those which have arrangements for collecting water and humus.

CLASS II.—*Epiphytes which absorb the nutritive solutions by means of their leaves.*

Class I.—Group i.

The first group of Epiphytes is made up of forms which merely absorb the nutritive solutions occurring on the surface of the host-plant.

The simplest of these do not differ in any way from terrestrial plants. They live in the gloomy depths of the forest on the mossy or furrowed bark of the lower parts of trees (Hymenophyllaceæ, *Lycopodium*, and an orchid *Stenoptera*).

However, most adopt measures to guard against injury from excessive loss of water. A few forms evade injury in this respect simply by their ability to endure being completely dried up without being harmed. This is well known in the case of lichens and mosses; but certain tropical polypodies are endowed with similar vitality. Rainless weeks of the blazing West Indian sun are insufficient to scorch out of existence *Polypodium incanum*, parched though it be.

In the majority of genera there are distinct structural modifications in the form of water-reservoirs. Parts of them become fleshy and store up water. Fleshy leaves occur in ferns, orchids, and Gesneraceæ; bulbs in Amaryllidaceæ; tubers in ferns, and epiphytic Utricularias; fleshy roots in Vacciniaceæ, and the curious mi- 365
stomaceous *Pachycentria*.

This water may be stored up in intercellular spaces, in ordinary parenchymatous cells, or in specialised cells ("aqueous tissue," or "storage tracheides").

The leaves of certain Gesneraceæ have, between the upper superficial layer and the green assimilatory tissue, aqueous tissue in the form of colourless cells. This tissue increases in size late in the life of the leaf, long after all other morphological changes have been accomplished. Isolating old leaves for four weeks, they diminish in thickness only to a very slight degree; but if whole branches be isolated for the same period, the old leaves become much thinner (and gradually die), because the aqueous tissue has parted with water, and has shrunk; but the young leaves are scarcely any thinner than at the commencement. These observations show that the aqueous tissue is a reservoir on which the younger leaves can draw, and that it takes on its peculiar function late in the life of the leaf.

It is, however, among aroids and orchids that the most interesting arrangements occur.

In the Araceæ, aqueous tissue is not known. *Philodendron cannifolium* may be chosen as a type of aroid belonging to this group of Epiphytes. It is a large herb with its leaves forming a rosette more than three feet in height. The stem is short and gives off numerous roots. Each leaf has a tongue-like lamina and a spindle-like petiole. The leaf possesses spongy tissue with large and conspicuous intercellular spaces. In the dry season the spaces are occupied by air, but when

the wet season comes on, the intercellular system being lined with mucilage, absorbs water which gradually replaces the air. The swollen petiole is a water-reservoir calculated to meet the demands of the lamina. This is proved by the following experiment: If a whole leaf be isolated and left to dry for fifteen days it remains unchanged in appearance; but if the lamina alone be treated thus, it fades and shrinks in a few days.

Among the Orchidaceæ, the arrangements for the storage of water are varied and characteristic. Some possess leaves with well developed broad tracheides, the function of which is to store water. In others, the water is retained in pseudo-bulbs. Experiments similar to those on *Philodendron* prove that the latter are water-reservoirs. For this reason, orchids with pseudo-bulbs usually have thin leaves; whereas, orchids with fleshy leaves are devoid of pseudo-bulbs. Only a few orchids have thin leaves and no pseudo-bulbs, but, to compensate, they have fleshy roots (*Isochilus*). Attention has already been directed to the orchid *Stenoptera* which has no water reservoir, as it grows in damp, gloomy spots.

The most curious structural peculiarity which is common to all epiphytic orchids (almost without exception) is the velamen possessed by the roots. Typically this envelope is made up of a number of layers of tracheides with perforated walls. These absorb water deposited as dew or flowing down the surface of the host; but it is quite a mistake to suppose that they have any appreciable power of condensing moisture from the atmosphere. The velamen is not always an absorptive layer. In *Bromheadia alticola* (6) it is a sheath to prevent the evaporation of water which is absorbed by root-hairs which attach the plant to the substratum.

The question arises, "Is the velamen really adaptive?" I am of opinion that the answer must be given in the affirmative, in spite of the cautious scepticism of Schimper on the subject. With the single exception of one species of *Stenoptera*, all epiphytic orchids have a velamen. All other known species of *Stenoptera* are terrestrial, and this species only grows in damp, moss-laden, furrowed bark in the lower regions of forests; moreover, its structure is purely terrestrial. We must conclude that this *Stenoptera* has only recently adopted its epiphytic mode of existence (6). On the other hand, with the exception of two species, all terrestrial orchids known are devoid of a velamen. *Epidendrum cinnabarinum* is one of these exceptions, and has a velamen. Now it is almost always a terrestrial orchid, but Fritz Müller[1] has found it as an Epiphyte. It is a renegade Epiphyte which has become terrestrial, as is confirmed by the occurrence on it of useless aërial roots, and by the fact that its relatives are Epiphytes. The other exception is the purely terrestrial *Bromheadia palustris* (6). It undoubtedly has been derived from an epiphytic form. It possesses a curious impermeable velamen like that of the epiphytic *B. alticola*; but this velamen is exceed-

[1] Communicated by letter to Professor Schimper.

ingly transitory and peels off almost simultaneously with the root-cap cells. The last reason for believing that the velamen is adaptive is afforded by its re-appearance in Epiphytes belonging to the widely-separated family, the Araceae. Thus Epiphytes acquired their velamen after they had deserted their terrestrial mode of life.

When young, the aërial roots of orchids usually contain chlorophyll; but their assimilatory function is insignificant. However, in *Æranthus* and in *Taeniophyllum* the leaves are reduced to scales, while the roots are large and green. The plant consists almost solely of root-system. In the roots of *Æranthus*, Schimper discovered that certain regions of the velamen are impermeable to water, but are easily penetrated by air. These regions obviously function as stomata do in leaves. In *A. fasciola* the leaf-like appearance of the roots is heightened by their dorsi-ventral structure.

Class I.—Group ii.

Members of the second group of Epiphytes possess roots of two sorts—they are hence heterorhizal, and exhibit an advance on the first class. When the seed germinates on the host-plant, some roots are produced which clasp the host like tendrils. These are purely organs of attachment. The plant also produces long nutritive roots which descend rapidly to the ground.

Such roots are possessed by *Clusia* and *Ficus*, among *Dicotyledons*: and by species of *Philodendron*, *Anthurium*, and *Carludovica*, among *Monocotyledons*.

The attaching roots are negatively heliotropic, but have no marked geotropic properties. They have a slow, definite growth, and coil closely round their support, giving off numerous root-hairs. They have no absorptive function. In them the xylem consists mainly of thick-walled lignified fibres with relatively few and narrow conducting constituents.

The nutritive roots, in some cases, possess negatively heliotropic characters, but they are always positively geotropic to an eminent degree. Their growth is rapid and indefinite, some roots attaining a length of more than a hundred feet. Till reaching the soil they remain thin and unbranched, but on dipping into the soil they give off numerous subterranean lateral roots. The nutritive materials from the soil have to travel over long distances by way of these roots. The conducting constituents of the xylem are accordingly numerous and prominent, whereas the fibres are feebly developed. The necessary amount of rigidity is ensured by the formation of peripheral sheaths of sclerenchyma or collenchyma.

Clusia and *Ficus* frequently "strangle" their hosts by investing their trunks with a fenestrated cylinder formed of the fused roots which embrace the trees on which these Epiphytes originally lodged.

Class I.—Group iii.

The members of the third group possess arrangements for the

retention of decaying humus-giving bodies, and the roots frequently branch copiously in the air, some of them deserting the surface of the host.

In many epiphytic orchids there are attaching roots which are negatively heliotropic, and in addition clusters of erect negatively geotropic roots which are wholly absorptive in function. In orchid houses, potted specimens of these orchids frequently possess roots growing vertically upwards out of the soil (species of *Cymbidium*, *Eulophia*); but there is no reason to believe that these roots act thus in order to supply the subterranean parts with oxygen. They simply point upwards because, when the plants are growing in their natural position (as Epiphytes), the nutritive materials come from above, and are absorbed by these peculiar roots.

An aroid, *Anthurium Hügelii*, is a good type of an Epiphyte belonging to this group. Although a large plant, it is often to be found fastened to the smooth aërial roots of *Clusia*, or to the band-like stems of *Bauhinia*. It is fixed to its support by means of very long roots, which stretch along the surface of the host and give off root-hairs only on the side towards the latter. An extensive rounded complex of roots surrounds and overtops the dwarfed stem, and gives off numerous lateral roots among the sub-sessile leaves. The last-named form a large rosette enclosing a mass of vegetable fragments, which decompose into humus. The complex is composed mainly of much branched and hairy nutritive roots which are closely interlaced at their bases, but which possess apices freely protruding into the air or dipping into the humus.

In this plant, as in the rest of this group of Epiphytes, the structural differences between the attaching and nutritive roots are not so wide as in the second group. This is due to the fact that the attaching roots absorb water to a certain extent, and that the nutritive roots do not conduct the absorbed substances over long distances. Still, the nutritive roots of *Anthurium Hügelii* possess larger and more numerous vessels and fewer sclerenchyma-fibres than do the attaching-roots.

Some ferns have precisely similar arrangements, *e.g.*, *Polypodium phyllitidis*.

In other ferns the humus is collected by single leaves which form pockets against the surface of the host's stem, or against their own stems. In *Polypodium quercifolium* (3) the differentiation has gone so far that there are two sorts of leaves: in the first place, ordinary stalked pinnate leaves; secondly, sessile negatively geotropic leaves, which form pockets against the opposed surface. The latter assimilate only for a brief period, but persist as large, dead, yellow oak-like leaves. Thus when dead they do not drop off (as do the normal foliage leaves), but, being strongly ribbed, persist as receptacles for humus.

Another heterophyllous Epiphyte is the curious twining, pitchered

Dischidia rafflesiana. Some of its leaves are hollowed out into pitchers. It has roots of two sorts—attaching and nutritive. The latter are given off into the cavities of the pitchers, within which they ramify. Treub (7) was of opinion that these pitchers functioned solely, or mainly, as receptacles for rain-water and economisers of the water transpired within the pitchers. He showed that the epidermis lining the pitcher-cavity could not absorb water. But I have shown that very considerable amounts of humus-providing materials are collected in these pitchers (8), and that the roots within the pitchers absorb this food placed at their disposal. These pitchers have the same function as the rosettes of *Anthurium* or the "pocket-leaves" of ferns —they collect humus. However, they are able to collect more water than is possible to their simpler analogues. One curious fact about these pitchers is the constant presence of ants living within them. Probably a large portion of the food found in these pitchers is conveyed thither by the ants. The plant provides shelter for the ants, and they in return bring these materials (for the purpose of building nests).

Externally unlike *Dischidia rafflesiana*, but very similar in fundamental design, are two Rubiaceous Epiphytes—*Myrmecodia* and *Hydnophytum*. In each of these genera the base of the stem takes the form of a great swollen tuber, which is hollowed out by a series of galleries or canals continuous with one another, and communicating with the exterior by, mostly small, apertures. These galleries are always found to be occupied by innumerable ants belonging to a certain definite genus and species. In a typical case, lenticels occur in the walls of the galleries from which there protrude, more or less, distinct roots. In principle, then, this great tuber corresponds to a pitcher of *Dischidia*; the stomata of the epidermal lining of the pitcher correspond to the lenticels opening into the galleries; and the roots protrude into the galleries and the pitcher-cavity respectively. Rain-water can only pour into some of the pitchers of *Dischidia*, because their mouths sometimes point downwards; but rain-water can enter freely into the galleries in only one known species. In most cases, the external openings of the galleries are so small that water could only enter in the form of a thin capillary film. Formerly it was supposed that the ants constructed the galleries, and that these animals were essential to the existence of these plants; but Treub (9) showed that the galleries developed in the absence of ants, and that the plants could exist without the proper ants. The hypothesis that the galleries are respiratory passages is quite inadequate. The whole arrangement has been looked upon as a mechanism for the economy of water. This hypothesis is backed up by no experiments, and, at present, is only bolstered up by a few facts, and more or less distant analogies.

It is still possible that the ants have been the prime factors in the evolution of these excavated tubers. The development of the

galleries is not out of harmony with this view. The observation that under cultivation these plants live without their ants, tells us naught of what would happen to them in their wild state under similar circumstances; and it is futile to object that the plants cannot be myrmecophilous (ant-loving) because they do not attract ants by means of honey or food. Ants are attracted to these plants. This is proved, first, by the constant presence of a certain definite species of ant in specimens growing wild; and, secondly, by the extreme difficulty in preventing ants crowding on cultivated seedlings of these plants, even when adjacent seedlings of other plants are neglected. It is possible that ants convey into the galleries materials which can be subsequently utilised by the roots therein. But at present *Myrmecodia* and *Hydnophytum* are living mysteries.

Class II.

The second class, represented by the Bromeliaceæ, stands apart from the types previously described in that food and water are absorbed by the leaves—and not to any appreciable extent by the roots.

Often the leaves together form a very close rosette—a funnel—which contains water with humus-giving bodies. By isolating some of these funnel-forming bromeliads, Schimper proved that the leaves absorbed nutritive solutions. Living specimens were deprived of their roots and the wounds were closed with Canada balsam. Water was poured into the funnels of some of these, but the rest were kept without water. The former remained fresh as long as the experiment lasted—ten to twelve weeks; but the latter died in the course of a few days or weeks. Water supplied only to the roots of isolated specimens neither restored faded plants nor appreciably retarded the death of freshly isolated individuals. This experiment shows that the roots absorb only a negligible amount of water—they are purely organs of attachment. Hence epiphytic bromeliads which have other means of fixation are devoid of roots.

Tillandsia usneoides is one of the commonest rootless Epiphytes belonging to the Bromeliaceæ. This plant forms silvery-grey, pendent masses, which resemble horses' tails or manes. These tufts attain lengths of more than ten feet. Each thread consists of a filamentous distichous shoot, which, at its base, twines round a branch of its host. Hanging down as it does, the plant possesses no external, funnel-like reservoir for water.

The water is absorbed by means of certain peculiar scaly hairs. In funnel-forming bromeliads these are confined to the basal parts of the leaves, thus they only occur in the region of the water-reservoirs; but in forms, like *Tillandsia usneoides*, having no external receptacle for water, the scaly hairs are distributed over the whole surface.

In *Tillandsia*, which may serve as a type, each scaly hair consists of: (i.) a stalk-like *base* sunk into the tissue; (ii.) a peltate upper part

—the *shield*—lying flat over the surface of the leaf. This shield has a central group of cells, and a thin extended margin composed of cells or cell-walls. Underneath the *base* is a group of three cells on top of one another; they are protoplasmic and have thin uncuticularised walls (except sometimes the lowest wall is thick but pitted). These three cells are "passage-cells."

If a drop of water be placed on a leaf of *Tillandsia usneoides*, the silvery-grey appearance is locally replaced by a green colour. Examination with a microscope reveals the fact that at first the central cells of the *shield* contained air, but on supplying water these cells become filled with it. Placing a drop of caustic potash or strong sodium chloride solution on the leaf, it can be seen that the colour changes or plasmolytic phenomena commence first in the epidermal cells nearest the scaly hairs, and gradually advance to more distant cells. Finally, the cells composing the sheets have their lower walls thin and uncuticularised, as also are the walls of the stalk and passage-cells; whereas the rest of the epidermal cells have thick cuticularised walls. Again, terrestrial bromeliads which have absorptive roots possess scaly hairs which are incapable of being wetted. These facts prove that the peculiar scaly hairs of the epiphytic bromeliads absorb nutritive solutions. The upper wall of the scaly hair is thick and heavily cuticularised. It functions as a mechanism to prevent evaporation of water. Accordingly when those hairs are sunken it is relatively thin, but in protruding hairs it is thick. When the central cells of the shield contain air their lateral walls are crumpled so that the shield lies close above the surface of the leaf (thus preventing loss of water by evaporation); but when these cells absorb water, their walls straighten and thus raise the shield above the general surface.

It is impossible to mention all Schimper's interesting observations on this part of the subject. However, he points out that some terrestrial bromeliads possess absorptive scale-hairs, and he makes out a good case for the view that the possession of these scaly hairs in epiphytic Bromeliaceæ is not adaptive, but that these plants become epiphytic because they already possessed these curious hairs.

General Conclusions.

It has been pointed out that in the first three groups forming Class I. the nutritive solution is absorbed by the roots. In the first group no provisions are made for the collection of water or humus otherwise than from the surface of the host-plant. This group commences with the simplest forms which are entirely terrestrial in structure, and only grow on the lower parts of trees in damp and ill-lighted spots; but the rest of the group are characterised by a great development of the surface of the root- and leaf-systems. The leaves have a thick cuticle, etc., to avoid excessive transpiration: and internal water-reservoirs are usually present. But owing to the poverty of the supply of water and humus, and to the simple measures adopted in

utilising these, the plants in this group are, for the most part, merely small herbs.

In the next two groups the xerophytic structure is, on the whole, less pronounced; for these forms have advanced and have adopted means to ensure a richer supply of food and water. In the second group the roots have become differentiated into two sets—attaching and nutritive. The latter descend to the ground. These forms can be directly derived from the first group, and intermediate types occur in which the nutritive roots reach the ground rather by chance than in virtue of any strong geotropic properties (*e.g.*, some Cactaceæ).

The third group has for the most part roots differentiated into attaching roots and absorptive roots which desert the substratum and often are negatively geotropic. The roots or leaves, or both, make provisions to collect humus. In the highest types not only are there two sets of roots, but there is a differentiation in the leaves, some of which are set apart to collect humus (species of *Platycerium*, *Polypodium*, and *Dischidia rafflesiana*). This group, too, is derived directly from forms like the first group.

The second class—the Bromeliaceæ—have been derived directly from terrestrial bromeliads. They have become epiphytic in virtue of the power which their leaves already possessed of absorbing nutritive solutions. They have been formed utterly independently of, and have pursued a course quite different to, that of the first class. Yet it is striking to note how the effect of the environment has been stamped on forms belonging to the two classes; and to see how the physiological anatomy of two forms may be identical and yet attained in a totally different matter. For instance, comparing *Tillandsia usneoides* with *Æranthus*, both are formed of pendent, green, assimilatory organs. Yet the one is wholly made up a shoot-system, and the other, practically, entirely a root-system. In both there is a general external absorptive mechanism—numerous scales and velamen respectively. The nutritive solution reaches the assimilatory tissue after first passing through a layer cuticularised except at certain points—the "exodermis," with thin-walled passage cells, and the epidermis with the passage cells under each scaly hair. Here and there free communication exists between the atmosphere and the assimilatory tissue—by stomata in one, and by air-containing patches in the other. The assimilatory tissue lies within the cuticularised layer. Rigidity is ensured in both by sclerenchyma-fibres. In both there is a feebly-developed vascular system, corresponding to the fact that each part of the whole surface is capable of assimilating and of absorbing nutritive solutions, and that hence conduction over long distances is not necessary.

Distribution of Epiphytes within their own Region.

Moisture is the first great factor which controls the distribution of Epiphytes. This is true, not only of the distribution of Epiphytes

in their own region, but also over the world as a whole. It is not temperature which determines the presence or absence of Epiphytes. They are absent from deserts which have a tropical temperature; whereas shrubby representatives of this class are to be found on the Himalaya, in regions where the average temperature is below that of England.

This demand for moisture is well illustrated by the distribution of Epiphytes in England. Out in the open they are confined to a few lichens, minute algæ, few mosses, and a couple of sorts of liverworts. But in woods, where the air is damper, the number of these epiphytic forms is much greater, and occasionally ferns are found epiphytic. Where dampness of the air is combined with good illumination, there is a still richer epiphytic flora; for instance, on pollard-willows lining a river-bank there are many Epiphytes, some of which attain the size of bushes (Rose, etc.).

So, in the tropics, the moisture-laden jungles and primæval forests abound in Epiphytes; on the other hand, the open savannahs have but few.

The second controlling factor is *light*. The demand for light drives Epiphytes to the tops of the trees in a tropical forest. In wandering through one of these forests it may seem as if there are but few Epiphytes, till one espies their great aërial roots descending to the soil and their fallen flowers and fruits dappling the ground.

To a certain extent, light and moisture are wants which the Epiphyte cannot obtain at the same time to the fullest extent. In general, one may say that shade involves a moister atmosphere and illumination a drier one; in addition, the stronger the light, the more rapidly does a leaf transpire. Hence, in a tropical (American) forest, one finds the Epiphytes ranged at different heights up the trees, and the same forms constantly occur in the same positions. Low down in the shade, on the trunks or thick lower branches, there are but few. These are moisture-loving and shade-bearing species, especially Hymenophyllaceæ, Lycopodiaceæ, delicate *Peperomias* and orchids (*Zygopetalum*), green bromeliads. On the tree-tops are forms which can endure strong light, and are constructed so as to withstand the danger arising from excessive transpiration (grey *Tillandsias*, thick-leaved orchids without pseudo-bulbs, leathery polypodies). Between these two extremes, on the branches below the tree-tops, the richest and most varied epiphytic flora prevails. It is at this level that the giants among Epiphytes reign, and countless orchids with pseudo-bulbs flourish.

Going out into the tropical American savannahs we find the Epiphytes far fewer. They are largely identical with those growing at the extreme tops of the forest-trees. Only a few forms from the middle stratum of Epiphytes occur, but none from the lowest stratum.

Corresponding to the above-mentioned facts, we find that densely-foliaged trees have few or no Epiphytes on them (Mango),

unless indeed the tree have deciduous leaves (South Brazilian fig trees), in which case numerous Epiphytes occur.

The *nature of the surface* of a plant also determines the number and sort of Epiphytes which lodge on it. Only a few forms can exist on very smooth surfaces. These are chiefly Bromeliaceæ, which may be found on polished stems of palms, in glassy shoots of bamboos, on leaves, and on smooth aërial roots. They are the pioneers among Epiphytes, as they germinate on smooth spots unsuitable to other forms. Among their roots these more exacting plants settle, so that a colony of Epiphytes springs up round the originally solitary bromeliad.

On the other hand, there are forms which cannot live except on very uneven surfaces, on rough or mossy bark, in the persistent leaf-sheaths of palms (especially ferns).

Most Epiphytes avoid trees with a peeling or scaling bark.

In addition, the distribution of Epiphytes on plants depends on causes not yet understood. For instance, an epiphytic orchid in North Florida especially affects *Magnolias*. *Crescentia cujete* is a tropical American tree which is the greatest favourite with Epiphytes. Schimper suggests that this is due to the facility with which root-hairs penetrate its soft superficial cork.

The Evolution of Epiphytes.

We are now in a position to trace out the mode in which terrestrial plants became Epiphytes in tropical America, and similarly in other tropical regions.

In the primæval forests one sees terrestrial forms allied to the epiphytic species growing at different heights up the trees. This is not so in the open country, say in the savannahs, where the Epiphytes are allied to those occurring in the tops of the trees in the forests. This fact, and the general principle that moisture of the air largely determines the presence, or absence, of Epiphytes, leads us to the result that Epiphytes were evolved in the regions of moisture—the forests.

There are now species which grow in the forests as terrestrial plants or as Epiphytes, but in the latter case they invariably grow in moist gloomy spots on the lower part of trees; and, in general, shade-loving forms occupying positions low down on the trees are the simplest and least modified Epiphytes. They are the most recent recruits in many cases.

Thus to us comes the idea that when Epiphytes started their aërial mode of existence, they commenced in these shady moist positions not far above the ground. I may suggest that this view of Schimper's receives support when one particular group of Epiphytes is considered—that is, the group in which some of the roots descend to the ground, often having to be more than a hundred feet long to attain this. The evolution of such roots is absolutely inconceivable,

unless we suppose that the epiphytic ancestors of the plants possessing them were once much nearer the ground than their present representatives are.

The plant, then, having become an Epiphyte occupying the lower regions of the forest, its next demand would be to get into more strongly-lighted places so as to assimilate at a greater pace. So the next step was for the Epiphytes to climb higher up the trees. With this arose the necessity for provision against excessive transpiration and for more elaborate organs for absorbing water. Gradually, then, the Epiphytes crept up the trees, becoming more adapted to their habitat as they struggled with one another (and with their hosts) in the fierce battle for light. In this manner they reached the higher branches of the trees—but below the tree-tops. This is the paradise of the Epiphytes; the light is not too intense, but filters through a veil of leaves.

To escape their many competitors, a few bolder spirits advanced almost into the full blaze of the sun on the tree-tops, taking measures, as they ascended, to avoid being parched. From these points of vantage, some of them, and a few from the middle stratum, winged their way to the savannahs, others climbed mountains and became tropical alpine forms living on the ground (1). Still others journeyed to the sea-coast, and descending to the ground became littoral forms with their roots dipping into the salt-laden soil (1). A few which have entirely, or almost entirely, deserted their epiphytic mode of life bear structural evidence of their former epiphytic mode of existence (6).

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PERCY GROOM.

II.

On the Relation of the Fauna and Flora of Australia to those of New Zealand.

THAT the flora of New Zealand should present so many features akin to that of Australia, and yet entirely want the distinctive botanical characters of its nearest continent, is a problem to which Hooker first drew attention. No solution was attempted by that writer, who limited his discussion of the question to a clear statement of the facts in a passage[1] so frequently quoted that it need not be here repeated.

The explanation offered by Wallace in "Island Life," and generally accepted, is: (1) commencing the biological history of Australia with the Cretaceous era, that Eastern and Western Australia were then totally severed; (2) that Eastern Australia was at that time quite devoid of a typical Australian terrestrial fauna and flora, which was then confined to Western Australia; (3) that a large area of what is now the floor of the Tasman Sea was upheaved, and nearly, or quite, connected New Zealand with Australia, whereby the flora and fauna, then existing in Eastern Australia, were enabled to colonise New Zealand; (4) that this hypothetical bridge then sank, isolating the New Zealand colonists, and afterwards dry land appeared between Eastern and Western Australia, upon which the characteristic Australian forms first crossed from west to east.

Apart from the difficulty they are supposed to satisfy, little or no evidence is advanced for the support of these propositions. That a Cretaceous sea separated Eastern from Western Australia may be granted, but that at any time Western Australia possessed a monopoly of characteristic Australian plants or animals is improbable. Recent discoveries show that the dry land representing Western Australia in Cretaceous times was much less than Wallace supposed, and than appears in the sketch map on p. 497 of the second edition of "Island Life." In the region explored by the Elder Expedition, "an area of Mesozoic formation extends as far as long. 123°."[2] The shallow inland Cretaceous sea was studded with islands, large and small, which served the fauna and flora as stepping-stones in their migrations from west to east and from east to west. Some of these are now

[1] J. D. Hooker, "Introductory Essay to the Flora of Tasmania," pp. lxxxviii.-ix.

[2] V. Streich, "Scientific Results of the Elder Exploring Expedition, Geology," p. 88.

represented in Central Australia by the Macdonnel, Musgrave, and Everard Ranges;[3] in Queensland by an extensive area of Palæozoic schists, extending almost from the Gulf of Carpentaria to beyond Cloncurry, reappearing in the south as the Grey and Stokes Ranges,[4] and yet again in New South Wales as the Barrier Range. If allowance be made for denudation, more islands probably then existed than geological maps now indicate. Tertiary fossils from Victoria, N.S.W., and Queensland show as marked an Australian facies as recent plants and animals. Such are the various species of *Banksia* from the Lower Miocene.[5] The very characteristically Australian remains preserved in the Darling Downs' deposits cannot, says De Vis, be considered as of later date than Early Pliocene.[6] Finally, many genera and species among recent plants and animals essentially Australian are confined to Eastern Australia. Wallace correctly remarks that West Australia is richer than the East in peculiarly Australian plants, but when, as described later, the archaic Queensland flora was submerged and almost extinguished by the Papuan invaders, great numbers of such peculiarly Australian plants must have perished in Eastern Australia.

More in need of confirmation than the preceding propositions stands the theory that a large tract of the Tasman Sea connecting Australia with New Zealand was upheaved, and when it had served its purpose conveniently, subsided at the close of the Cretaceous. Except about the twentieth parallel, where a bank no deeper than 1,300 fathoms connects the Great Barrier Reef with New Caledonia, the shores of Eastern Australia everywhere front deep water of over 2,000 fathoms, to the north the abyss of the Coral Sea, to the south that of the Tasman Sea. The geological history of this coast, as opposed to that of New Zealand, indicates great stability; such a vast elevation and subsequent depression as Wallace demands could scarcely have occurred without leaving signs decipherable by geologists. If the upheaval was local instead of general, it must, to satisfy the theory, have taken place between Bowen and Rockhampton, where the Caledonian Barrier Bank abuts on the Queensland coast. No consequent distortion of the strata appears to have been there observed by local geologists.

Upon detailed examination, it appears that although the fauna of New Zealand sometimes approximates to that of Queensland, yet it could not have been derived therefrom; that, in short, the relation is not that of mother and daughter, but that of sisters. An analysis of the Australian land shells shows the range of the operculated forms

[3] V. Streich, *Op. cit.*, p. 80.

[4] *Vide* sheets 3, 4, and 5 of the Geological Map issued with "The Geology and Palæontology of Queensland and New Guinea." By R. L. Jack and R. Etheridge, Jun.

[5] Ettingshausen, "Contributions to a Tertiary Flora of Australia," pp. 138-143.

[6] C. W. De Vis, *Proc. Linn. Soc. N.S.W.* (2), vol. vi., p. 456.

to be restricted to the coast of Queensland, and that, proceeding from south to north, the group becomes gradually richer in species and in genera. In the New Zealand fauna the operculates are represented by a tenth of the snail fauna as compared to none in Australia, except Queensland, to one-sixth in Queensland, and one-third in New Guinea. So far our investigation conforms to Wallace's theory, and as that demands an origin in Australia for the New Zealand fauna, so we expect the operculate land mollusca of New Zealand to be descendants of those of Queensland. Here, however, facts rebel against the theory, the New Zealand species being ranked under *Lagocheilus*, *Realia*, and *Hydrocena*, while the Queensland ones are contained in *Georissa*, *Truncatella*, *Pupina*, *Callia*, *Pupinella*, *Hedleya*, *Ditropis*, *Diplommatina*, *Leptopoma*, and *Helicina* ; than which two faunas could hardly be more distinct.

Most European writers who have touched on the zoo-geography of Australia have described the fauna and flora as falling into a temperate and a tropical division, which again subdivide into eastern and western sections. A little real experience proves these divisions to be quite artificial. The dry interior, for example, is everywhere inhabited by the same plants and animals, regardless of latitude or longitude; examples are, the great red kangaroo among mammals, the plain turkey, the maller hen and the rosy cockatoo among birds, the frog *Notaden bennetti*, the lizard *Lygosoma monotropis*, and the snakes *Pseudonaja nuchalis* and *Hoplocephalus nigriceps*, among plants the predominance of salt-bush, spinifex, gidya (acacia), and *Frenela*. On the eastern seaboard, the difference between north and south is that between a conquering and invading fauna and flora and the indigenous population where not exterminated or obscured by them. The types encountered by a traveller in tropical Queensland, or rather in that narrow belt of tropical Queensland hemmed in between the Cordillera and the Pacific, all wear a foreign aspect. Among butterflies the *Ornithoptera*, among reptiles the crocodile, among birds the cassowary and rifle bird, among mammals the tree kangaroo and the cuscus, and among plants the profusion of epiphytic orchids and of palms, all point to a northern origin. In the heart of a great Queensland "Scrub" a naturalist could scarcely answer from his surroundings whether he were in New Guinea or Australia.

Late in the Tertiary Epoch, as I read the record, Torres Straits, now only a few fathoms deep, was upheaved, and across this bridge there poured into Australia a stream of Papuan life. Between the coastal range and the sea as far as the tropics the irruption flowed in undiminished strength; on reaching the border of N.S.W. the cooler climate diminished its vigour, and at the Clarence River, N.S.W., with few exceptions, it found its southern limit. Within this area grow side by side, like oil and water—touching yet not commingling, two distinct vegetations, the dense Papuan jungle called "Scrub" by the Queenslanders, which has usurped every rich volcanic upland and

every fat alluvial plain, and the lightly timbered "Forest" of acacias and eucalypts confined to the inferior soils. As sharp a line is drawn between "Scrub" and "Forest" as a European may see between an Alpine meadow and a thick pine wood.

In considering the contour of the floor of the Western Pacific in relation to the distribution of species, we are greatly hampered by want of information. Too few and distant are the deep-sea soundings to determine the trend of the numerous banks and troughs of this region. Something can, however, be deduced from the "Challenger" discoveries. When that vessel ran a line of soundings across the Coral Sea, she found that the temperature of the soundings diminished down to 1,300 fathoms, below which depth it was stationary, proving that no cold stream from the depths of the Central Pacific or Antarctic Oceans could penetrate and chill this basin;[7] from which it is to be inferred that no gap deeper than 1,300 fathoms crosses the rampart extending from the Solomons to New Zealand. This region, in contrast to Australia, is one of great volcanic activity, and there are evidences of great fluctuation of levels in the past. If we consider that this track was ridged up by the contraction of the floor of the South Pacific, the fact would be accounted for that the various earth-waves of this plateau—for example, the Southern Alps of New Zealand—all curl, or have their steepest face towards Australia; and this argument, if granted, would render more probable the former existence of a continuous land-surface from the Solomons to New Zealand. It would explain the undisturbed condition of the Australian Tertiary and Mesozoic beds, which escaped the shocks of contraction, as compared with those of New Zealand which received them. Holmes and Hinde assume from the contained sponge remains that the siliceous beds of Oamaru, New Zealand, were formed at depths of not less that 6,000 to 9,000 feet in Eocene seas[8]; while Guppy considers that since Post-tertiary times the Solomon Archipelago has been upheaved at least 12,000 feet.[9] The vertebrate fauna of the Solomons evinces a recent descent from Papuan types, apart from which it shares with the New Hebrides and New Caledonia that remarkable poverty of mammalian and reptilian forms so distinctive a trait of New Zealand. Besides this negative evidence may be adduced the instructive distribution of *Placostylus*. This large Bulimoid snail forms one of the most striking components of the snail fauna of New Zealand, Lord Howe Island, New Caledonia, the New Hebrides, the Solomons, and the Fijis. This distribution appears to be in no way connected with trade winds or ocean currents. On the supposition that the remoter colonies of *Placostylus* were seaborne emigrants, it would be difficult to explain how, when Lord Howe was populated from New Zealand

[7] Voyage of H.M.S. 'Challenger," narrative, vol. i., p. 519.

[8] *Journ. Linn. Soc.*, vol. xxiv., pp. 178 and 255.

[9] H. B. Guppy, "The Solomon Islands, their Geology, etc.," p. 126.

or New Calendonia, that the emigrants who had successfully traversed the wide and shallow portion of the Tasman Sea should be stayed by the narrower but profound waters from reaching Australia. Similarly, a narrow but a deep strait intervenes between Fiji with numerous species and Samoa with none, between the Solomons where the genus is well represented and the Louisiades where it is absent. The beaches of Queensland, as I can testify from personal observation, are strewn by a constant drift of pumice accompanied by coco-nuts and pearly nautilus shells. The coco-nuts might have floated from any tropical island in the Pacific, the nautilus shells are only derivable from the narrower limits of the Solomons, the Fijis, and the New Hebrides, while the pumice must almost certainly be the product of the active volcanoes of the New Hebridean group. It is therefore obvious that in the Western Pacific the chief route of drifting objects is that of the trade winds from N.E to S.W. To an objection that the range of *Placostylus* might be due to transmarine migration, it is a sufficient answer to observe that this genus extends beyond and across, but not parallel to, the direction of the principal agency of dispersal in this region. Both birds and plants contribute evidence of the homogeneity of the fauna and flora of these islands, and of their distinctness from those of Australia. Among the former may be quoted *Merula* and *Aplonis*, and especially the distribution, past and present, in Lord Howe, Norfolk Island, and New Zealand of *Nestor*, *Platycercus*, *Notornis*, and *Ocydromus*.[10] Of the botany of Lord Howe Island it is especially to be remarked that, whereas on Wallace's theory it should, lying nearest to Australia, be most akin to that region, yet "those typical Australian families the Leguminosæ and the Myrtaceæ are barely represented, whilst the Proteaceæ are said to be wholly wanting."[11]

Upon these grounds I conjecture that an ancient continent, separated on the west from Australia by the abysses of the Coral and of the Tasman Seas, is represented by the Solomons, the Fijis, the New Hebrides, New Caledonia, Lord Howe, and New Zealand, with its outlying islands—an area that I have elsewhere[12] proposed to call the Melanesian Plateau, and upon part of which Forbes has more recently conferred the name of Antarctica.[13]

In conclusion, I would contend that New Zealand is associated with the Solomons and the New Hebrides, firstly, as a member of their volcanic system; secondly, by community of fauna and flora; whereas to Australia it is related not at all physically, and to a foreign and intrusive element biologically; and that a theory which derives the fauna and flora of New Zealand primarily from these archipelagoes and remotely from New Guinea, necessitates fewer unproved assumptions than that which derives them from Australia.

C. HEDLEY.

[10] R. Etheridge, Jun., "Lord Howe Island, General Zoology," p. 13.
[11] R. Etheridge, Jun., *op. cit.*, p. 103.
[12] *Proc. Linn. Soc. N.S.W.* (2), vol. vii., p. 335.
[13] See NATURAL SCIENCE, vol. iii., p. 54.

III.

Recent Researches on the Fauna and Flora of Madagascar.

THE remarkable faunal riches of this African island have been chiefly made known in M. Grandidier's volumes, but there is still much material at Paris lost to science for want of someone energetic enough to work it out. Particularly so is this the case with the Mammalia, and the delay in publication is not only vexatious to the zoologist, but allows others to describe as new, recent accessions, of which specimens are probably buried in Grandidier's collections.[1] The practice, too, of issuing volumes of plates unaccompanied by descriptions, may, perhaps, be useful when dealing with the larger animals, but is absolutely valueless (even to secure priority) for insects or plants in which so much depends on microscopic characters, and can only lead to endless confusion in nomenclature.

The latest information about the Madagascar fauna as a whole may be found in chapter xix. of Wallace's "Island Life" (ed. 2, 1892); but since that time, thanks to the liberal-handedness of Mr. Grose Smith and Mr. Walter Rothschild, material as remarkable for its abundance as for the peculiar value of the specimens has been received in this country; nor must it be forgotten that other workers, such as the Rev. R. Baron and M. Gautier, are largely interesting themselves in the accumulation of material which, though less in quantity, is every bit as valuable as the collection made by Mr. Last. Indeed, one may safely say that any remains, whether fossil or recent, if properly localised and carefully packed, add new light to our imperfect knowledge of this great island.

MAMMALIA.—The remains of Hippopotamus from the Post-tertiary deposits of the island have been known since the time when Murchison exhibited to the Geological Society in 1833 a tusk and molar tooth from a conglomerate, some 30 miles from Antananarivo. Further remains of what may be considered to be the same species were described by Grandidier and Milne-Edwards in 1868 as *H. lemerlei*. Portions of the skull of another individual of possibly the same

[1] We understand that a monograph on the genus Lemur, by Milne-Edwards and Filhol may shortly be expected, accompanied by 60 anatomical plates, when we hope the plates issued in 1890 will be properly described.

species have recently been received in London, and will form the subject of a paper by Dr. Forsyth Major.

In June last, Dr. Major read before the Royal Society a paper on an extinct Lemuroid from Ambolisatra, a slightly imperfect skull—with a right and left mandibular ramus—of which has been sent to this country by Mr. Rothschild's collector, Mr. Last. This skull, belonging to an animal approximately three times the size of any described Lemuroid, has been named *Megaladapis madagascariensis*. It is characterised by an enormous lateral development of the anterior inter-orbital portion of the frontals; a comparatively narrow and elongated post-orbital frontal region, separated by a slight contraction from the equally narrow parietal region; a thick and flattened sagittal, and an equally strongly developed occipital crest. The zygomatic arch is high and projects moderately outwards. The general appearance of the skull and teeth indicate an old individual. The brain-case is comparatively small in size, and is viewed by Dr. Major as a degenerate feature, and he anticipates that in young specimens the cranium would be more rounded in the cerebral region, more voluminous, and the facial portion much shortened.

Evidence has also been forthcoming of the former existence of a ruminant, but the remains are too imperfect to permit of identification. The colossal hippopotamus and rhinoceros (six times the size of an elephant) of M. Hamelin (*Standard*, 25 July) are imaginative, and the reported destructive powers of the *Cryptoprocta*, which attains a size about equal to that of a lynx, is, we venture to think, somewhat exaggerated.

Aves.—Many remains of *Æpyornis* have been received during recent months, some of which—sent to Mr. Rothschild, and exhibited by him at the Zoological Society—far exceed in size any specimens previously recorded. An egg of this bird was sold in a London sale-room a few months ago in a perfect condition, realising the sum of sixty-seven pounds, which may be considered as quite a fair price. The most recent information on the genus will be found in Burckhardt's paper on some remains which have reached Berlin from Sirabé, and here the pelvis, the upper portion of the metatarsus, and the immature metatarsus have been made known for the first time. A metatarsal of a large eagle has been found with the remains of hippopotamus at Ambolisatra.

Reptilia.—In the May number of the *Geological Magazine*, Mr. R. B. Newton described and figured portions of a rostrum and mandible of a crocodile from the Jurassic rocks of Andranosamonta. This fossil, which was obtained by the Rev. R. Baron, has been named *Steneosaurus baroni*. It was associated with mollusca, referable to *Mytilus* (near *M. tigrensis* from Abyssinia), *Modiola*, *Perna*, and *Trochactæonina*; and since the occurrence of this genus was, in our imperfect knowledge, limited to European areas, the discovery is of the highest importance in questions of geographical distribution.

Remains of the huge land tortoises, named by Vaillant *Testudo grandidieri*, have been lately received, and two fine specimens have been placed in the Palæontological galleries of the British Museum.

In Mr. G. A. Boulenger's "Catalogue of Snakes in the British Museum" (Natural History), vol. i. (1893), information will be found regarding the Madagascar forms; and the book itself will be found invaluable to anyone studying Ophidia.

PISCES.—Dr. Sauvage published in 1891[2] the description of the fishes from Grandidier's collection, and this text, with the two volumes of plates issued in 1887 and 1888, completes our present knowledge of the group.

MOLLUSCA.—The Rev. A. H. Cooke, dealing with the distribution of the mollusca in the Malagasy Region, notes the great development of the carnivorous land mollusca, the occurrence of large numbers of true Helicidæ of great size and beauty, and the prominence of the genus *Cyclostoma*. The molluscan fauna of Madagascar, even as imperfectly known, appears to possess sufficient individuality to separate it off distinctly from that of the main-land. The Helicidæ are quite peculiar, not being found at all in the Mascarenes, Seychelles, or Comoros. They seem to be rather related to the *Acavi* of Ceylon and the *Pandæ* of N.E. Australia. Fifty-four species of *Cyclostoma* are known from the group, but are not confined to Madagascar, being distributed over Comoros, Seychelles, Mauritius, and Bourbon as well. The African *Bulimini* are represented by two species, but *Achatina* is scarce. Two groups of Bulimini (*Leucotænia* and *Clavator*) are peculiar. A single species of *Kaliella*, identical with a common Indian form, is also recorded. In the fresh-water forms unmistakable traces of Indian relationships are found. There are two species of *Paludomus*; *Bithynia* is recorded; several of the *Melaniæ* are of a type common in the Indo-Malay Region, while the *Melanatriæ*, quite peculiar to Madagascar, have their nearest affinities in Ceylon or East India. Not a single one of the characteristic African fresh-water bivalves has yet been found in Madagascar. Several African genera of gasteropoda occur, and indicate, in common with the land mollusca, as Mr. Cooke points out, that the land connection of Madagascar with Africa must have taken place, but that it occurred at an immeasurably remote period. References to recent literature are given in Mr. Cooke's paper.

INSECTA.—Since the publication of the Lepidoptera (1887) from Grandidier's collections, there have been issued three parts of his great work on Madagascar, devoted to the Hymenoptera. These contain both text and plates. A second series of plates of the Coleoptera was issued in 1890, but, in the absence of the text, the new species can only be considered in the light of manuscript names.

FLORA.—The latest information respecting the botany of Mada-

[2] NATURAL SCIENCE, vol. i., p. 152.

gascar will be found in the *Journal of the Linnean Society* for 1890. In this volume Mr. Baron gives a general sketch of the flora, dividing the island into regions, and treating of the geographical distribution. The affinities of the plants with those of America, the fruits, cereals, and vegetables, garden trees and shrubs, are all treated of by Mr. Baron, who also gives an appendix of introduced plants. Mr. J. G. Baker contributes to the same volume descriptions of 160 new species collected by Mr. Baron, of which four belong to new genera. A fourth part of Grandidier's great work, dealing with the plants, was issued in 1892, but the foolish practice of issuing plates without text makes this publication of little value at present.

The Mosses and Hepatics are treated of by Stephani, who publishes technical descriptions, with figures of new species.

Geology.—The geology of Madagascar is best known from the researches of Cortese and of Baron. Cortese supplies a map of the whole island, while Baron deals only with the northern half. Roughly speaking, a line drawn from north to south, and dividing the island longitudinally, shows a granitic and a volcanic area on the right side, and a Secondary, Tertiary, and Recent area on the left side. The crystalline area consists of granite, gneiss, syenite, diorites, amphibolites, and basalts; the sedimentary series, by their included fossils, representing the Jurassic, Upper and Middle Cretaceous, Nummulitic series, and Recent deposits. Much blown sand occurs round the coast, and numerous old lake basins are found, one of which, according to Mr. Baron's map, must have been nearly 300 miles along. It is to these old lake basins that we mainly look for evidences of the ancient fauna of Madagascar. The petrological characters of the rocks have been described by Cortese, while Hatch has dealt with those specimens brought to England by Baron. For general information on the fossil fauna, see Newton's two papers, quoted below, with the references therein contained; and a paper of Stanislas Meunier in *La Naturaliste*, for Aug. 1, 1893. In this last paper figures are given of several Cretaceous oysters from Mahamayo, recently received from M. Gautier, and some general notes on Malagasy geology are appended.

Geographical and Zoological Relationships.—The observations of Mr. H. O. Forbes, which include Madagascar, and of which the essence was given in this Journal for July, may be compared with those of Rütimeyer "Ueber die Herkunft unserer Thierwelt" (4to, Basel und Genf, 1867), and with those of Emile Blanchard, "Les preuves de l'effondrement d'un continent Austral pendant l'âge moderne de la terre" (*Comptes Rendus*, vol. xciv., 1882, pp. 386 and 395); while much general and particular information will be found in Dr. Blanford's address to the Geological Society, and in his review of Boulenger's "Catalogue of Snakes," in *Nature*, Aug. 3, 1893.

Hans Gadow, in the last number of Bronn's "Klassen und Ordnungen des Thierreichs" (vol. vi., pt. 4, 1893), has some observations on the geographical distribution of the Ratite and rail-like birds,

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C. DAVIES SHERBORN.

IV.

The Interlocking of the Barbs of Feathers.

THE exquisitely beautiful, and remarkably modified epidermal products, which we know as feathers, have probably received more attention than any other form of clothing in the animal kingdom. Science, Commerce, Poetry, Art, and Fashion have each in turn laid them under contribution, a fact which, it will be readily admitted, has been beneficial to all but those most immediately concerned—their possessors, the birds.

Now we have just implied that feathers are structures with which the world in general is tolerably familiar. To a certain extent this is true; nevertheless, I have ventured to present the readers of NATURAL SCIENCE with what I hope will prove a concise account of the mechanism, by which the characteristic unity and elasticity of the vane of a typically perfect feather is secured, believing that this will be received not altogether as stale news. I wish it, however, to be distinctly understood that, for the most part, what I have to say will be found little else than a restatement of a tale already told, though in one or two somewhat minute points my claim to originality will, I believe, be allowed to pass unchallenged.

Before, however, proceeding to the more special subject of this article, it may be well to direct attention briefly to the accompanying diagram of a feather. (Fig. 1.) There may be noted (1) the strong pliant *scapus* or stem (*Sc.*), divided for convenience sake into two parts, (α) a *rhachis* (R_1) or distal, and (β) a *calamus* (C.)—commonly called the quill—or proximal portion, and (2) the *rami* or *barbs* (R^1), lanceolate processes, seated in two rows, one on either side of the rhachis, and pointing to the tip of the feather. The interstices between the rami, it will be observed, are filled by exceedingly numerous and very small processes, looking to the unaided eye in the natural feather more like bristles, perhaps, than anything else; these are the *radii* or *barbules* (R.), and it is mainly with these that we have to deal in explaining the structure of the *vexillum* (V.) of a feather. The vexillum, it must be remarked, is the compact, well-knit, elastic fringe or web ("beard" as it is called in German), composed of rami and radii interlocked in a manner to be described presently.

It has just been remarked that the interstices between the rami

are filled up by exceedingly small processes called radii; of these, there will be found two rows to every ramus, one directed towards the tip of the feather—the distal series—and one looking towards its outer or inner edge, as the case may be, and neither to the tip, nor the base of the feather, as has been described. Now if we press a sharp knife along the edges of one of these barbs, we shall succeed in getting a number of radii of two quite distinct varieties, though they have this much in common, both may be likened to lamellæ which have undergone the process familiarly known as "warping," in a longitudinal direction. Those of the distal series (Fig. 4) are found to be lamellate for about half their length, when the lamella might be imagined to have been cut up into a number of long, slender, filiform processes, some of which are hooked at the tip. If hooked, they are called *hamuli*, otherwise they are commonly known as *cilia*. The hamuli are confined to the ventral edge of the radius, but the cilia may occur on both dorsal and ventral edges.

The radii of the proximal series will be found, in addition to the warping, to have their upper edges folded somewhat sharply over towards the hollow surface, as in Fig. 3. After rather more than half of its length, this edge gives place to a series of notches, which soon—rapidly diminishing in size—blend with the almost and often quite filiform tip, into which the distal moieties of these radii are produced. It must here be noticed that, in the region of the notched edge, the radii are bent at a slight angle upon themselves, the bent ends being closely approximated, as shown in the figure.

So much for the general conformation of the radii. What is the reason for this warping? and what is the actual method of interlocking? are questions that my readers will here naturally ask.

Probably a glance at Fig. 2 will explain more than could ever be done by mere description. Here we have a semi-diagrammatic representation of an oblique section across two rami (R), taken parallel with the distal radii, or what amounts to the same thing, parallel with the shaft of the feather (F). As I have hinted, the figure tells its own story, but I should like to draw attention to the following details.

It will be observed in Fig. 2 that the proximal halves of the distal radii appear as flattened, overlapping laminæ, which at about the middle of their length suffer a sudden twist in a vertical direction, then breaking up into the hooklets and cilia previously described. These hooklets, it will be seen, are thrust down between the scroll-like upper margins of the proximal series, between which there is only just room enough to allow the hooklets to pass. The proximal radii are somewhat puzzling, perhaps, because seen in section; this, however, is unavoidable, as a reference to the diagram, Fig. 1, will, I think, make clear. The true relation of this series having been grasped, there is little else to note for the moment, except the folded upper edge, whose function is obviously to afford a secure hold to the hooklets, just

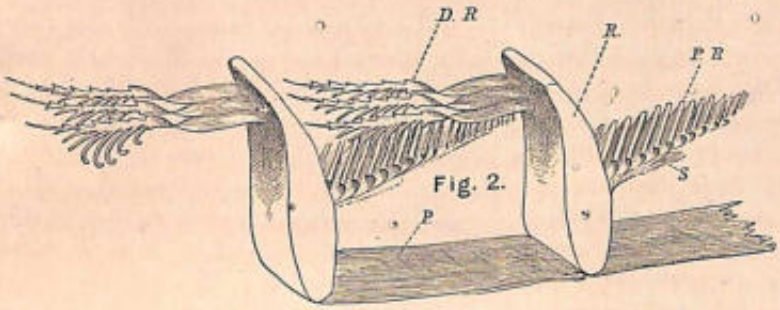

Fig. 2.

Section through 2 rows parallel to the distal Radii.

D R. Distal Radii P R Proximal Radii.

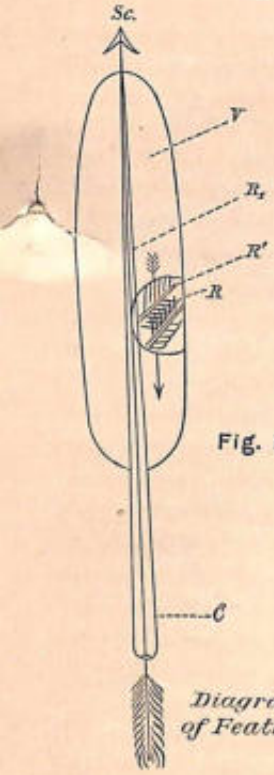

Fig. 1.

Diagram of Feather.

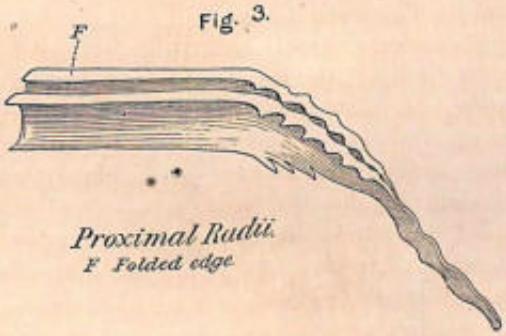

Fig. 3.

Proximal Radii.
F Folded edge

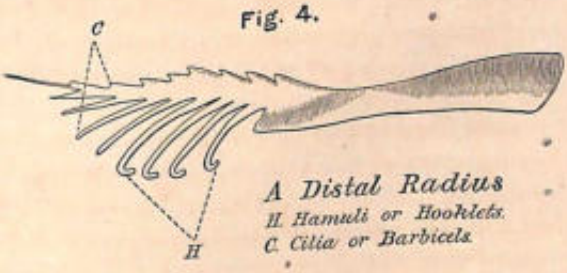

Fig. 4.

A Distal Radius
H. Hamuli or Hooklets.
C. Cilia or Barbicels.

Section through the bases of 3 Distal Radii

Fig. 5.

Fig. 6.

Magnified Section through a Hooklet and folded edge of a Proximal Radius.

F. Folded edge.
H. Hooklet.

DIAGRAMS OF FEATHER-BARBS.

described as being thrust down between them; and the formation of comparatively deep channels by the approximation of their lower edges. These channels are not maintained far, if at all, past the most proximal hooklet; from this point, it will be seen, these sectionised radii diminish in size, terminating close against the surface of the next ramus.[1] Now it only remains for the reader to draw upon his imagination, to see at once that a comparatively severe strain would have to be exerted upon the rami to pull them apart, and if he will only make the experiment on some large quill feather, he will be amply satisfied; then let him sever a few rami at their base, and see how easy it is to separate them by pulling the ends in opposite directions, the hooklets, of course, gliding along the grooved edges.

Now a word as to the "warping." The radii of both distal and proximal series are, it will be remembered, longitudinally curved, or "warped," as I have termed it, but in Fig. 2 the radii have the appearance of *flattened* not curved laminæ! How is this? Well, I have chosen to give the appearance they present under the microscope when seen *in situ*, rather than avail myself of the licence allowed me in adopting a semi-diagrammatic figure, for the sake of those who take up this work for the first time; for unless seen in section, it is difficult, if not impossible, to consider these anything but what they appear—flattened lamellæ, seated obliquely along the ramus, and even examination under high powers does not reveal the true state of things. Their actual form appears in section as in Fig. 5, which represents three radii cut off close to their base.

As touching the "channels" of the proximal series, what we have now to do is not so much to tender a reason for their formation—this being rather too presumptuous, as to offer a suggestion to that end. The first thing that will occur to the thoughtful reader is, if these edges were free, and directed straight downwards, might they not be a source of danger to the hooklets, inasmuch as the latter would perhaps frequently catch their lower instead of their upper edges; while the present arrangement suggests a twofold purpose: firstly, to afford a wind-proof surface for the downstroke of the wing (in the case of the remiges and flight feathers), thus preventing the displacement of the hooklets by the upward rush of air; and, secondly, to conduce to that marvellous elasticity of the feather we paused to admire in the earlier part of this paper, the hooklets gliding freely over the smooth, hollow surface, yet with difficulty letting go their hold. Surely, there seems to be good reason for these views. At any rate, just such an interpretation as this seems to find favour with so good an authority as Dr. Gadow. I find on reference to his most valuable work the following remarks, which, by the way, however,

[1] These diminished portions, it should be noted, are the distal ends of radii arising nearer the proximal end of the ramus. A moment's reflection will show—bearing in mind their oblique direction—how, that being placed one in front of the other, the sections will gradually decrease.

leave us in a little uncertainty as to whether the full extent of the curvature of the radii is appreciated or not. Thus (quoting Klee) he says, "Ihr *oberer Rand*[2] ist einfach umgeschlagen, wodurch längs dieses Randes eine vollkommene Rinne zu Stande kommt, die unter dem Mikroskop allerdings den Eindruck eines verdickten Randes macht." And again, speaking of the elasticity of the feather, he says, "Zugleich ist die Fahne in höchsten Grade elastisch, nicht nur durch die Elasticität aller einzelnen Federtheile, sondern weil die Hacken auf den glatten Strahlen *Kanten*[2] hin—und hergleichen können."

The relative position of the radii, like their form, varies. Both distal and proximal rows may arise from what would otherwise be the ridge of the ramus, or the distal radii may arise from the upper border of the ramus, while the proximal may run along quite near the lower border. Frequently the distal radii are overhung by a projecting ledge, formed by the much compressed ridge of the ramus, and between these every possible gradation may be found, according as the part examined be from the primaries, secondaries, or coverts, or from the inner or outer margins of the vexillum.

Along the lower edges of the rami in Fig. 2 will be noticed a thin transparent band (P) of a width corresponding to the section. This I recently found in the remiges of a Teal, *Querquedula crecca*. The free edge may be either closely approximated to, or even fused with, the lower edge of the next ramus. To the naked eye, this appears along the side of the shaft as a narrow band of silvery whiteness, and very smooth, but I am at a loss to account for its function, and do not care to hazard a guess. I have indicated the presence of a similar band under the proximal radii; it seems, however, to occur but rarely. Mr. R. S. Wray, to whom we shall have occasion to refer presently, makes mention of such a band in his paper, shortly to be noticed.

I propose now to present a sort of census of opinion as to how the "Interlocking of the barbs of a Feather" was obtained; but, to be brief, it will be well to confine attention to the descriptions of some three or four writers whom we may consider to have been more or less well-qualified to speak on this subject. This will carry us back some sixty years, to the time of the great Nitzsch, the father of the science of Pterylography. His description was fairly accurate; the wonder is it was not quite so. He perceived that the radii could be divided into two series—an anterior and a posterior, as he called them, but he failed in his endeavour to explain their mutual relations, considering that the hamuli of the anterior radii were for the purpose of catching into "little pits" prepared for their reception in the proximal radii. Burmeister, editing Nitzsch's work a few years later, exploded this theory, and gives essentially a correct explanation. He says, "I have noticed in all parts a strongly-thickened, superior margin of such a size that the hooklet can just grasp it. It is behind this margin . . . . that the hooklets take hold."

[2] The italics are mine.

Owen, writing some thirty years later, gives a much less truthful description. He says, "The barbules arising from the upper side of the barb, or that next the extremity of the feather, are curved downward or toward the internal surface of the shaft; those which arise from the underside of the barb are curved in the contrary direction; so that the two adjoining series of hooked barbules lock into one another in a manner which has been compared to the fastening of a latch of a door into the catch of a door-post."

Huxley's description is as follows:—"The barbules may be laterally serrated and terminated by little hooks which interlock with the *hooks*[3] of the opposed barbules."

The last and most authoritative account is that of the late Mr. R. S. Wray. Practically, his paper, short though it is, tells us all that is to be told; but the author presumably failed to appreciate, or missed entirely, one or two of those nice little differences that will so often escape the notice of the most painstaking workers, of whom he was one. Possibly "'tis a rule in Nature," in order that those who come after us may have something left to do.

As just stated, Wray's figures and descriptions may be held in all but a few details to be correct. Exception is taken to his interpretation of the proximal radii. These he describes as ". . . thin laminæ, with a thickened upper edge forming a small ledge or kind of flange"; and continues, "If a single barb be examined, the proximal barbules appear . . . at their proximal end to join on to an edge-piece running parallel to the barb . . . this edge is seen to be formed by the attenuated halves of these barbules overlapping one another, and being turned at a considerable angle upon the other half." On referring to his figures, however, we find these barbules represented as perfectly flat laminæ, vertically inserted by their base along the ramus, and with a slight "flange" running along the upper edge, but with no trace of the "attenuated halves overlapping one another." True, this is a diagram, but surely it would have been more correct to figure the specimen, since the essence indeed of a diagram is to give us *the* points of a structure, divested of all superfluous detail. So far as my experience goes, this edge-piece occurs but rarely, neither do I believe it is formed as Wray imagined it, since, in the only case in which I ever found it, the radii were normally related, "the edge-piece," though closely approximated to, being yet quite distinct from them. While this was in the Press I detected a precisely similar flap under the *distal* radii. Again, as touching the notches, these are accounted for by supposing that they are for the purpose of allowing the distal portion of the radius to be turned upon itself without "crumpling." Would it not seem more probable that these notches are—if we are to explain their presence at all—rather to keep the edges of these radii sufficiently far apart to allow not only of the passage of the hooklets, but to prevent their

[3] The italics are mine.

being injured by chafing when in position. The curvature does not seem sufficiently great to account for these notches in the first place, and in the second, seeing that the curve is not delayed till the radius has completed its development, and is then suddenly bent, but that the curvature is synchronous with growth, a far greater inflection could have taken place without any special arrangements, as witness a thousand instances in Nature. Lastly, I would point out that the proximal radii, instead of curving abruptly just at the point where the laminæ of the distal radii break up into hooklets, as shown in Wray's figure, are actually not only curved less, but, as already pointed out, extend right across to the next ramus. I have purposely dwelt upon the shortcomings of these figures, because they are rapidly making their appearance into the various text-books. Though this is a fact to be deplored, it may be readily understood. I suppose, nowadays, if one finds himself engaged in bookmaking—of the more honourable kind, I mean—he must add yet more drudgery to his labours, by verifying every statement to its minutest details, when probably, by the time he has finished the last chapter, he will have to begin again and bring his work "up-to-date."

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W. P. Pycraft.

V.

The Lucernarians as Degenerate Scyphomedusæ. A Note upon the Phylogeny of the Order.

PRELIMINARY.

BEFORE entering upon discussion of the problem of the descent of the Lucernariæ, it may be well to set out for the guidance of the general reader the more important facts already known concerning the class to which they belong.

The Hydrozoa, in which together with the Zoophytes, Jelly Fishes, and Siphonophores they find place, is conveniently grouped in two divisions (2). The first includes the Lucernarians themselves, together with those fleshy Medusæ or jelly fishes without velum (hence sometimes called "acraspedote medusæ") and with eight marginal sensory organs (tentaculocysts) so well-known under the forms of *Aurelia*, *Pelagia* and *Rhizostoma* (termed collectively Discomedusæ), and finally the Conomedusæ, a small group with four tentaculocysts and with a velum—*Charybdæa* being the type. Such sub-class comprising these orders constitutes the Scyphomedusæ, so called on account of the general presence of a particular stage in the life-history of typical species. Normally this is as follows:—A ciliated embryo (proceeding from the sexual or medusa stage) after a short period of free-swimming pelagic existence, settles down and becomes attached to some stationary object by one end. A mouth forms at the other, around which sixteen finger-like tentacles appear; and at the same time the lower part of the body becomes drawn out into a stalk.

This elegant form, $\frac{1}{16}$ to $\frac{1}{8}$ inch in height, is the *scyphistoma* stage, and is what is sometimes termed the hydriform phase, from its general resemblance to the fresh-water polyp, *Hydra*. Soon, as growth goes on, food being captured by the transient tentacles, transverse constrictions—*strobilation*—cut the body into a rouleau of discs, appearing thus as a tiny pile of sculptured plates. Each disc has eight arms, bearing each a tiny tentaculocyst at the tip. This is the *strobila* condition, and the discs composing it, as the constrictions become deeper, are, one by one, broken off and sent out on a free-moving life. Known at this period as *ephyræ*, these organisms rapidly take on the form of ordinary jelly-fishes, develop sexual organs, and

send out swarms of ciliated embryos to go through the same strange cycle of life.

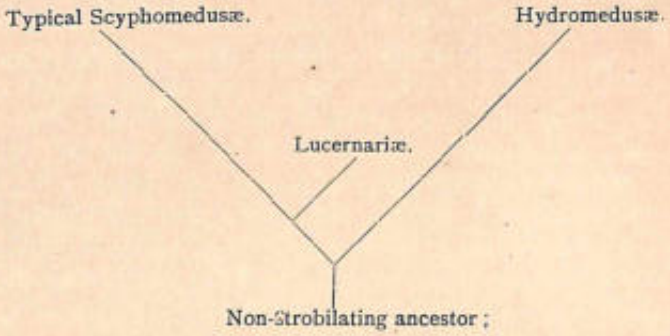
The second great division of the Hydrozoa, viz., the Hydromedusæ, is, by most authorities, made to include both those fixed colonial-living polyp forms, known generally as the Hydroid Zoophytes, as well as the highly-specialised and complex floating siphonophores exemplified by *Physalia*, the Portuguese man-o'-war. The former—the Hydroid Zoophytes—which alone we need mention here, have a life-cycle closely akin to that of the Scyphomedusæ, the main difference being that the fixed stage—the hydriform—does not give origin to medusæ by transverse constriction, but, by oft-repeated budding, produces usually large tree-like colonies. A medusiform sexual stage is, however, present, though its importance is overshadowed and masked by the great development of the hydriform. Thus such a typical Zoophyte as *Obelia gelatinosa* forms, by repeated budding from a ciliated embryo which has become fixed, a large colonial arborescent community of many hydra-like and purely nutritive polyps. Later on, certain modified organs produce, *by budding*, numerous tiny medusæ, which break loose, and float away as beautiful glassy, pulsating bells. These differ from the medusæ of the Scyphomedusæ in having a muscular membrane—*velum*—closing, all but for a central opening, the mouth of the bell, hence the term "craspedote medusæ." This stage is sexual, and gives rise to free ciliated embryos.

The Scyphomedusæ have internal or gastral filaments, and their hydriform stage is simple; the Hydromedusæ have no gastral filaments, and their hydriform stage is colonial and frequently complex. The Lucernarians possess the two former characters, and for these and some less easily stated reasons, their intimate relationship with typical Scyphomedusæ is universally admitted. They, however, differ from both divisions in that their life-cycle is not complicated. The adult hydriform animal gives rise directly to ciliated embryos, which settle down and become of the adult form without metamorphosis.

Several species of Lucernarians are found in British seas. The most common in the South (English Channel) is *Haliclystus octoradiatus*, a most elegant, short-stalked, bell-shaped species found almost invariably attached by an adhesive disc to blades of the sea-grass (*Zostera*) close to low-water mark. The edge of the bell is drawn out into eight points, each bearing a cluster of short clubbed tentacles, well provided with stinging cells. Midway between each two groups of tentacles is a hollow papilla, the "colleto-cystophore," usually believed to be the equivalent or homologue of the tentaculocysts of the medusæ. Normally, these are eight. A like number of genital bands extend from the mouth into each of the marginal points. In general outline *Haliclystus* resembles a shortly pedicelled inverted cone with scolloped margin worked into eight projecting tentacle-armed points. The size frequently reaches 1 in. in height, by 1¼ in. across.

NEW CONCLUSIONS.

The position assigned to the Lucernariæ (=Calycozoa) has long been that they represent, comparatively but slightly modified, a simply organised ancestral form of those Scyphomedusæ called by Haeckel and Lankester the Discomedusæ. This view was embodied by L. Agassiz in the comparison of the relation borne by the order to the Discomedusæ, to that which the stalked Crinoids (primitive form) bear to the free-moving Comatulidæ. This meaning is expressed in the following "tree," which gives the accepted view:—



Such pedigree has the excellent advantage of simplicity—a lowly-organised animal being placed as the common ancestor both of complex and of, presumably, little changed, simply organised descendants.

Quite recently some slight observations (4), which I recorded in the July number of NATURAL SCIENCE (p. 33), decided me to examine afresh into the truth of this conclusion, with the result that new evidence points distinctly to a completely different phylogenetic arrangement, whereby the Lucernariæ would appear to owe origin to an ancestor having as complex a life-cycle as, say, such a typical Discomedusid as *Aurelia* has at the present day, and *not* to a simple non-strobilating scyphistomatous type of animal.

This origin, I believe, came about by the premature development of genital products during the scyphistoma stage of the ancestral form. When such hastening of events took place—ova and sperm being formed and set free prior to strobilation—the latter phase would in consequence be rendered abortive, no ephyræ would be thrown off, and no medusid stage would occur. Such a change in the life-cycle would clearly be advantageous, as under sessile conditions of life competition with free-swimming medusæ would be avoided, and thus the variation resulting from premature sexual maturity would tend to be repeated, and finally to be perpetuated. This same avoidance of competition with the Discomedusæ accounts for the non-operation of that most general law of Natural Selection—that when through advantageous variation a new species is evolved, the old or stock species is forced from the field and becomes

extinct, since it cannot compete for long *in the same sphere* with the better equipped varietal form.

The clue that led me to this conclusion as to the phylogeny of the Lucernariæ was the fact of finding great and remarkable variation or inconstancy in *Haliclystus octoradiatus* in those bodies termed colleto-cystophores, and which are generally supposed to be homologous with the marginal organs—tentaculocysts, etc.—of the medusæ. Such variation consists, as I have recorded elsewhere, (*a*) in duplication of the organ, (*b*) in the frequent suppression of one or more in the same animal, (*c*) in the still more frequent reversion in structure to that of the form of the normal papillæ or tentacles as seen in the marginal clusters of these appendages. This gives us the fixed fact of *great variability in the marginal bodies*. Now inconstant presence and frequent reversion are known to be the accompaniments of a vestigial organ; hence we conclude that these bodies are vestigial, consequently have been more important than they now are, and that the origin primitively was from marginal tentacles. As bearing upon this, it is important to remember that it is admitted generally that tentaculocysts arise from modification of ordinary tentacles.

These facts bring us now to the conclusion that *Haliclystus* is descended from an animal possessing a stage wherein certain marginal tentacles had become modified into highly important sensory organs, approaching, if indeed not almost similar to, the tentaculocysts of medusæ. Further, organs of this complex description can scarcely be imagined as in any way serviceable to a sedentary animal unprovided with powers of locomotion, whatever use such may subserve in a free-swimming organism, whether we follow the old theories adducing auditory and visual functions to these sense-organs, or follow Dr. Hurst's more probable theory as put forward recently (3). Sessile animals of the description of the Lucernariæ can neither follow prey nor avoid capture, and hence do not require auditory and visual organs, much less automatically-actuated direction-controlling bodies.

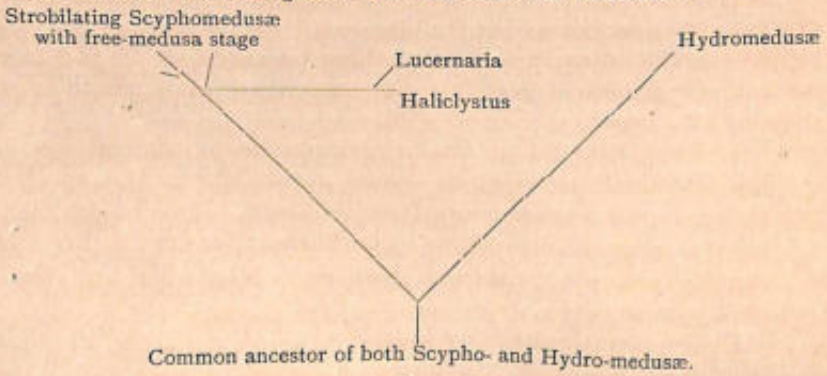
This brings me back to my initial proposition, that among the ancestors of *Haliclystus* was a form having a strobilation stage throwing off ephyræ, which became acraspedote medusæ provided with eight marginal sensory organs and with numerous tentacles separating these, approaching indeed such a form as the present-day *Aurelia*. Then, when the premature development of sexual glands occurred in the scyphistoma stage, as already mentioned, it is reasonable to infer that the other organs belonging normally to the fully-developed adult form (acraspedote medusa) would also appear. Chief among these would be the marginal sensory bodies; but these would occupy a very different platform to the sexual glands. The latter are necessary to the existence of the animal; the former—under the changed conditions of *sexual maturity in sessile position*—would be of no more use, as pointed out above. The courses open to them must either be, (*a*) reversion to the original form, *i.e.*, that of marginal tentacles, (*b*)

adoption of a new function, or, (*c*) disappearance. As we have seen, the first and third of these alternatives take place frequently in *Haliclystus*, which, perhaps, more than any other Lucernarian remains most closely related to the old form.

In some species, however, *e.g.*, *Lucernaria campanulata*, the third alternative has been unmistakably adopted, accompanied, too, by the very significant fact that, though absent in the adult, yet the marginal bodies do exist for a short period in an early stage of life. (It may be that the genus comprising this species is directly descended from *Haliclystus*.)

As to the second alternative—the adoption of a new function—probably in *Haliclystus* there is indeed a secondary one coming, or come, into operation. In this genus, the marginal bodies are used, apparently, as accessory to the ordinary tentacles in holding on to any prey in course of capture. Thus, if with a pencil point one of the marginal bodies be touched, it adheres strongly to the pencil, and with very perceptible power. Coincident with the loss of sensory organs, there has been decay of the whole nervous system, the purpose for which departed with the cessation of rhythmic motion and the lost utility of the tentaculocysts.

The view I have above put forward as to the relationship and descent of the Lucernariæ is expressed in "tree" form as follows, and shows clearly on comparison with the diagram given at the commencement wherein the divergence from the old view consists :—



Dr. Hurst has arrived independently and simultaneously at conclusions which, I believe, are practically the same as above put forward, but as I know but the outlines of his reasoning, the reader is referred to his article on the subject following this (pp. 209–217).

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JAMES HORNELL.

VI.

Biological Theories.

VI.—THE PHYLOGENY OF LUCERNARIANS.

THE following speculation is now published, not only on account of the intrinsic interest of the question suggested by the title, but also as an illustration of the application of various views, already published in this series of essays, to the solution of one definite problem; and I have deliberately chosen a case in which the conclusion arrived at is the direct opposite (or converse) of the view to be found in most modern zoological works which treat of the subject. I have made this selection in order that anyone who is prepared to give my views a fair test may do so by comparison of my result with that reached by the usual methods and on the usual assumptions.

As it is necessary, in the course of the argument, to make use of at least six views not as yet familiar to zoologists in general, I will begin by enumerating those views, giving references to the papers in the earlier volumes of NATURAL SCIENCE where fuller accounts of them may be found.

1. Heredity is a mere similarity or likeness among corresponding individuals belonging to several generations of one species. It is not a force or other objective influence, simple or complex, capable of producing effects. The likeness between father and son is not the effect of heredity; it is heredity itself (vol. i., p. 502).

2. This similarity among individuals, or if we prefer to call it so, this constancy of structure in successive generations, is the result of Natural Selection. It is not simply an effect produced once for all, but is dependent upon the continued action of Natural (or other) Selection for its maintenance. Natural Selection under constant environmental conditions tends steadily towards the production of a constant structure, which, when it has become almost constant, we speak of as the specific structure, and the constancy of which may be called "heredity," though the abuse of this term makes it safer to use only the term "constancy" (vol. i., p. 578).

3. Natural Selection is therefore, in a certain sense, independent

of "heredity," and is older than "heredity," and has led to the evolution of "heredity" from "variability," or to adopt the safer terms, the evolution of constancy from inconstancy. Inconstancy is the older: constancy which is never complete is only inconstancy kept within narrow limits by the continued action of Natural Selection. The assumption that "heredity" occurred *ab initio*, and that "variation" has been produced by various external agents—Lamarckian and others—is a mistake (vol. i., p. 580).

4. The ontogeny of an individual, though in some cases it presents a marked resemblance to the real or supposed phylogeny of the species, with reference to certain characters, usually shows only a very slight such resemblance, and may show none at all. We are, therefore, not justified in relying upon it as a "record" of the phylogeny, even though we admit that the supposed record is highly imperfect. Von Baer's law is readily verified in some cases at least; and while it appears to contain all that is true in the "Recapitulation" Theory, it is contradictory to the rest of that theory (vol. ii., pp. 197 and 365, and especially 368).

5. The resemblance between an adult animal on one hand and the larva of another species on the other hand, while indicating a close relationship between the two species, does not show that the second is descended from the first. A stalked crinoid may resemble the immature form of a free-swimming one, and the latter may be actually descended from a form very like the former, but the resemblance in question does not *alone* justify this conclusion, inasmuch as the same resemblance might have arisen from a relationship the direct reverse of that supposed (*Locc. citt.*) The following essay treats of a case in which this reverse relationship appears to be very near the true one.

6. Tentaculocysts cannot subserve an auditory function; and such a function would be useless to a medusa if they could (vol. ii., p. 353, last paragraph); but they may and probably do serve a function of the utmost importance in the preservation of jelly fishes from destruction by oceanic waves (vol. ii., p. 421).

While writing the essay last referred to, the following question occurred to me with almost maddening persistency:—*Haliclystus has tentaculocysts or something of the kind and does not swim: can it then be that the function suggested is the true one, seeing that such organs are present also in an animal, to which such a function would be useless?*

It was not till I had answered that question in my own mind that I ventured to publish the above-mentioned view on the "true function of tentaculocysts." The reason for withholding my answer till now will appear later.

The structure of the marginal bodies of *Haliclystus* (*Lucernaria*) *octoradiatus* shows that they are unfitted for the performance of the

function which I have ascribed to tentaculocysts. In position, however, these bodies correspond exactly with the rudiments[1] of the tentaculocysts in a scyphistoma.

The function of the tentaculocyst-rudiment, supposing it to have only one function of importance, is to develop into a tentaculocyst. The development in such forms as I have examined (and have failed to identify!) is direct, and exhibits nothing which justifies the assumption that they have any specially "larval" function, or function not connected with the production of the adult organ: and though we may be unable to say at what precise stage they come to be capable of performing the "tentaculocyst-function," I think we are justified in believing that the efficient performance of that function after the organ is fully developed may reasonably be regarded as the chief factor which has determined the evolution of these organs under the guidance of Natural Selection.

Considering the resemblance in form, and, to a certain extent, in structure, and the identity in position of these rudiments on one hand, and of the marginal bodies of *Haliclystus* on the other, a community of origin is hardly doubtful; in other words, the influences (or some of them) which have determined, or helped to determine, the presence and the structure of either may reasonably be supposed to have played a part in the case of the other also.

According to the usual view, the Lucernarians are the unmodified, or little-modified, descendants of the common ancestor of Scyphomedusæ in general. This view further suggests that *Lucernaria* is the "most primitive," and that *Haliclystus* is "higher" or more modified; and that all free-swimming medusæ (Scyphomedusæ only, I mean, of course) are descendants of forms which, like *Haliclystus*, exhibited no "alternation of generations"—no strobilation, while remoter ancestors still were very similar indeed to *Lucernaria*. Such a view is an almost necessary consequence of the Recapitulation Theory.

For convenience, I will now use the names *Lucernaria* and *Haliclystus* a little loosely, so as to include all hypothetical ancestors possessing the distinctive characters of these genera.

The orthodox view just set forth brings us face to face with a problem which presents, I believe, insurmountable difficulties, *i.e.*, how to account for the evolution in *Lucernaria* of marginal bodies so as to give rise to *Haliclystus*.

We must, in attacking the problem, assume either—(1) that the

[1] I use the term "rudiment" in its true sense, and as equivalent to the German "Anlage." Use may justify a change in the meaning of a word, but the persistent misuse of the terms "rudiment" and "rudimentary," when "vestige" and "vestigial" are meant, does not appear to me to justify the change in this case, especially as we have, so far as I know, no other words to put in their place; for the German word "Anlage" is so utterly "un-English" that its adoption in English (or even American!) writings appears to be open to serious objection. I deem it best, therefore, to use "rudiment" for what it really means, and to ignore the prevalent misuse of the term, in the hope that it will soon die out.

bodies in question are useful, or (2) that the evolution has been independent of Natural Selection. Few will be willing to accept the second hypothesis—I least of all, for I believe more thoroughly in the adequacy of Natural Selection than even Mr. Wallace, to whom, with Darwin, we owe the theory itself.

If, however, we adopt the first hypothesis (provisionally), we must further suppose that a portion of the genus *Lucernaria* has been isolated—geographically or otherwise—and so protected from the action of the selection; otherwise the evolution of the new genus (*Haliclystus*) from the old one (*Lucernaria*), under the direction of Natural Selection, would, of course, involve the extinction of the old one.

Lucernaria and *Haliclystus* are, however, found in the same seas, though their geographical ranges may differ considerably. As to the geographical distribution of any one species of *Haliclystus* or of *Lucernaria*, I will not venture to speak, for I am not sure that we can yet say with certainty what are, and what are not, distinct species (or even genera) in this family. For present purposes it is sufficient that forms which have been called *Haliclystus*, and others which have been called *Lucernaria*, are found on both east and west coasts of the North Atlantic, as well as elsewhere, and that some zoologists consider that some of the American specimens in both genera are specifically identical with some of the European ones; and since the mode of life is the same in both, and both live on *Zostera*, and on red sea-weeds, just below low-water mark, there arises a very serious difficulty in the way of the assumption of isolation, either geographically or otherwise. Though the marginal bodies are, perhaps, not *very* complex bodies, yet, in comparison with the whole body of the animal, they are sufficiently complex to justify the assumption that Natural Selection could only determine their evolution during a very long and very severe struggle for existence and for multiplication.

The organs may give some slight advantage, or even a great advantage, to *Haliclystus*. They may serve, for instance, as "anchors" (Clark) (1); but that is not enough. If Natural Selection has caused the evolution of animals possessing those organs (*Haliclystus*) from animals not possessing them (*Lucernaria*), that means that the organs have at every stage in their evolution conferred such an advantage on their possessors as to enable them to vanquish, to starve out, to render extinct, the forms not possessing them, or possessing them only in a less perfect state of development. It means that the evolution of *Haliclystus* from *Lucernaria* would have exterminated the genus *Lucernaria* in the seas where this evolution occurred; and this the geographical distribution of the two genera emphatically contradicts.

We may therefore regard—nay, we are compelled to regard, the evolution of *Haliclystus* from *Lucernaria* as disproved.

From what, then, is *Haliclystus* descended?

I believe the clue to the solution of this problem is to be found in

a consideration of the evolution of tentacles, and of tentaculocysts, and of the marginal bodies of *Haliclystus*.

The evolution of the marginal bodies in *Lucernaria* offers the difficulties already considered, and, therefore, we may not take these bodies as the starting-point in speculating as to the origin of tentaculocysts. Before this procedure would be allowable, we must account for the origin of the marginal bodies themselves. As this origin is not obvious, we may consider the other organs, tentacles and tentaculocysts, first; and we will begin with tentacles, as these are the simpler. In doing so, however, it is necessary to bear constantly in mind that the more complex is not necessarily a modification of the simpler—the simpler may just as well be a modification of the more complex, or the two kinds of organs may even have been independently evolved.

The tentacle in its simplest form is a mere outgrowth of the body-wall, identical in histological structure with the adjacent portions of that body-wall. Almost in this simple form tentacles may be seen in *Hydra*, though *Hydra* is probably far from being a primitive form. A mere projection of the wall of the animal, provided it be contractile and suitably placed with reference to the "mouth," may well be regarded as useful in the prehension of food. Its usefulness would, moreover, be greater, within certain limits, the greater the length of the process. Every individual possessing such outgrowths would, therefore, be favoured thereby in the struggle for existence and multiplication; and this advantage would, moreover, always rest more especially with the individuals in which the processes were, within limits, the longest. Hence the evolution of tentacles from mere irregularities of the body-wall can be explained without any assumption beyond that of the operation of Natural Selection.

How a free-swimming medusa was evolved we are not now considering, and fortunately it is not necessary that we should answer the question at present. Suffice it to say that free-swimming medusæ exist, and that the evolution of their tentacles in the above-described way offers no difficulty.

With tentaculocysts the case is different. If we suppose them to be modified tentacles the difficulties vanish; otherwise the difficulties are, so far as I can see, insurmountable. The slow transformation of a *few* tentacles *out of many* so as to better perform a special function, performed, at best, only badly by a tentacle, leaving the remainder to carry on the original chief function of these organs, appears possible; for the chief changes required are the formation of a depression around the base of the tentacle and subsequently a shortening of the tentacle itself, and these offer no difficulty. Every stage of such an evolution provided only that the medusæ are fairly large, and becoming larger, fulfils all the requirements of the theory of Natural Selection; that is, the transformation is a gradual one, and every stage in it is an advance upon the previous one; or in other words, every supposed change in the evolution is a minute one; is one

which may well be supposed to occur "fortuitously"; and is one which confers an advantage upon the species undergoing the transformation—however numerous these species may be.

It is not necessary to go more into detail in this question, as the view here urged is, I believe, adopted almost unanimously: difference of opinion, if there be any, will probably be confined to the question of the possibility, or perhaps probability, of such a transformation occurring independently in several species.

There are, however, not a few who have only adopted this view as to the origin of tentaculocysts at a moment when they were not thinking of their homologues in a supposed *Haliclystus*-like ancestor. These, no doubt, will require that any hypothesis or conclusion as to the course of evolution of tentaculocysts shall regard the marginal bodies of *Haliclystus* (or something very like them) as having served as a stage in the evolution. To such I commend the following considerations.

(1) The evolution of marginal bodies in a Lucernarian *ab initio* offers difficulties already considered, and apparently insurmountable.

(2) The evolution of these bodies *directly from tentacles* in ancestors of Lucernarians offers difficulties no less formidable. Natural Selection will not on this assumption account for the evolution of two genera *Lucernaria* and *Haliclystus* in the same seas, and in like positions in those seas. If the marginal bodies are advantageous, then their evolution from tentacles would be accounted for easily enough, provided we had *Haliclystus* alone to deal with. The evolution of the two side by side negatives any such hypothesis.

We may therefore turn in another direction for the solution of our problem—there is only one left! The marginal bodies may be *vestiges* of the tentaculocyst-rudiments of ancestral scyphistomata.

This involves, of course, the supposition that *Haliclystus* is descended, not merely from scyphistomata, but from free-swimming medusæ.

The evolution of tentaculocysts in free-swimming medusæ I have explained. In that explanation is involved an explanation of the evolution of their rudiments in the young. The evolution of strobilating from non-strobilating medusan types appears to me to offer no serious difficulty, and I leave this portion of my argument (which seems almost obvious) to be filled in by my readers. If it should be called for I will publish it.

Given, then, that there has been evolved a type of medusa in which the formation of a scyphistoma, strobilation, and the separation of *Ephyræ* provided with rudimentary tentaculocysts have been phases in the life-history, the origin of Lucernarians from such a type offers no further difficulty of great magnitude. (*Re* the meaning of "rudimentary," *see* footnote, p. 211.)

On this hypothesis the community of origin or "homogeny" (as Professor Lankester has well called it) of the marginal bodies and the

tentaculocysts is not a mere community of origin; the marginal bodies are vestiges of the rudiments of tentaculocysts, *i.e.*, they are vestiges of tentaculocysts themselves; but what is their function? Are they *mere* vestiges, or are they organs which have undergone a change of function?

Again, the coexistence of two genera in the same localities and with the same mode of life shows the way to the answer. They are mere vestiges. The ancestral function is impossible: they cannot serve as rudiments whose function is to develop into organs (tentaculocysts) useful in a free-swimming phase, because there is no such phase in the life-history. They cannot serve a function of sufficient importance to determine their preservation through Natural Selection, for *Lucernaria* thrives abundantly without them living in the same way and in the same places. We are therefore driven to the conclusion that these bodies, if not functionless, are at least of so little value to their possessors that the chief competitor in the struggle for existence (*Lucernaria*) survives although devoid of them. If this be true, then (*see* view 2 at commencement of this essay) they *must* be inconstant ("variable") in structure, or at least must tend to become so.

Now this point seemed to be so easy to put to the test of observation that I delayed to publish this essay till I should have an opportunity of applying the test.

I had not yet had the opportunity of so testing my conclusion when the July number of NATURAL SCIENCE appeared, and in it a record of observations made by my friend, Mr. Hornell, on this very point (vol. iii., p. 33). No communication on the subject had passed between us. His observations were made in ignorance of my conclusions, and hence the element of bias is happily absent.

I should have been sanguine indeed to expect so striking a confirmation of my conclusion! Out of one hundred and eighteen specimens of *Haliclystus* taken in June last in Jersey, seventy-eight (that is, two-thirds of the whole number) exhibited a very marked abnormality in the structure of these organs, *i.e.*, some of the organs were "crowned with tentacles." Of specimens gathered earlier in the year (that is, older specimens which had lived through the winter) a small proportion possessed less than the normal number of these bodies. None were observed to be "crowned with tentacles." Either the tentacle-crowns had been absorbed, or the individuals possessing them had been worsted in the struggle for existence during the past winter.

Further observation is necessary before we can say which is the true explanation. The fact that many—about 15 per cent. in a lot I received from Messrs. Sinel and Hornell in the spring of the present year—have survived through the winter with less than the normal number of these bodies, shows at least that the possession of the full number of these bodies does not give a very great advantage, if any, to their possessors.

Some years ago I obtained from Mr. Bolton a large number—probably nearly two hundred—of specimens of so-called *Lucernaria*, which, he informed me, had been collected on the coast of Cornwall, and they were forwarded alive on some red algæ. They were all small—it was autumn—and I then noticed that they were of two kinds—*Haliclystus* and *Lucernaria*—mostly *Haliclystus*. I had not considered this problem then, and simply took them to be two genera mixed, and thought no more about it. I did not examine them closely, and hence cannot say whether intermediate forms with less than the full number of marginal bodies were present or not. The naturalists at Plymouth may well take that problem in hand and decide for us whether *Lucernaria* and *Haliclystus* are generically—to say nothing of specifically—distinct or not.

Whatever answer may be given, Mr. Hornell's description of *Haliclystus* must be enough to convince any non-evolutionist—if such a zoologist still exists—of the mutability of species (or genera). It is a simple description of a species in the very middle of its transformation—*Haliclystus* is in the very process of being transformed into *Lucernaria*, there, on the coast of Jersey!

On receiving my friend's paper I at once wrote to him, giving him an outline of the above argument, and pointing out the remarkable way in which his observations fitted in with my conclusions, and was not a little surprised to receive, by return of post, a long letter, in which he showed how he had, by an altogether different line of reasoning, come to the same conclusion as my own, viz., that *Haliclystus* is a "degenerate" scyphomedusan—an "arrested scyphistoma"—and that *Lucernaria* has gone further in its "degeneracy."

If, however, such a "degeneration" or "arrest" may occur once, it seems probable that it may occur often. There may be a well-established genus, *Lucernaria*, to which new species are now being added, not by the modification of the old one or ones, but by new "degenerations" of scyphomedusæ, or new "arrestments" of scyphistomata of various species of medusæ; and hence existing well-established species of *Lucernaria* (supposing there *are* such) may have arisen, not from a common ancestral form of *Lucernaria* at all, but by a series of separate "degenerations" of scyphomedusans through a *Haliclystus* stage. Or, on the other hand, it may be that *Lucernaria* and *Haliclystus* are only names for different forms of "monstrous" scyphomedusa, arrested in the scyphistoma stage—that is, arrested so far as strobilation is concerned, but developing the reproductive organs.

I would submit, therefore, the following conclusions for criticism:—

(1.) That *Haliclystus* is a degenerate or arrested scyphistoma (*cf.* axolotl!);

(2.) That its marginal bodies are *arrested rudiments* (*i.e.*, vestiges) of tentaculocysts;

(3.) That *Lucernaria* is a still further modified ("degenerate") descendant of some free-swimming medusæ of one or more species;

(4.) That the degeneration and disappearance of the tentaculocysts, when these ceased to be of use for the function I ascribed to them, supports the view I have expressed in a former essay on the subject of their function.

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C. HERBERT HURST.

VII.

Notes on Pipernoid Structure of Igneous Rocks.

MOST geologists are acquainted with a peculiar rock, much employed as a building stone in Naples, that occurs near that town at a village called Pinaura, and known by the name of "Piperno." What the origin of this name may be, I do not pretend to know, but it much resembles the word *peperino*, a true though peculiar tuff, derived from a sub-lacustrine eruption of the Colli Albani, near Rome, but entirely different in character.

For all practical purposes, this rock may be called a rather basic trachyte, but not uniform in structure. It is of a clear grey colour, interrupted by innumerable flackers,[1] when seen in section lying parallel to the surface on which the rock bed rests. These, when isolated, are seen to be flattened cakes of an original pasty material, rather longer in one definite axis with striæ parallel to the same, and of such a nature as to show that shearing occurred in the viscous material between the upper and lower surface of each cake. Chemically and microscopically, these two constituents are identical, except that the black flackers, or cakes, are more compact, exhibit true fluxion structure in harmony with their planes of shearing, and are much more vitreous; in fact, they present all the characters of a lava. The grey material is very spongy, but does not present true vesicular structure, extending beyond the minute particles of which it is composed, and, in fact, exhibits all the characters of a tuff—that is, a consolidated volcanic dust.

Piperno occurs in one or more beds in the neighbourhood of Pianura, Soccava, and Monte Spina. Of late years I have paid much attention to this rock, and I have been successful in working out the stratigraphy and physical history in detail of the Phlegrean Fields and the Campanian plain. The principal conclusions that I have come to have been published in the Reports Brit. Assoc. since 1884. In these researches I was able to show that we could trace the piperno into the grey tuff which covers several hundreds of square miles of the

[1] This word is used by cabinet-makers to denote the markings due to the cutting the medullary rays in an oblique manner, which resemble the graining of piperno.

Campanian plain, and even remnants of which are found filling depressions in the limestone as far as Bari. This tuff much resembles the piperno, except that the grey interstitial matter gets finer and lighter the further we extend outwards radially from the true piperno. Coincident with this, we find that the blacker cakes become more and more spongy, not at first rapidly decreasing in size, but decreasing in bulk specific gravity. After a certain distance, we find they also decrease in size, until at distances of 20 to 30 kilometres they are almost invisible. The flattening out of the cakes, parallel to bedding, soon disappears as the black inclusions get lighter and more scoriaceous.

All geologists who have attempted to explain these *principal* peculiar characters, have utterly failed to do so, and had I space to enumerate many minor ones, the difficulty would be still greater. Unfortunately, most of these conclusions have been jumped at, as the result of that useful instrument though unfortunate misleader of geology, the microscope, which has caused investigators to forget that it is only one means to an end, and that field investigation is of far greater importance.

Some have supposed it to be a metamorphosed tuff, but I have shown that subjacent beds of pumice are as fresh almost as the day they fell; others, have imagined it metamorphosed by infiltration or some internal reaction; others, again, maintain it to be a lava. It was only after years of careful investigation that the true explanation suggested itself to me.

In my researches on the Geology of Monte Somma and Vesuvius,[2] I was able to bring forward conclusive evidence that the volcanic paste that occupied the duct of a volcano was nearly, if not entirely, vapour-free, and that it dissolved H_2O out of the upper stratum of aquiferous rocks of the earth crust. I showed that in any explosive eruption, the uppermost part of the paste in the canal was the most gaseous (H_2O), and that, as the eruption progressed, the pumice ejected became more compact, and cooled slower by a less conversion of its potential into kinetic heat for the evolution of less gaseous contents.

The Geological Society published my facts, refusing to do so with my conclusions, but the Royal Dublin Society was more charitable and put them before the public.[3] Those facts and conclusions have never been assailed, but have carefully been passed over by subsequent writers on volcanoes, although they are, I maintain, the only scientific and generally applicable explanation of volcanic action offered to the public—not a romantic hypothesis, but one borne out in every region I have yet examined.

Now, what appears to have occurred with the *piperno* eruption is

[2] *Q. J. Geol. Soc.*, 1884, vol. xl., pp. 35–112.

[3] *Q. J. Geol. Soc.*, 1885, vol. xli., pp. 103–106; and *Scientif. Proc. Roy. Dublin Soc.*, vol. v., n.s., pp. 112–156.

as follows: A trachytic paste occupied a volcanic duct towards the surface. During a considerable period of repose the upper portion absorbed much water, its temperature fell, and its tension rose in proportion; lower down, less change of this nature had taken place. When the final outburst occurred, the vent must have been of tremendous size, as proved by the enormous quantity of material ejected. In consequence, the lower part rose almost coincident with the upper, as shown by the arrangement of the deposit. The upper cooler, more aquiferous part, was soon reduced to a solid dust by vesiculation, and fell as such, while the hotter, lesser aquiferous, was only separated into cakes. The heaviest and most compact of these would fall amid the dust as hot masses, close to the vent where they would spread out by their own weight and by the pressure of other material that immediately buried them. They were squeezed out by a movement hardly amounting to flow, but just sufficient to elongate them where they rested on an inclined surface. The greater the inclination, the more are they pulled out, so that in the buttress under Camaldoli, which shows the bed fairly inclined, the flackers are reduced to that of the thinnest paper.

The top and bottom of each piperno bed are less compact, and have less of the enclosed flackers of lava, for very obvious reasons, too lengthy to detail here. Even in a single bed of piperno, the variation in size and numbers of the flackers varies greatly with the horizon, but such variations are continuous over considerable distances. These differences of structure really record the fluctuations in the force of the eruption and the temporary predominance of one or other of the two kinds of ejecta.

Near the vent little dust fell, because it reached great altitudes and was carried away by the wind. The heavier lava cakes, however, fell by preference near the vent, especially the more compact and heavier ones; more and more scoriaceous, and therefore lighter were they, the farther they travelled, and the colder were they when they fell, so that at a short distance they were too hard to flatten out. At still greater distances, however light and scoriaceous they were, the large, then smaller and smaller ones, fell, until finally the deposit is practically composed of the dusty part only.

The *piperno* at Pianura consists of several beds intercepted by brecciated fragments of trachyte of the same composition, which, no doubt, mark an interval during which a crust solidified, to be re-broken up by the fresh outburst that formed the next bed.

I have spoken of one vent, but I do not deny the possible existence of a long fissure that reached the surface at several points. Neither should I dare to attribute certain pipernoid tuffs, south of and near Rome, to the vent, or the main one if several, that opened between Monte Spina and Camaldoli. At Monte Spina the blobs of lava were so large and so slow in cooling that they actually flowed as a stream.

In Ischia, on the east coast of the island, is a lava in which two magmas seem to have issued and flowed together, almost reproducing that kind of structure of earthenware, of mottled design, made by working together different coloured clay. From that we pass to the banded trachytes of Palmarola and the so-called felsites (because they are old) of the neighbourhood of Dresden, or the liparites of Basiluzzo.

The vitreous crust of some of the flackers, the peculiar conformation around foreign enclosures, the nature of these latter, and many other points might be discussed as confirming this explanation. At any rate, I hope to have cleared up a knotty point in lithology by showing that my theory of volcanic action is able to explain even an intricate rock structure, such as that which I have called "pipernoid," from its remarkably perfect illustration in the *piperno*, and which I have frequently seen exhibited in rocks from Iceland to Sicily.

H. J. JOHNSTON-LAVIS.

VIII.

The Evolution of Life.[1]

PROFESSOR BROOKS has printed in advance from his memoir on the genus *Salpa*, chap. vii., on "Salpa in its Relation to the Evolution of Life," and chap. viii., on the "Origin of the Chordata considered in its Relation to Pelagic Influences." Salpa is distinctively a pelagic animal borne hither and thither on the surface of the ocean as currents and the wind direct, living entirely on the minute micro-organisms and, unlike so many of its nearest relations, spending no part of its life attached to rocks, or weeds, or mud. Professor Brooks has been studying its life-history and its structure minutely, and has come to some biological conclusions of very general interest, conclusions which, perhaps unnecessarily, he apologises for as heterodox; but we hope that morphology so far is free from the doctrine of authority. Its generalisations tread so closely on the heels of observed facts that, granted the necessary skill in manipulation, no morphologist need accept much on the authority of a memoir, or a text-book, that he cannot verify with the scalpel and microscope.

Dr. Brooks leads up to his results by consideration of the contrast between terrestrial life and marine life. The surface of the land is more or less completely covered by verdure. Forest trees, and herbs and grasses, ferns and mosses stretch their green expanses into the air, and the chlorophyll of the cells in the presence of sunlight is constantly building up from the inorganic elements of the earth and air rich supplies of protoplasmic food; and so on the surface of the earth the vegetable eater, as insect, or bird, or mammal, abounds not only in species but in individuals, while carnivorous forms are found in smaller numbers, and obviously in immediate dependence on the vegetarian world.

The ocean waste is very different. Here and there are floating tufts of sargassum, but save round the coasts the vegetable world is meagre, and the richly-coloured gardens of the bottom are nearly all animal forms. So, too, in the sea, the conspicuous animals are nearly all carnivorous. The seals live upon fish; the sea elephants and walruses on lamellibranchs; whales, dolphins, and porpoises

[1] STUDIES FROM THE BIOLOGICAL LABORATORY OF JOHNS HOPKINS UNIVERSITY, vol. v., No. 3. "Salpa in its Relation to the Evolution of Life." By Professor W. K. Brooks, Ph.D., Baltimore, May, 1893.

live upon sea animals. The enormous numbers of terns, gulls, petrels, cormorants, and so forth, are all carnivorous. Among lower forms, practically only a few molluscs, echinoderms, and annelids are plant-eaters, and they play an inconspicuous part in the economy of the ocean.

If a small amount of *débris* coming from the land is left out of count, the source of the food-supply of the ocean is to be found in the minute organisms of the surface. *Trichodesmium*, *Pyrocistis*, *Protococcus*, and the coccospheres, rhabdospheres, and diatoms form the lowest link in the chain. These and the globigerinæ and radiolarians that feed on them, are so abundant and prolific as to supply practically all the food for the animals of the ocean. Such a simple pelagic food-supply Dr. Brooks believes to be, not only the fundamental, but the primæval supply—the supply which has determined the course of evolution of marine life, and secondarily of all life. The conditions of pelagic life are so simple and so universal, that the dawn of life appeared there. There is no fierce competition, and very little stimulus for the production of diversity of habit. All the metazoa have pelagic larvæ, or else embryonic stages recalling such larvæ, and best interpreted as the degenerated vestiges of a pelagic habit. During the long period in the history of the earth when there was practically only pelagic life, the pelagic ancestors of the great groups of the metazoa were evolved.

These advanced little till multiplication in the number of individuals drove some to the bottom or to the shore. In the strain and stress of the new conditions with their more varied mutable environment, increase of size and complexity, appearance of shells and skeletons began. All this happened before the earliest fossiliferous strata, for these show abundance of complicated types, of which their minute primæval pelagic ancestors left no traces in the sedimentary rocks. Secondarily, a number of forms returned from the bottom to the surface just as the Cetacea and many sea-birds returned from the land to the sea, and the improved wanderers rapidly produced changes in their ancestral pelagic home; but none the less a number of surface forms Dr. Brooks believes to retain the simplest possible structure, and to resemble closely the primæval ancestors of their groups.

Among these are notably the ancestors of the Chordata. Dr. Brooks criticises very closely Dohrn's famous degeneration theory of the Ascidians and finds in *Appendicularia* a little modified ancestral pelagic chordate.

One difficulty in the way is the possible importance of metamerism; but Dr. Brooks makes light of this, and many morphologists will be ready to agree with him. Metameric segmentation is a feature which occurs in widely-separated groups: which appears again and again in cases where there can be no possibility of community of descent. It may, in fact, be said that metamerism is so characteristic of life that

it is more likely to disguise than to reveal kinships. Dr. Brooks considers it a corollary of growth by cell-reproduction, but to whatever biological cause it may be due, too much importance has been attached to it in the framing of phylogeny.

The ancestor of *Appendicularia* he pictures as a "simple, minute, unsegmented chordate animal, leading a free locomotor pelagic life, and subsisting on the micro-organisms of the ocean. It had an elongated, unsegmented body stiffened by an axial, unpaired, unsegmented notochord like that of *Amphioxus*, *Appendicularia*, and the ascidian larva: a simple elongated dorsal nervous system: and an elongated, ventral digestive tube without pharyngeal clefts." This tube was distended and ciliated, and the water with the micro-organisms in it was swept through by ciliary motion, not by contractions of the body-wall.

In process of time there came slime cells to retain the food, and Natural Selection preserved and accentuated the slime cells in the anterior region. The free current of water passing through the animal at first swept in new organisms, and swept even any partially digested organisms. However it might have appeared, a pharyngeal cleft that would allow this free stream of water to pass out, leaving behind the slime-bound food, would be an advantage too great not to be selected, and one gill cleft being established, Dr. Brooks believes that by a "law of growth" it would become paired.

Thus he finds in the primitive gill clefts mechanical advantages for nutrition quite unconnected with respiratory or excretory functions. The velum, the endostyle, and the peripharyngeal bands similarly were food-catching mechanisms.

He traces out stages by which fixed Ascidians might easily have been derived from a free-living form like *Appendicularia*.

In the next section Dr. Brooks criticises the Annelidan theory very closely; but we imagine that, however fertile they have been in suggestions, and full as they are of brilliant morphological detail, not many naturalists support the main thesis of Dr. Dohrn's well-known studies; and for that reason it is hardly necessary to refer in detail to the present criticism. Still less to the associated criticism of the hypothesis that the Tunicates are degenerate Selachians.

What is more interesting is the view Dr. Brooks takes of the coelome. Since Hertwig first insisted on the importance of this method of development of the body-cavity, it has been accepted as final that the chordate body-cavity is an enterocoele—that is to say, that it is developed from series of pairs of gut-pouches, and that the simplicity of *Appendicularia* cannot be primitive, as its ancestors once possessed a segmented enterocoele. Seeliger gives a minute account of the origin of the mesoderm in *Clavelina*, and states that it comes from two totally unsegmented rows of cells, while the body-cavity is really a primary segmentation cavity. Davidoff, working on *Distaplia*,

agrees with him, and Dr. Brooks, from the development of Salpa, coroborates both. Practically, his conclusion is that the Chordate ancestor came straight from a simple pelagic form of which *Appendicularia* is a near living kinsman. The relations between the Chordates and other groups are to be looked for only among the characters common to such simple pelagic forms, and all chimæras, like the Arachnid or Crustacean ancestry of the Chordates, are relegated to the same oblivion as the more useful, if not more satisfactory, Annelidan hypothesis. It must, however, be pointed out that while Dr. Brooks received Willey's new work on Ascidians and *Amphioxus* in time to criticise it sharply, he had apparently not received, and so has not yet tried to consider, the bearings of the newly-discovered nephridial organs of *Amphioxus*.

SOME NEW BOOKS.

BIRDS IN A VILLAGE. By W. H. Hudson, C.M.Z.S. 8vo. Pp. 232. London: Chapman and Hall, 1893. Price 7s. 6d.

WE congratulate Mr. Hudson upon the publication of this pleasant volume, which is sure to receive a hearty welcome from all who like graceful essays about country life. "Birds in a Village" contains nothing absolutely new to us, nor is it intended in any sense as an ornithological treatise. Its function is to convey to us in a singularly felicitous style the impressions which the author has gathered from his rambles beside English brooks and across our home commons. Therein Mr. Hudson has found an agreeable variation from city life. Accordingly, he introduces us to simple village folk, and interprets for us their thoughts and instincts; or sketches the picturesque details of local scenery, following out his observations with such obvious faithfulness of narrative that we seem to see with his eyes and to hear with his ears.

There is, it must be confessed, a large element of "twaddle" in these pages; but *that* is such a constant ingredient of "popular" books upon Natural History, that it would be absurd to quarrel with it. The general public will perhaps prefer it to the enunciation of scientific truth; if it does not do much good, assuredly it is impotent to do anyone an injury. We know that Mr. Hudson is at heart as devoted to scientific ornithology as could be desired, and that when he romances or tells a pretty fairy tale, it is only because he prefers to be amusing rather than to edify us. Slight as he admits his knowledge of English birds to be, his diffidence cannot conceal the fact that with many of our homely birds he is thoroughly familiar, and his descriptions of their idiosyncrasies are pleasant to glance over. Not that Mr. Hudson discovers new points in their life-history; but because, being perfectly in correspondence with his environment, he plays the part of a faithful interpreter of their emotions and sensations, whether occupied in studying the movements of a jay amid the leafy foliage of some quiet copse, or comparing the song of the hedgerow nightingale with the richer notes of the white-banded mocking bird of his native country. Mr. Hudson is always certain to enlist our sympathy in his patient study of the most commonplace incidents.

We differ from Mr. Hudson in one particular. We refer to his tirade against the supposed enormity of keeping song-birds in captivity. The most that he can say against this custom, which exists all the world over, is that Mr. Rennell Rodd has protested against the practice in the dainty verse which our representative at Zanzibar so well understands how to frame. We have ourselves derived a vast amount of pleasure from our caged pets, and having enjoyed in this way the friendship of such rare and interesting forest birds as the Nutcracker, or the Great Spotted Woodpecker, may here register our dissent from Mr. Hudson's perfervid sentiments.

H. A. MACPHERSON.

HANDBOOK OF PALÆONTOLOGY. [HANDBUCH DER PALÆONTOLOGIE.] By K. A. von Zittel. Vol. iv., pt. ii. Pp. 305–592. Munich and Leipsic: R. Oldenbourg, 1893. Price 10 marks 50 pf.

As briefly mentioned in our last number, where we gave a notice of the preliminary issue of the Introduction, this important work is now fast approaching completion, and we have the pleasure of welcoming the second fasciculus of the volume devoted to Mammals.

This part, which fully maintains the high standard of its predecessors, contains the greater portion of the Ungulates, which are concluded, together with the Rodents, Insectivores, Bats, and the commencement of the Creodont Carnivores. We are glad to observe that among the Ungulates special attention is devoted to the numerous South American forms which have been described of late years; and we may safely say that nowhere else can the student obtain so much information on a very difficult subject in a very short space. In regard to the arrangement of the Ungulates, the author, while following in the main the divisions into sub-orders adopted by most modern writers, makes a new departure in respect of the limits of families. He takes, for instance, the Suidæ to include genera so widely different from the typical forms as *Achænodon* and *Chæropotamus*, while the Anoplotheriidæ embraces the Cænotheres, Dichobunes, and Dichodons, and under the Cervidæ are ranged, not only the Deer, but likewise the Giraffes and Sivatheres. We confess that this arrangement is not to our liking, as it involves the introduction of a number of sub-family names; while the inclusion of the Giraffe and its allies in the same family as the Deer tends to obscure their manifest affinities in some respects with the Bovidæ. In discarding the sections Tragulinæ and Pecora, we are, however, persuaded that, from a palæontological standpoint, the author is correct, since, as we ourselves have previously urged, it is impossible to draw any line of demarcation between the Tertiary Deer and Chevrotains. That the Professor should have seen reason to employ family names in such a wide sense is the more surprising to us, seeing that among the Ungulates he splits up many groups, such as the Elephants, Oxen, and Uintatheres, into so-called genera, which it has been the tendency of modern writers to ignore. We are pleased to notice that the author fully confirms the view that the three-toed Eurytheriums of the Vaucluse Oligocene are not generically separated from the typical Anoplotheres of the Paris Basin; but we should like to be more fully assured that the latter were likewise tridactylate. So far, indeed, as the casts of Cuvier's typical specimens in the British Museum admit of forming a judgment, the Paris specimens certainly appear to have been didactylate; and an important difference in the form of the astragalus in the specimens from the two localities has been noticed by several writers. As a minor criticism, we may mention that we cannot assent to the inclusion of the Hartebeests in the same group as the Gemsbok and Sable antelope, the cheek-teeth of the two being so essentially different; while the writer of this notice may take the opportunity of disclaiming the position of sponsor to the genus *Helicophorus* assigned to him on page 418.

As the treatment of the other orders described in this fasciculus does not call for any special notice, we may conclude with the hope that we shall soon have the opportunity of congratulating the learned author and his able colleagues on the completion of their arduous and monumental task.

R. L.

CHEMICAL AND MICRO-MINERALOGICAL RESEARCHES ON THE UPPER CRETACEOUS ZONES OF THE SOUTH OF ENGLAND. By William Fraser Hume, D.Sc., etc. 8vo. Pp. 104. 3 Tables. London: [Printer—Whitehead, Morris & Co., 9 Fenchurch Street], 1893.

THIS publication is, we believe (though it is not so stated), the author's thesis for the degree of D.Sc. of London. It is the most interesting contribution that has appeared for some time on the Cretaceous rocks, and bears evidence of a vast amount of laborious and painstaking research. Commencing with the Chalk Marl, the author gives chemical and mineralogical analyses of the washings of samples of 80 grammes average weight of each of the zones of the Chalk, from the Chalk Marl to the *Belemnitella mucronata* zone, and also discusses the micro-zoological remains which occurred in the residues. The samples were mostly taken from the immediate neighbourhood of some distinctive fossil, so as to be typically characteristic of the zone to be examined. Each zone is treated separately and in detail, the chemical analysis coming first, then the organisms, thirdly, the secondary minerals, and, lastly, the detrital minerals.

The author has wisely taken into his confidence specialists in their own departments, and has thus not fallen into the fatal error of describing everything that came into his hands. Thus, Dr. Hinde has looked over the sponge spicules, and Mr. Chapman has examined the Foraminifera. Towards the end of the paper we find a summary of results, and here Dr. Hume discusses the secondary silicification of Foraminifera in a clear and exhaustive manner. He says, "there seems little reason to doubt that considerable replacement of the calcareous constituents by colloid silica has taken place," and he notes that the most delicate interior divisions are in a fine state of preservation, after the residue has been treated with acid. So complete has this replacement been in many cases that, in the process of solution by acid, the bubbles issue from the mouth only of these microscopic tests. Following these mineralogical notes, Dr. Hume mentions a comparative identity between the foraminiferal fauna of the Chalk Marl and the Gault, and commenting on the depths at which many forms live in recent seas, inclines to the view that the Chalk Marl was deposited at a depth of from 350–500 fathoms. No new light has been thrown by Dr. Hume's researches upon the distribution of the sponges in Cretaceous times, nor has he met with remains of Radiolaria or Diatomaceæ in the rock-specimens examined.

In the general summary devoted to mineralogy, the author notes the predominance of quartz in angular fragments, with occasional crystals, worn and clear, in the lower Cenomanian beds. Inclusions in quartz of apatite, rutile, tourmaline, ferruginous oxides, liquids, and gases are recorded, and other forms of silica, as jasper and chalcedony, have been detected. Felspar occurs in Chalk Marl at Folkestone and in certain of the Turonian and Senonian beds of other places, but it is much altered. Hornblende, augite, tourmaline (widely distributed), zircon, rutile, garnet, glauconite, limonite, and pyrite are also recorded in the paper. The glauconite abounds in the Chalk Marl, but rapidly diminishes in the higher Cenomanian. It reappears as an important constituent in the Chalk rock, and continues to form a considerable portion of the residue in all the lower Senonian zones. Preference is given to the theory, in accordance with Cayeux's views, that glauconite may be formed independently of the presence of organisms. According to the author's observations, the greater the thickness of the beds the more calcareous do they

become, and he does not accept Cayeux's view of the terrigenous origin of Chalk as applicable to the Chalk as a whole, but considers that "such evidence as is forthcoming from faunal considerations points to an ocean which, if not abysmal, at least possessed depths far exceeding those of many prominent marine areas."

Space prevents any further notice of this interesting contribution to Cretaceous history, the researches for which have been carried out in the laboratory of the Royal College of Science; and while congratulating Professor Judd on the success of his choice of demonstrators, we thank Dr. Hume for providing so much food for reflection to the students of oceanic deposits, micro-zoology, and geology.

LESSONS IN ELEMENTARY BIOLOGY. By T. Jeffery Parker, D.Sc., F.R.S. With 88 illustrations. Second Edition. London: Macmillan & Co., 1893. Price 10s. 6d.

PROFESSOR PARKER'S well-known text-book is designed for reading—a complement to laboratory work with a text-book like the "Practical Biology." It aims at teaching the ideas rather than the details of the science of Biology—these ideas being studied in connection with concrete types of animals and plants. It is hardly necessary to do more than welcome the second edition of a volume so familiar and so satisfactory. It has been revised; several new figures have been introduced; and the lessons dealing especially with cell-structure and sexual cells (vi. and xxiv.) have been practically re-written. It is hoped that the few notes now to be made will be taken, not as criticisms directed against the book, but as footnotes which may be of some use.

On page 7 Professor Parker writes that "the theory is that protoplasm has a slightly acid reaction." This is very doubtful; it is practically certain that the enchylema is slightly alkaline—and it is doubtful if there is acid reaction in living protoplasm except in the secretions in food vacuoles.

On page 16 the author distinguishes between "true excreta" and the solid excreta, or, "more correctly, *fæces*, of Amœba—the rejection of which latter is no more a process of excretion than the spitting out of a cherry-stone." But certainly the "fæces" of the higher animals are largely composed—not merely of undigested substances, but of matter actually excreted by the cells lining the digestive tract.

On page 21, where the author is discussing the effects of salt on Amœba, he conveys the impression, although he does not definitely assert, that Amœba is confined to fresh-water—whereas it is quite common along the shore.

In the section on Diatoms no mention is made of Bütschli's account of the cause of their movement. (See NATURAL SCIENCE, vol. i., p. 537.)

SUBTERRANEAN PICARDY [LA PICARDIE SOUTERRAINE. Album Composé de dix Planches en Chromolithographie reproduisant des Armes, Outils et autres Utensiles en Silex trouvés dans les Alluvions Quaternaires de la Vallée de la Somme (Époques Chelléene et Moustérienne). Dessinées et Lithographiées en couleur par M. Pilloy, officier d'Académie [etc.]; et imprimées par M. Bourbier, Rue Saint-Jacques, 9, Saint-Quentin.] 4to. Pp. 10, pls. x. Saint-Quentin: Poette, 1892.

UNDER this lengthy title we gladly chronicle a book of ten pages and ten plates, relating chiefly to flint implements. The text is very

brief, is dated May, 1892, and is mainly descriptive of the plates which follow. These plates are so remarkable in execution, and so true in colouring, that it is scarcely an exaggeration to say that they are as good as the implements themselves. The extraordinary markings, so familiar on broken flints, are here produced in a most perfect and striking manner; indeed, we had scarcely hoped that chromolithography had reached so high a degree of excellence. MM. Pilloy and Bourbier, for their names must be linked together in this beautiful production, have evidently worked for the love of their subject, and whether we regard these plates from the point of view of draughtsmanship or from that of artistic skill in printing, they have given us results of which they may be justly proud, and which, instead of being buried in our bookshelves, should rather find a place upon our walls.

REPUBLICATION OF CONRAD'S FOSSIL SHELLS OF THE TERTIARY FORMATIONS OF NORTH AMERICA. By Gilbert Dennison Harris. 8vo. Washington: May, 1893. Price 3 dols. (after November 1, 3.50 dols.)

CLOSELY following on Dr. Dall's collation of Conrad's works, Mr. Harris, who has devoted much time and labour to the collation of these rare publications, has issued an exact reprint of the text, and facsimile of the plates, of one of them. Mr. Harris was largely responsible for the facts in Dall's work, and has received full acknowledgment in the preface. Now that we are in possession of Conrad's paper, many doubtful points in the identification of species of American Tertiary fossils will be cleared up, and without entering into controversy as to the actual dates of publication of the parts of this work, we feel that the thanks of all palæontologists are due to Mr. Harris, in reproducing for us a book so rare, that it could only be compiled from fragmentary and incomplete copies of the original.

CATALOGUE OF SOUTH AUSTRALIAN MINERALS, with the Mines and other Localities where found; and brief remarks on the mode of occurrence of some of the principal metals and ores. By H. Y. L. Brown. 8vo. Pp. 36. Adelaide: 1893.

THIS Catalogue supplants that of Goyder issued in 1883, and is intended to supply miners, prospectors, and others, with a record of mineral discoveries for convenient reference. The author does not pretend that it is exhaustive, as many private collections which possibly contain minerals are unavailable. The Catalogue is arranged in alphabetical order, and in each case consists of the name of the mineral, followed by a list of the localities.

WE understand that Messrs. Cassell & Co. have resolved to discontinue the issue of the *Year Book of Science*. Only two annual volumes (1891, 1892) have been published.

OBITUARY.

JOHN RAE, F.R.S.

BORN 1813. DIED JULY, 1893.

AS briefly noticed last month, this veteran Arctic explorer has passed away. He was a native of the Orkneys, and at the age of 16 entered the medical school of the University of Edinburgh. In 1833 he was appointed surgeon on one of the vessels of the Hudson's Bay Company, and in 1846 he undertook an important survey of the North-east Coast of North America. Two years later Dr. Rae accompanied Sir John Richardson in searching for the missing Franklin expedition; and the next six years were occupied almost continually in exploring the Arctic regions. He first proved King William's Land to be an island, and his detailed surveys have placed him in the front rank of the pioneers in Arctic exploration. Besides numerous smaller memoirs, Dr. Rae published a "Narrative of an Expedition to the Shores of the Arctic Sea in 1846 and 1847" (1850) and in 1852 he was awarded the gold medal of the Royal Geographical Society.

JAMES WILLIAM DAVIS.

BORN APRIL 15, 1846. DIED JULY 21, 1893.

AS briefly announced last month, a sad breach has been made in the ranks of amateur naturalists by the death of Mr. James W. Davis, of Halifax, at the early age of 47. Born at Leeds in 1846, and educated at the local grammar school, he and a brother succeeded to the cloth-dyeing business of his father; and his active pursuit of scientific investigation was carried on only in such leisure as he could secure from business and from the numerous political, municipal, and educational movements in which he was a leading figure. The patronage of Art, Literature, and Science, and researches in Geology, Palæontology, and Archæology, formed Mr. Davis's means of recreation; and it is astonishing how much he was able to accomplish in the limited time at his command.

Though a prominent worker in the Yorkshire Societies from his earliest youth, Mr. Davis did not enter the scientific world as an original investigator until 1873, when he became a Fellow of the Geological Society and a member of the British Association. At

first he confined himself to the Geology of Yorkshire, and in 1878 he co-operated with the botanist, Mr. F. A. Lees, in publishing the well-known volume on "West Yorkshire." He had always felt especially interested in the fish-remains discovered in the Yorkshire Coal-measures, and on making the acquaintance of the late Earl of Enniskillen and Sir Philip Egerton, at the meeting of the British Association at Belfast in 1874, his attention was definitely directed to Fossil Ichthyology. He published many small papers, chiefly relating to the Carboniferous fishes, until 1883, when, at the suggestion of the Earl of Enniskillen, he began to contribute a series of large illustrated memoirs to the *Transactions of the Royal Dublin Society*. His first memoir dealt with the fossil teeth and spines of the Carboniferous limestone, chiefly based upon the Enniskillen collection. The second memoir related to Professor Lewis's great collection of Cretaceous fishes from Mount Lebanon. Subsequently there were memoirs on the Cretaceo-Tertiary Fish-remains of New Zealand and on the Cretaceous Fish-remains of Scandinavia; while only last year the first part of a contemplated series of memoirs on the Coal-measure Fishes of the British Islands appeared in the same form. It is a matter of profound regret that Mr. Davis's untimely death should thus abruptly end the work.

Mr. Davis, however, was not merely an indefatigable palæontologist—one who has left a permanent impression upon Fossil Ichthyology; he was also a keen man of business, whose advice was eagerly sought in all manner of scientific undertakings. He had travelled extensively, and was on intimate terms with the leading men of his time in many walks of life. His extensive knowledge of the world, his genial disposition, and his enthusiasm in watching the advancement of every department of learning, gave him a position in British Science that none but his intimate friends could estimate; and it may truly be said that the demise of so valued a patron and trusted an adviser is a great and almost irreparable loss.

GEORGE BROOK.

BORN 1857. DIED AUGUST 12, 1893.

THE death of Mr. Davis in the prime of life was an unexpected shock, but still sadder is the sudden demise of his intimate friend and fellow-Yorkshireman, Mr. George Brook. While out shooting on the moors in Northumberland on August 12, Mr. Brook was paralysed by sunstroke and died immediately, though just before apparently in the best of health. Mr. Brook was born at Huddersfield 36 years ago, and entered his father's well-known cotton manufactory in that town. His interest in scientific matters, however, soon estranged him from business, and he trained himself in scientific work until he became an accomplished zoologist. His earliest observations were made in a

private aquarium he established at home, and some of his results were issued in pamphlet form. In 1884 he was appointed Assistant-Naturalist under the Scottish Fishery Board, and in the next year he became Lecturer on Embryology in the University of Edinburgh—a position he held until his death. Mr. Brook's first great work was his memoir on the Antipatharian Corals, published in the "Challenger" Reports in 1890; and for the last three years he had been occupied in cataloguing the corals in the British Museum. The first part of this catalogue, a fine quarto volume, dealing with genus [*Madrepora*, and exquisitely illustrated with Mr. Brook's own photographs, has only just been issued. Now, alas! the work is suspended, and must be relegated to other hands. Mr. Brook was a member of Council of the Linnean Society, and the loss of his genial presence will be mourned by a large circle of friends.

GEORGE WILLIAM SHRUBSOLE.

BORN 1827. DIED JULY 22, 1893.

CHESTER has lost a valued citizen, and Science an enthusiastic worker, in the person of Mr. G. W. Shrubsole, F.G.S., the geologist and antiquary. He belonged to the Kentish family of the same name, being a brother of the well-known geologists, Messrs. W. H. and O. A. Shrubsole, and settled in Chester some forty years ago. Commencing as a chemist's assistant, in the course of a few years he started a business of his own in Market Square, which is now in the hands of his two sons. In addition to his retail trade, he had a considerable connection as an analytical chemist among mine-owners in North Wales. He was Honorary Curator to the Grosvenor Museum, a member of the Chester Archæological Society, and a painstaking student of the antiquities of his own city. Mr. Shrubsole was one of the founders, with Charles Kingsley, of the Chester Society of Natural Science, and held the office of Chairman of the Geological Section for some years. He contributed numerous papers on local matters to the Chester societies, and published a handbook to Chester antiquities on the occasion of the visit of the British Archæological Association. In 1879 and 1880 he contributed papers on the Carboniferous Polyzoa to the *Quarterly Journal of the Geological Society of London*, and in 1883 was awarded the Kingsley Medal of the Chester Society of Natural Science in recognition of the value of his researches.

MAXIMILIAN VON HANTKEN.

GEOLOGY in Hungary has sustained a loss by the death last June of Maximilian von Hantken von Prudnick. This venerable naturalist was Professor of Palæontology in the University of Budapest, Director of the Hungarian Geological Society, and a

Counsellor of the Hungarian Ministry; his chief care being, we believe, that of Education. V. Hantken's chief work was "A Magyar Korona országainak széntepelei és szénbányászata," 1878, a translation of which appeared about the same time in German as "Die Kohlenflötze und der Kohlenbergbau in den Ländern der Ungarischen Krone." He was the author of many papers on the Tertiary beds of his country, and paid special attention to the Foraminifera and Ostracoda therein contained: his papers on the former group occupying two and a half pages of Sherborn's "Bibliography." His "Clavulina Szabói regetck Faunája," with its sixteen beautiful plates, was really a monograph on the Foraminifera of this horizon, and his studies on the Nummulites have been of considerable value to others working in the same field. V. Hantken visited this country for the last time on the occasion of the International Geological Congress, 1888, when his amiability, courtly polish, and enthusiasm endeared him to many friends.

WE also have to record the death, at the advanced age of 87, of Miss ANNE PRATT (Mrs. Pearless), authoress of the well-known "Flowering Plants and Ferns of Great Britain." Miss Pratt's first work on "Flowers and their Associations" appeared in 1826, and she published many small volumes on Natural History, chiefly Botany. She revised the last edition of her "Flowering Plants" so recently as 1880.

THE deaths are announced of the geologists, GEORGE ROBERT VINE and D. HOMFRAY, the former well known for his researches on fossil Polyzoa, the latter a successful collector of Cambro-Silurian fossils in Wales; also of the Norwegian bryologist, FANTZ C. KIAER.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

We accidentally overlooked the name of Mr. William Fraser Hume in the list of successful candidates for the degree of D.Sc. in the University of London this year. We review Mr. Hume's thesis elsewhere.

The honorary degree of Ph.D. has been conferred upon Mr. Arthur Gardiner Butler, Assistant-Keeper of the Department of Zoology in the British Museum, by the Western University of Pennsylvania, U.S.A.

Dr. E. P. Ramsay, the curator of the Australian Museum, Sydney, is at present incapacitated for work, and is not expected to return for three months. During his absence the duties of the post are being performed by Mr. R. Etheridge, Junior.

Mr. T. D. A. Cockerell, whose resignation of the Curatorship of the Jamaica Museum we noticed some months ago, has been appointed Professor of Entomology, Zoology, and Physiology in the New Mexico Agricultural College, Las Cruces, New Mexico, U.S.A.

Professor W. J. Sollas has been appointed on the Geological Survey of Ireland, to carry on the petrological work, in the room of Mr. W. W. Watts, who has been transferred to the Geological Survey department in London. Mr. Watts now continues the examination and description of the Volcanic rocks of Great Britain.

Dr. A. Dendy, who has for the last four years been assistant to Professor W. B. Spencer at Melbourne, has recently been appointed Professor of Biology in Canterbury College, New Zealand. At present, during Professor Spencer's visit to England, Dr. Dendy is taking charge of all the biological work at Melbourne University, but after February next his address will be Christchurch, N.Z.

Mr. D. T. Macdougal, assistant in Botany at Purdue University, has been appointed instructor in Plant Physiology at the University of Minnesota. Mis Alice Eastwood has succeeded Mrs. Katherine Brandegree as curator of the Herbarium of the California Academy of Sciences and as acting editor of *Zoe*. Dr. J. M. Coulter, one of the editors of the *Botanical Gazette*, has left the Indiana University to become President of Lake Forest University, Illinois, and has taken with him his large herbarium, of which Mr. E. Uline has been appointed curator. Dr. Hans Schinz has been appointed to succeed Dr. Cramer as Director of the Botanic Gardens at Zurich; and Dr. F. Kaunhowen becomes Assistant in the Section of Fossil Botany in the Royal Prussian Geological Survey, Berlin.

THE Smithsonian Institution has secured a table at the Naples Zoological Station for the use of American investigators, in response to a memorial signed by nearly 200 working biologists.

ACCORDING to *Erythea*, Messrs Sandberg and Leiberg are at present engaged, under the auspices of the Department of Agriculture at Washington, in botanical exploration on the Columbia plains. The late summer and early autumn they expect to give to the eastern slope of the Cascades and the desert plains beyond. Mr. John Macoun is devoting the season of 1893 to the botany of Vancouver Island.

WE understand that a new zoological quarterly will shortly be issued by the Tring Museum, containing memoirs on Mr. Rothschild's collections, partly by himself and his curators, partly by other specialists.

A NEW part of the "Notes from the Leyden Museum" (vol. xv., no. 3, July, 1893) has been issued. The contributions are technical papers on systematic zoology, the only one of general interest being a Comparative List of the Birds of Holland and England, by F. E. Blaauw. It appears that Holland is the regular home or resort of 221 species of birds, while the larger area of the British Isles has only 211 species —of course excluding casual visitors.

THE collection of Italian fossils made by the late Baron A. de Zigno, of Padua, has been acquired by the University of that city. It comprises many type-specimens of Vertebrata described in the Baron's memoirs, and will form an important addition to the fine Geological Museum in the University building.

A NEW hall has just been opened at the Industrial and Art Museum of Melbourne. It is a fine large room, with a wide gallery running round, and is lighted by skylights. It is devoted chiefly to Practical Geology and Mining, and, besides containing numerous models and examples of mining machinery, has a fine collection of rocks and minerals, at present being excellently arranged by Mr. Walcott.

OWING to the prevailing depression the public scientific institutions of Australia are in a bad way. The vote for the Australian Museum, Sydney, has been reduced by a half, which involves the dismissal of over a third of the staff, and considerable lessening of the salaries of those who remain. A large new hall has recently been built at this Museum, chiefly for the reception of the geological collections. It is, however, impossible to exhibit the specimens, as no money is forthcoming to provide fittings for the cases.

SIMILAR tales of woe reach us from other places. The collections of the Queensland Geological Survey were only a year ago moved from Townsville to Brisbane, where they have been displayed in appropriate quarters by Mr. Jack and his assistants, Messrs. Rands and Maitland. Now, since the Government can no longer pay the rent for the building, they must be shifted into rooms of little more than half the size.

THE Ethnological Collection belonging to the Museum at Auckland, N.Z., has recently been moved into a new hall, where it has been admirably arranged by the curator, Mr. T. F. Cheeseman, F.L.S., who has drawn up and printed very useful descriptive labels. The collection is especially rich in Maori curios, but contains also many valuable specimens from the South Sea Islands.

THE American Museum of Natural History has despatched another expedition this summer to the western territories for the discovery of fossil mammalia. This is the third annual expedition of the kind, and the Museum is already beginning to exhibit a remarkable collection as the result of Dr. J. L. Wortman's former explorations. In 1891 only a small collection was made in the Wahsatch beds of the Big Horn Mountains. In 1892 the Puerco Eocene formations of New Mexico, the White River Miocene of South Dakota, and the Cretaceous Laramie rocks were examined. About five hundred specimens were obtained from the first-mentioned horizon, many fine fossils were discovered in the Miocene, and a very large collection of the minute mammalian teeth was made in the Laramie. The recent writings of Professor H. F. Osborn give some idea of the value of the investigation, on which the American Museum is to be congratulated.

A NEW scientific society, named the "Società tra i cultori delle Scienze mediche e naturali in Sardegna," has been founded at Cagliari, Sardinia.

THE new Malacological Society of London has decided to issue Proceedings at undetermined intervals. The publication will be edited by Mr. B. B. Woodward, and the first part may be expected shortly.

WE are glad to observe that the "Société belge de Géologie" has now succeeded in bringing its *Bulletin* up to date. Two parts have lately appeared, the one completing the volume for 1892, the other being the first part for the current year. The Treasurer is able to report that the deficit, which for several years has crippled the Society, is steadily disappearing, and that the year 1892 showed a small surplus.

THE Bristol Naturalists' Society has issued a new part of its *Proceedings* (n.s. vol. vii., pt. ii., 1892–93), containing several papers of much value in reference to the Bristol district. There are records of Phenological and Meteorological observations, Mr. A. J. Heath and Professor Lloyd Morgan publish a concise account of the fossil fish-teeth and spines found in the Carboniferous Limestone, with some original observations; Dr. A. R. Prowse discusses the ancient British remains on Clifton Down, with a map; and Mr. C. Druitt has an interesting original paper on the Green Woodpecker, based on his own observations in the neighbourhood.

THE Croydon Microscopical Club has just issued its *Proceedings and Transactions* for Feb., 1892–June, 1893. The most original paper is that of Mr. Murton Holmes, who discusses "The Microscopical Structure of Hearthstone from Detchworth, Surrey." Foraminifera and casts of the same in glauconite, sponge spicules, coccoliths and rhabdoliths, are recorded, with abundant scales of mica. It is gratifying to learn that the loss on the last soirée was only £10; for the Croydon Club soirées are excellent as a rule, both in exhibitions and arrangement.

CORRESPONDENCE.

Phylogeny v. Ontogeny.

A chance has enabled me to see Dr. Hurst's reply to my article on "The Recapitulation Theory in Palæontology." Without reference to his original paper it is impossible for me adequately to examine his argument; but, that I may not be thought to ignore his defence, I venture to send these lines.

I am not so sure that I have misunderstood Dr. Hurst's position, despite his assertion that none of my missiles hit it. At any rate, it is clear enough now: he does not deny that, in many cases, individual development may follow the same lines as the evolution of the race; he merely denies that the past history of its race has any influence on the growth-stages of an individual. Considering the impossibility of producing experimental evidence, such a position is, in strict logic, impregnable. For, let us suppose that the ontogeny of every individual was an epitome of its phylogeny, it would still be open to anyone to deny any *causal* relation between the two. But, if I remember rightly, Mr. Hurst, in his original paper, maintained that the development of any individual was a regular progress from the embryo to the adult; modifications of early stages, adaptations to larval environment, might of course occur, but, on the whole, each growth-stage represented an approximation to the adult. Consequently, when race-history consisted in a regular advance from the primitive ancestor to the specialised descendant, then the individual development must normally repeat it; ontogeny and phylogeny would necessarily move in the same direction along parallel lines, but there would be no need to suppose any causal relation. But if the history of a race were no such regular advance; if, on the contrary, it could be clearly shown by the evidence of fossils that the race had ascended to an acme and then descended; if a structure could be seen to appear and disappear during this history; then it is equally clear that, on Mr. Hurst's hypothesis, ontogeny would not correspond with phylogeny; it is clear that vanished structures or forms would not temporarily make their appearance during the growth of an individual, unless indeed they were produced by some other cause, as adaptations to a temporary environment. This appeared to me to be Mr. Hurst's position, and the following was the nature of my attack. I adduced instances of structures or forms appearing in the earlier stages of individual development, and then disappearing; these characters were not such as could be supposed due to larval adaptation or to any modifying causes of a temporary nature; but these characters could be shown by palæontological evidence to have arisen and disappeared in just the same way in the history of the race. Now, I will merely ask Mr. Hurst to tell me what cause can have produced these deviations of ontogeny from the path of simple development. Unless some plausible explanation can be found, it seems reasonable to suppose that they are due to heredity, and are, in fact, as well as seeming, recapitulations of ancestral history. In Mr. Hurst's reply I see no attempt to meet these difficulties. He merely denies that difficulties exist, and asserts that my shots have all passed by him. Despite his explanations it seems to me that the only reason he has not felt my attack is, because he is encased in some bullet-proof cloth or other, which has a little dulled his perceptions.

It is needless to reply to Mr. Hurst's other remarks while the above question remains unanswered. Only with regard to the figures illustrating the ontogeny and phylogeny of *Antedon* which he suggests should accompany a fuller account in a

future number of NATURAL SCIENCE, it seems enough to refer those interested to the classical description by Dr. W. B. Carpenter (*Phil. Trans.*, 1879), and to the second of my own papers on British Fossil Crinoids (*Ann. Mag. Nat. Hist.*, 1890).

Townsville, Queensland, 20 June, 1893. F. A. BATHER.

"RECAPITULATION" AND "EARLIER INHERITANCE."

IN reply to Mr. Buckman's remarks on pp. 138 and 139, let me beg him to be more careful in future.

I have never expressed any opinion whatever as to what views he had adopted either "as creeds" or otherwise, nor as to the nature of the "inspiration" under which he has written, and I have not either on p. 198 (vol. ii.) or elsewhere denied that there is "any" causal relation between ontogeny and phylogeny. P. 197 (vol. ii.) treats, as there distinctly stated, not of changes in individuals but of changes "in average constitution of successive generations" leading to production of new species. The word "structure" was used deliberately on p. 197 (vol. ii.), and in its strictest sense. I did not mean "character," I meant that complex abstract quality made up of *all* the characters together.

The "law of earlier inheritance" is on a par with the law of the north wind. Because characters are in *rare and exceptional cases* (*see* Darwin, "Origin," 6th edition, p. 10) "inherited" earlier in the offspring than in the parent, are we to have a "law" enunciated to the effect that the rare and exceptional is not rare or exceptional but universal? The "law" of the north wind states that all winds at all times and in all places blow from the north. Many authors might be quoted to prove that a north wind had occasionally been observed in some places. The "law" stands on as good a basis as does that of "earlier inheritance."

I used the term "late stages of development" believing that it would be clear from the context that I meant stages which, though late relatively, are passed through before the adult stages are reached.

The "contention" concerning loss of teeth and other senile changes does not affect me. It is at most a matter of words, and probably everybody agrees with both Mr. Buckman and myself that the words may properly be used in the way he has "contended" for.

By "acceleration of development" I mean a change in the average constitution of successive generations of a species by virtue of which the descendants of to-day pass more rapidly through some or all of their ontogenetic changes than did their ancestors. Is another meaning for the term in use?

Physiologically, of course, the term is applied to the shortening of the period of development by the artificial application of heat, etc., *e.g.*, in Weismann's experiments upon seasonal dimorphism ("Studies in the Theory of Descent," vol. i., p. 120, last paragraph).

I regret that the *Proc. Cotteswold Nat. Soc.* are not accessible to me, and I have therefore not read Mr. Buckman's "Some Laws of Heredity." I hope I may yet have an opportunity of doing so.

C. HERBERT HURST.

THE CLASSIFICATION OF ARACHNIDS.

IN a paper on some recent memoirs on "The Classification of Arachnids," by Mr. G. H. Carpenter (NATURAL SCIENCE, no. 16, June, 1893), it seems to me that the author has sadly neglected the work of at least one writer on the same subject; leading the unwary reader, if I am not mistaken, to conclude that many of the theories mentioned in that review are novelties brought forth for the first time, within the last year or two, by Mr. Pocock, whereas they are really the views published twelve years ago by Professor E. Ray Lankester in a classical paper not even mentioned, namely "Limulus an Arachnid" (*Quart. Journ. Micr. Sci.*, vol. xxi., 1881).

Now, I venture to think that in a review of that kind, intended presumably to be read by students and others not intimately acquainted with the literature of the

subject, the greatest care should be taken to avoid conveying, by implication or by direct statement, an erroneous idea as to who was the discoverer of any new fact of importance or the originator of a theory. For instance (I am speaking, of course, of the review, and not of Mr. Pocock's valuable paper), it is stated, page 448, that "On these grounds he [Pocock] considers the scorpions as the lowest living branch of the arachnid stem, in opposition to the views of Thorell and others, who have regarded them as the highest. There can be no doubt that Pocock's opinion will meet with general acceptance among biologists. Deriving the arachnid orders immediately from an ancestor with a long abdomen of twelve segments and a telson, Pocock indicates the modifications which have probably occurred in each group . . . nearly related to the Pedipalpi . . . are the Solifugæ . . . , on a higher branch . . . come the harvest men (Phalangida) . . . from these the mites (Acari) . . . a degraded offshoot." Turning to Lankester (*loc. cit.*, p. 644) we find, "the Scorpions, having once been developed, appear to have given rise to the whole series of living Arachnida, to the Pedipalpi first, and through these to the Araneina, and through the Araneina to the Acarina . . . we have to admit a very extensive process of degeneration . . . leading from the Scorpion to such Acarina as Demodeæ . . ."

In the discussion (p. 450) on the light thrown by palæontology "upon the question whether the lung-bearing or tracheate orders of arachnids are the more primitive," no notice whatever is taken of the mass of evidence collected by Professor Lankester showing the relationship between the Scorpions and the extinct Eurypterina and Trilobita.

On page 449, we are led to believe that Mr. H. M. Bernard originated the view that the "spinning-glands, coxal glands, and poison-glands" were derived from the "ventral setiparous glands of an annelid ancestor," a theory put forth by Eisig in his famous Monograph of the Capitellidæ.

The Museum, Oxford. E. S. GOODRICH.

"UNILATERAL" SLEEP IN ANIMALS.

THE late Sir Emerson Tennent, in his "Natural History of Ceylon" (1861) at page 279 observed that each eye of the Chameleon (*Chamæleo vulgaris*, Daud.) had not only a separate action quite independent of the other eye, but that also one side of its body appeared sometimes to be asleep while the other side was vigilant and active.

Hence, while one side of the Chameleon was green, the other was red.

It is said that the Chameleon is unable to swim because the muscles of its two sides cannot act in concert. Is this true?

Similarly some other accurate observers, including Matthias Dunn of Mevagissey, suspect that certain fish can secure partial sleep with one eye at a time.

I should be grateful for further information and references as to whether other animals have been observed in this alleged condition of one-sided sleep while the other side of their body continues awake.

30 Sussex Square, Brighton. J. LAWRENCE HAMILTON, M.R.C.S.

TO CORRESPONDENTS.

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

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NOTES AND COMMENTS.

The Organisation of Science.

ONE of the most gratifying announcements of the month in British Science, is a statement by Lord Rayleigh to the effect that the Royal Society of London has lately appointed a committee to consider, in an entirely impartial manner, whether or not the procedure of that body can be altered with advantage. His lordship's speech was made before Section A of the British Association, in the course of a discussion on the publication of scientific papers. The physicists have been much excited of late on the publication question, and it was thought that a debate at Nottingham might do something further towards a solution of difficulties.

The anonymous author of a little pamphlet "On the Organisation of Science," which we reviewed in June, 1892, seems to have been the first, in recent years, to issue a reasoned plea for a more systematic method of publication of scientific papers, and the centralisation of the publishing authority; but as we pointed out at the time, and as Lord Rayleigh remarked at Nottingham, there is the great difficulty of censorship and the fear of a dominating clique. One of the essentials for progress is absolute freedom; and so long as there are competitive journals and rival societies, there is little danger of any novel views being suppressed merely on account of their unorthodoxy. The physicists and mathematicians of the British Association seem to have been agreed that there is no real necessity for interference with the "disorganisation" of present arrangements; but that there ought to be some recognised central authority for the periodical issue of volumes of abstracts relating to each branch of Science. This is the work that Professor Rücker and some others would like to see undertaken, or at any rate supervised, by the Royal Society; and the committee will certainly confer a great boon upon

workers if they are able to make any suggestions for rendering records of progress more systematic and elaborate than they are at present.

Scientific Linguistics.

When a layman asks a naturalist why he invents and employs such a multitude of incomprehensible technical terms, the common reply is that exact ideas necessitate a precise and universally-understood nomenclature. We wonder how this explanation would apply to the terms of "Auxology" or "Bioplastology" just discussed again by Professor A. Hyatt in the *Zoologischer Anzeiger* (concluded August 28). We should like to know how much scientific precision there is in the determination of the nepionic, metaneonic, gerontic, parageróntic, etc., stages of any organism; and what grain of solid fact, as compared with mere speculation, in the so-called definition of the phylonepionic, phyloneanic, phylogerontic, etc., phases of development in any group of animals. We may be enslaved by some prejudice, and our patience may have been ruffled in the attempt to decipher some recent writings of American authors on fossil shells; but we cannot help uttering a protest against the clothing of a tissue of hypothetical fabrications in the garb of a precisely-defined scientific nomenclature. It is, of course, a matter of everyday knowledge that each organism passes through several marked stages in its individual development; and most naturalists will admit that a good deal of evidence is constantly being discovered as to the evolution of races, genera, and species in course of time. Nevertheless, there is as yet nothing very exact in this knowledge; and until that exactitude is reached, the invention and application of scientific terms is a delusion and a snare, and a veritable hindrance to progress.

The embryologists need not be considered in the matter; for they do not seem to have taken kindly to the classification of stages of development by those whose practical acquaintance with the study of embryos is, for the most part, probably *nil*. The latest work on the subject (Marshall's "Vertebrate Embryology") does not even mention that any such classification has been attempted. When they begin to discuss protembryos, mesembryos, metembryos, etc., it will be time to return to the subject, and examine the cogency of their reasoning.

It is with the speculative palæontologists that we have to deal. It is they who arrange ammonites and brachiopods in rows, and unfold to us, with surprising confidence, the history of a genus, a species, or a race. The manner in which they are studying growth, variation, and the stratigraphical sequence of forms, by the examination of enormous collections, is one of the most gratifying signs of the times. This is certainly the only method by which the modern ideas of Biology can be advanced. It ought, however, to suffice at present to record clearly and simply the facts, carefully distinguishing all legitimate suggestions as to their likely explanation; and, to our mind,

it is one of the greatest misfortunes to entomb and obscure these plain facts in an array of would-be scientific expressions, which are nothing more than the outcome of the guesswork of each individual author.

If there is any logic in this attempt at a nomenclature, why do not Messrs. Hyatt, Beecher, Buckman, Bather, and its other advocates complete its definition in general terms? Why do they not show how to correlate the growth-stages, for example, of a brachiopod with those of a mollusc, and these, again, with those of a mammal? How do they determine which period in the life of a snail is scientifically equivalent to one of a crab? If no reasoned answer to these questions can be given, then, assuredly, the time has not yet arrived for applying technical terms to the various periods of the life-cycle.

In certain quarters, indeed, Biology seems to have reached a phase equivalent to that in which the originators of the Geological Society of London found the "Theory of the Earth" in 1807. That Society was established to combat the spirit of the day, "to multiply and record observations, and patiently to await the result"; and it was at first their favourite maxim "that the time was not yet come for a general system of geology, but that all must be content for many years to be exclusively engaged in furnishing materials for future generalisations." The older Societies dealing with Biology in Europe are wise enough still to withhold their patronage from any such reckless developments of "Auxology" and "Bioplastology" as we find in some magazines and transactions elsewhere; and the sooner the false appearance of this "whited sepulchre"—this clothing of pure hypothesis in a garment of precise terminology—is demolished, the more conducive will it be to sound progress.

Lake Superior.

An important geological memoir bearing upon the question of the local instability of the earth's crust has been issued by the Survey of Minnesota, U.S.A.[1] The evidence of changes of level on oceanic coast-lines is, of course, easily enough observed; but nearly everywhere inland very little progress has hitherto been made in detecting such movements. When, however, the area is large enough, recent unequal changes of level can usually be discovered by the study of the topography, such as the nature of stream-erosion, the distribution of sediment, and the deformation of the abandoned shore-lines of lakes. It is from this point of view that Professor Lawson has studied the old beaches to the north of Lake Superior.

It is now generally recognised that streams are very sensitive to any change in the slope of their trenches or of any portion of them.

[1] A. C. Lawson, "Sketch of the Coastal Topography of the north side of Lake Superior, with special reference to the Abandoned Strands of Lake Warren (the greatest of the late Quaternary Lakes of North America)," *Geol. & Nat. Hist. Surv. Minnesota, 20th Annual Report*, year 1891 (1893), pp. 181–289, with illustrations.

There is a uniform minimum slope which they constantly seek to establish and maintain. Any movement of the land leaves its record in a change in the intensity of the action of the stream, whether it be cutting or depositing; and in non-glaciated regions streams are now systematically examined by geologists for the purpose of ascertaining whether the country traversed by them has been uplifted or depressed, or has maintained a fairly constant altitude. Evidence of this kind, however, is not always available, and it is necessary to turn to other features. For instance, there are some regions in which the shores of ancient lakes can be traced for many miles—in the centre of North America sometimes for hundreds of miles. These old beaches must, of course, have been originally horizontal, and if, therefore, any one of them appears now to be displaced, variously inclined, and at somewhat different levels in different parts, there is clear evidence of movements of the crust since the lake dwindled or disappeared. Such evidence of earth-movements has been known for some years in North America on either side of the Lake Superior basin,[1] and Professor A. C. Lawson's investigation of the old beaches to the north of the present Lake Superior was undertaken with the object of ascertaining how far the effect of movements could be traced in that region. A beautifully illustrated detailed report leads to the conclusion that there once existed an enormous "Lake Warren" (to use Professor Spencer's term) probably at least twice as great as the combined areas of the present lakes Superior, Michigan, and Huron, or about 150,000 square miles. In some places as many as nineteen distinct beach-lines can be traced along the northern shore of Lake Superior, and Professor Lawson believes there is evidence of at least thirty-two definite stages in the recession of the lake. It seems probable that the lowering of the water was gradual, though perhaps varying in amount at different times; and it is hardly possible to believe that the existence of ice barriers had any connection with the phenomena. It is likely, therefore, that the local warping of the earth's crust, in some region far removed from Lake Superior, is accountable for the lowering of the land barrier which held back the waters of "Lake Warren." The outlet may even have been at different places at different times in consequence of the unequal continental warping of which there is evidence elsewhere. Professor Lawson enumerates the possible situation of some of these outlets, and makes many suggestions that will be of much service to future observers, both in this and other regions.

The Feeding of Fishes.

It is well-known that the sense of smell in fishes is very keen, and that all use it more or less in feeding, whether or not sight aids

[1] *See* "Great Lakes," by Clement Reid, NAT. SCI., vol. i., p. 117.

them in the process. Some further experiments on the subject have been made by Mr. Gregg Wilson in the Plymouth Marine Biological Association, and the following observations from his recent report to the British Association will be read with especial interest :—

"So far as I could determine, fish that are not *very hungry* habitually smell food before taking it. The pollack seems usually to be ready for a meal, and on almost all occasions when anything eatable is thrown into the tank in which it is swimming, it rushes towards it and bolts it. It does not hesitate to take stale food or food that has been steeped in strong smelling fluids; and time after time I have been amused to see its too-late repentance, after it had swallowed clams that had been saturated with alcohol, chloroform, turpentine, etc. It is only when it is satiated with fresh food or disgusted with what is nauseous that it takes the precaution to smell before eating. On the other hand, various fish that are equally keen-sighted, and habitually recognise their food by the use of their eyes, are more prudent. The whiting (*Gadus merlangus*), for instance, appears to pay much more attention to smell, and, as a rule, turns about and withdraws on approaching within a few inches of high-smelling objects that the pollack would take without hesitation. Even whiting, however, cease to be delicate if they are very hungry, and if other fish are present to compete for the food that is thrown to them. In such circumstances bait that is very distasteful may be taken by even the most cautious of sight-feeders; and likewise, in such circumstances, a quite smell-less artificial bait may be successfully employed. Where large shoals of fish are, there are likely to be many that are very hungry, and the consequent keen competition will lead to hasty feeding by sight alone; and hence it is, probably, that lead-baits are successfully employed in cod fishing in the Moray Firth and off the Northern Islands, while they are of no avail among the scanty fish further south.

"It may be said that in these cases the fish actually *search* for their food by sight alone, and merely test the quality of what they have found by smelling it; and Bateson quite recognised this. But more is possible: *habitual sight-feeders can be induced to hunt by smell alone*. The pollack, which is such a pronounced sight-feeder that it will take a hook baited with a white feather or a little bit of flannel and trolled along the surface, is yet able, when blinded, to get his food with great ease. Several blind specimens in the Plymouth tanks were carefully watched by me; and I had no difficulty in deciding that it was by smell alone that they found their food. Their conduct was exactly such as was seen in the smell-feeders, to which I shall presently refer.

"Again, the cod (*Gadus morrhua*), which Bateson puts among the sight-feeders, is generally believed—and with good reason, I think—to feed more by night than by day; which suggests that it, too, not only tests its food, but actually hunts by smell.

"Lastly, in this connection I would state the results of my experiments. I worked with a number of fish, and always with the same success; but I shall here only refer to one case—that of the dabs (*Pleuronectes limanda*). That they were sight-feeders was evidenced by their behaviour when I lowered a closed tube full of water, and with a worm in the middle of it, into their tank; time after time they bumped their noses against the glass at the very spot where the worm was situated. That they could also recognise the smell of food, apart from seeing it, was demonstrated in various ways. First, if instead of a closed tube, as in the last-mentioned experiment, one open at the bottom was used, after a short interval the nosing at the part where the worm was seen ceased, and *the lower end of the tube*, from which, doubtless, worm-juice was diffusing, was vigorously nosed. If, again, instead of putting worms into a tube I placed a number of them in a closed wooden box with minute apertures to let water pass in and out, there was a similar excitement produced, and the dabs hunted eagerly in every direction. When water in which many worms had lain for some time was simply poured into the tank through a tube that had been in position for several days, and by a person who was out of sight of the dabs, the results were most marked. In a few seconds hunting began, and in their excitement the dabs frequently leapt out of the water, apparently at air-bubbles, and on one occasion one even cleared the side of the tank, which was about two inches above the water, and fell on to the floor of the aquarium. Yet there was nothing visible to stimulate this quest."

The Thyroid Gland.

In the August number of our excellent contemporary, the *Medical Magazine*, Mr. Lorrain Smith has an interesting paper on the thyroid gland, which, as everyone knows, is the fashionable subject in current Pathology. It has been shown recently that many glands in the mammalian body have, at least, a double function: they form their special secretion as the liver, for instance, forms bile, or the kidneys remove nitrogenous material from the blood; but in addition they have some part to play in the general economy of the organism. Their loss by disease or excision not only prevents the formation of the special secretion, but disturbs the general economy of the body. The thyroid is a gland limited apparently to the second form of activity. This activity is at present being sought for: some results go to connect it with the regulation of heat; others with the general metabolism of the body. So far physiologists have not paid much attention to the comparative anatomy and development of the thyroid, but, without question, this side of the subject will raise problems of very general interest. It is, for instance, most probable that the thyroid was originally a slime-secreting organ correlated with the

mode of life of pelagic forms: it secreted slime to gather together the small organisms in the intaken sea-water. The gradual change of function of any organ is a biological problem of vast interest, and here seems a case to be dealt with by the morphologist and the physiologist hand in hand.

The Polynesian Islands.

Under the title of *Flora of French Polynesia*, M. Drake del Castillo gives a systematic account of the Flowering Plants and Ferns which grow spontaneously or are generally cultivated in the Society, Marquesas, Pomoton, Gambier, and Wallis Islands. The Society Islands are by far the most important, whether we consider extent, population, and productions, or flora, and of these Tahiti holds the first rank.

With but slight differences, the climate of French Polynesia is the same as that of the other Polynesian islands. In the Society Islands, the mean annual temperature is a little above 75° F. The year is divided into two seasons of almost equal duration, determined by the direction of the wind. From August to October a S.E. wind, called *Maarama* by the natives, prevails, and the weather is rather dry, though storms are not rare. A N.E. wind rules from November to March, and rains are frequent. However, the difference between the two seasons is not strongly marked, and it rains more or less during the whole year. The climate is moister in the high valleys than near the shore, and there is a corresponding difference between the vegetation of the two regions, while the eastern side being more exposed to the storms brought by the S.E. winds, the limit of the humid zone is lower than on the western side.

The Marquesas are drier than the Society Islands. These conditions of climate necessarily produce a luxuriant vegetation, which is, however, more brilliant than varied, and remarkable more for the development in number of individuals than species. The poverty of the flora chiefly consists in a want of special forms, a characteristic of small islands. Annuals are poorly represented, almost two-thirds of the whole vegetation consisting of small perennial shrubs, while trees and large shrubs constitute another third. This predominance of shrubby plants is explained by the nature of the soil. On the abrupt sides of the valleys, woody plants with short stems and vigorous roots, and ferns with creeping rhizomes, are almost the only kinds which can profit by the scanty support afforded by the soil. Tall trees occur for the most part only in the lower valleys or in the ridges. Feebler plants can only get on by the help of their neighbours, which they use as supports or hosts; hence it comes that 15 per cent. of the vascular plants are climbers, parasites, or pseudo-parasites. The few herbs live on the borders of streams or dry hills. Owing to its rocky nature and steep slopes in the mountainous islands, the soil retains but little of the large amount of water it receives,

while the narrow deep valleys keep the air constantly charged with moisture. Thus, if the roots absorb little, the leaves exhale but little, a fact which explains the chartaceous or coriaceous nature of the foliage of many species.

M. Castillo remarks that the flora of French Polynesia is a poor one, containing, according to our present knowledge, only 588 species of vascular plants distributed among 79 families, and representing 262 genera. They are classed under three headings. First, species confined to the islands, numbering 161; secondly, species distributed throughout Oceania, 123; and thirdly, comprising a half of the flora, or 297 species, those which are also found in the Indo-Malayan region. The predominating families are, Ferns, Leguminosæ, Orchids, Rubiaceæ, Grasses, Cyperaceæ, Euphorbiaceæ and Urticaceæ. The predominance of ferns—with their light and easily-carried spores—over the less easily dispersed flowering plants is a well-known characteristic of insular floras, consequently it is not surprising to find this group represented by as many as 142 species, or nearly one-fourth of the whole. There are 37 species of Leguminosæ, 34 Orchids, 31 Rubiaceæ, and 30 Grasses. A *Freycinetia* is specially mentioned as very plentiful and covering large areas, and along with the plaintain (*Musa paradisiaca*), a plant of a very different type, recalling the vegetation of a Malay or East Indian jungle.

Lake Vegetation.

In two recent numbers (nos. 54 and 55) of the *Revue Générale de Botanique*, A. Magnin discusses the vegetation of the lakes of Jura. After exploring a number of lakes, the regularity with which the plants are distributed becomes very striking. Passing from the shore to the centre, distinct zones are noticed somewhat as follows.

1. A littoral zone of reeds (*Phragmites communis*) and bulrushes (*Scirpus lacustris*), which stand out of the water. Sometimes the two plants are more or less intermingled, but usually the reeds alone line the shore, forming the *Phragmitetum*, extending to a depth of 2–2½ metres, while the bulrushes form a second ring, or *Scirpetum*, to a depth of 3 metres, a mixed zone uniting the two. Sometimes the order is inverted through the formation of a bar beyond the shore, the reeds installing themselves in the shallower water. Besides these, where the water is shallow, sedges, *Phellandrium*, *Equisetum limosum*, and others are observed, as well as some plants with swimming leaves, like *Polygonum amphibium*, the white water-lily (*Nymphæa alba*), and *Potamogeton natans*.

2. Beyond the bulrushes plants with stems standing out of the water are no longer found, but only those with swimming or floating leaves. Especially characteristic of this zone is the yellow water-lily (*Nuphar luteum*), which occurs in almost all the lakes of the Jura, frequently forming a large continuous girdle, or great patches, from the limit of the scirpetum to the edge of the first slope to a depth of 3–5,

usually 4, metres, and constituting the *Nupharetum*. Sometimes it is associated with species of the preceding zone, chiefly *Potamogeton natans*, while the water milfoil (*Myriophyllum*), *Potamogeton perfoliatus*, and others of the following zone begin to appear.

3. The third zone, or *Potamogetonetum*, is formed by plants different according to the lake; sometimes pondweeds, chiefly *Potamogeton perfoliatus*, then *P. lucens*, or *P. crispus*, less often *Myriophyllum spicatum*, or still more rarely the mare's-tail (*Hippuris vulgaris*), all bearing their leafy and flowering branches at or near the surface, on stems 4 to 6 metres long. Their rootstocks occupy the edge of the slope, below the yellow water-lily, at a depth of 4–5 metres, descending to a depth of 6 and 7 metres. Here also lives the hornwort, *Ceratophyllum demersum*, which, after wintering in depths of 3 to 6 or even 8 metres, breaks loose and becomes quite free, floating like the bladder-wort (*Utricularia vulgaris*).

4. The fourth or deep zone, the *Characetum*, is formed by plants which never reach the surface, but remain always fixed to the bottom at a depth of 8, 10, and 12 metres. Such are the numerous Characeæ, *Naias major*, and the mosses *Fontinalis antipyretica* and *Hypnum giganteum*.

In deep lakes with rocky borders, plant-life is absent or represented only at points where erosion or a falling has occurred by tufts of reeds or bulrushes, associated or isolated, and sometimes followed inside by the yellow water-lily and *Potamogeton perfoliatus*. The turf-lakes with abrupt, sometimes sharply-sloping margins differ in having a very narrow littoral zone, consisting of the Bog-bean, (*Menyanthes*), the reed mace (*Typha*), *Cladium*, the reed, and the bulrush, while the yellow water-lily reaches almost to the edge. The floor is generally carpeted, to a great extent, with myriophyllums and charas, together with, in some lakes, the hornwort, the two mosses above mentioned, *Nitella* and others.

Shallower lakes with muddy or marshy banks with but a slight incline, and alternately swamped or exposed, show a very variable vegetation. In some can still be seen the littoral zone, the yellow water-lily zone, and a bottom covered with myriophyllums and charas; in others mare's-tail and water milfoil clothe the whole lake, while scattered patches of *Nuphar*, *Polygonum amphibium*, and others are seen at the surface.

Classification of Conifers.

The most recent issue of the Linnean Society's *Journal* (vol. xxx., no. 205) contains some valuable notes by Dr. Masters on the genera of the two large orders of Gymnosperms, Taxaceæ and Coniferæ. The observations are the outcome of a comparative examination of the morphological characters of all the genera as far as was possible in living plants, while the available museum and herbarium specimens have been studied as well as the literature. Dr. Masters is well-

known as an authority on the subject, and the notes are a valuable addition to the work he has already published on Conifers and Taxads. The orders, tribes, and genera are carefully defined, and the history of each genus as well as the morphology of the vegetative and reproductive organs is discussed. Constant reference is made to the schemes of arrangement of the older writers, and, more recently, of Eichler, Van Tieghem, and others, but, in the main, the author has followed Bentham's plan, as elaborated in the "Genera Plantarum." Several deviations from the last arrangement claim to be "more in harmony with ascertained facts, or with more recently-acquired information." Thus, the Oriental genus *Cephalotaxus*, which Bentham places among the Taxodineæ, forms with *Gingko* and *Torreya* a distinct tribe, Salisburineæ, of Taxads. The drupe-like seed, and the germination, recall those of *Gingko*, and another point which separates both from all other Conifers is the presence of a resin-canal in the centre of the pith, observed by Van Tieghem. That author is also responsible for the genus *Stachycarpus*, a name originally proposed by Endlicher for a section of *Podocarpus*, to include those species where the peduncle does not become fleshy. The characteristic of the new genus is the arrangement of the fruits on a loose spike, the axis of which does not become fleshy. There are also distinctions in minute structure.

An important deviation from Bentham's arrangement is the splitting of the genus *Callitris*. In the "Genera Plantarum" this includes a species from North Africa, a few from Southern and Tropical Africa, and a number from Australia. Dr. Masters separates the North African plant as a new genus *Tetraclinis*, distinguished by its flattened (not triangular) Salicornia-like branches, and its solitary female cones, the axis of which is not prolonged beyond the four scales. Endlicher's *Widdringtonia* is revived for the other African and Madagascar species with angular branches, panicled female cones, and four unequal scales, while *Callitris* is reserved for the Australian species, the genus *Frenela* of Mirbel differing from *Widdringtonia* in having six scales in the female cone. Another genus of Endlicher, *Glyptostrobus*, included by Bentham, following Brongniart, in *Taxodium*, is restored.

In the tribe Abietineæ, which includes the spruces, cedar, larches, firs, pines, and their allies, Dr. Masters recognises the same genera as Bentham, except that Carrière's *Keteleeria* is revived on the ground of Professor Pirotta's recent observations on the male flowers and other remarkable characteristics. It contains, besides the original *Abies Fortunei*, a few other Chinese species.

The paper concludes with an account of the geographical distribution of the genera. China and Japan are remarkable for the number and variety of their Taxads and Conifers; two genera *Pseudolarix* and *Keteleeria* are confined to China, *Cunninghamia* extends to Cochin China, while *Gingko* is of Chinese origin, but widely cultivated in Japan. *Sciadopitys* is exclusively

Japanese. In North America the rocky mountains form a marked dividing line, the species to the west and east being, though closely allied, for the most part different. No true Conifers are found on the Andes, though the range forms, as it were, a continuation of the Rocky Mountains; their place is taken by Podocarps and Taxads working up from the south. *Juniperus*, *Libocedrus*, and *Pinus* are the only true Conifers found both north and south of the Equator, while among Taxaceæ, *Podocarpus* and *Dacrydium* occur in both subdivisions. Junipers, cypresses, spruces, firs, and pines spread across the northern hemisphere from west to east, but there is no such continuity in the south among true Conifers, though *Araucaria* is common to Eastern South America, Chile, Australia, New Zealand, and some of the South Sea Islands.

In the recent issue of the *Trans. and Proc. New Zealand Inst.* (vol. xxv., 1892) T. Kirk describes the heterostyly, or differences in length of the style or stamens of flowers in relation to reciprocal fertilisation in the New Zealand Fuchsias. Each of the three species, *Fuchsia excorticata*, *F. Colensoi*, and *F. procumbens*, is trimorphic, having a long-styled, mid-styled, and short-styled form. The arrangements for cross-fertilisation are much less complicated than in our own purple Loosestrife (*Lythrum salicaria*) as the long-styled flowers are, at any rate in the first two species, practically female, the pollen-grains being abortive.

The three forms of *Fuchsia excorticata* grow intermixed, usually in about the same proportion, wherever the plant is plentiful, the long-styled form producing the largest quantity of fruit, a fact which, it is suggested, is largely due to the application of pollen from both the mid- and short-styled forms. This assumption is supported by the great reduction in the quantity of fruit on the long-styled form in the few observed cases where one of the others appeared to be absent. The author knows of no instance of the different forms of *F. procumbens* growing intermixed; only one being found in any one locality. This, he says, may well account for the fact that the handsome fruit of this species has not been seen in the wild state. The mid- and short-styled forms are often cultivated, but were never seen in the same garden. Under these conditions although most frequently sterile, they do occasionally fruit. Hence it is inferred that each form of flower is almost sterile with its own pollen.

Dr. E. v. Halácsy spent last June in a botanical exploration of some of the North Peloponnesian mountains, and his collections may be expected to yield results of interest. The characteristic elements of the different plant formations were noted, and the upper and lower limits of the vegetative zones determined. Of special interest is the discovery of a Berberidaceous plant on the northern

slope of Panachaion, most nearly allied to *Lemtice Altaica* from the Altai mountains, and probably representing a new genus. On Mt. Olenos Dr. Halácsy found above the fir region a girdle of large trees of *Juniperus fœtidissima*. The high mountain flora at the edge of the snow-fields made a gorgeous display, especially in the heights of Chelmos, where *Ficaria Peloponnesiaca*, *Anemone olanda*, Crocuses, Scillas, the endemic violet (*Viola Chelmiani*), and others, formed brilliant carpets, but nowhere were the species found which characterise the Austrian alpine flora.

Biologists, as a rule, are apt to overlook the physical aspect of the facts and phenomena with which they deal, and it is therefore gratifying to observe the occasional incursion of the Physicist into the Biologist's domain, such as has just been made by Professor Johnstone Stoney. The Professor asks (*Sci. Proc. Roy. Dublin Soc.*, n.s., vol. viii., pp. 154–156): whence comes the vital energy of the innumerable bacilli which are excluded from the direct influence of sunlight, and why are these organisms all extremely minute? There are some bacilli, *e.g.*, the nitrifying bacilli of the soil, which seem to thrive entirely upon mineral food, and, not only so, but perform their functions while completely removed from the influence of the sun's rays. The manufacture of protoplasm and other complex compounds from inorganic materials involves a considerable amount of energy, and the bacilli must somehow obtain this from the surrounding gases and liquids. Professor Stoney regards it as conceivable that the energy may be imparted to the organisms directly by the impact of the more swiftly moving molecules of these gases and liquids; and if that be the case, then the necessity for the excessive minuteness of the bacilli—scarcely more than molecules—is explained.

Our knowledge of the more minute structure of the Protozoa has considerably increased of late, as the result of improved methods and apparatus for study; and the protoplasm of an *Amœba*, for example, now appears as something far more than mere jelly and granules. It is now possible even to cut sections of these minute organisms and to stain them in such various ways as to distinguish a most elaborate arrangement of parts in many cases. Last year Dr. F. Schütt published in Berlin the result of some studies of *Peridinia*, in which he not only demonstrated the existence of remarkable spaces in the protoplasm, but a true excretory system in connection with them; and another investigator (Professor Greef) supposes that in sections of *Amœba* he can see striations denoting the arrangement to which the contractility of the animal is due. Mr. J. E. S. Moore is now following up these investigations in the Huxley Research Laboratory at South Kensington, and the last number of the Linnean Society's Journal

(*Zool.*, vol. xxiv., pp. 364–368, pl. xxvii.) contains some of his first results. Mr. Moore has examined sections of *Spirostomum* and *Paramœcium*, and detailed descriptions, with beautiful figures, are given. It would be premature at present to speculate as to the meaning of the various structures, but it is evident that a new line of research of great biological importance has been inaugurated.

THOSE who are interested in the progress of the American theories of mechanical evolution, ought not to overlook Professor John A. Ryder's papers on "Energy as a Factor in Organic Evolution," and "The Mechanical Genesis of the Form of the Fowl's Egg," in the last number of the *Proc. Amer. Phil. Soc.* (vol. xxxi., pp. 192-209). The arguments are ingenious and interesting, but we are by no means convinced that the phenomena of life are such simple, mechanical matters as Professor Ryder and his school suppose.

M. MAREY is still making progress with his study of the locomotion of animals, by means of photography. He has just published figures of the gait of a snake, a gecko, and a scorpion (*Comptes Rendus*, vol. cxvii., pp. 355–359).

A RED deer (*Cervus elaphus*) with ten points on each antler has lately been shot by Lord Burton in the forest of Glen Quoich, Scotland. This is apparently the most complex form of antler hitherto obtained in a Scottish specimen.

IT has long been known that the Tapirs—which are curiously restricted at the present day to the Malay Archipelago and to Central and South America—once had a very wide range throughout Asia, Europe, and North America. There are traces of these marsh quadrupeds in the Miocene and Pliocene rocks of Germany, France, and England, and in the Pleistocene of China; and quite a long series of ancestral types has been discovered in North America. A brief synopsis of all the known extinct forms has just been contributed to the *Geological Magazine* (Dec. 3, vol. x., pp. 391–396) by Mr. Charles Earle, and all who are acquainted with the tooth-structure and foot-structure of Ungulates will read this account with great interest. So far as known, the Tapirs occur a little earlier in Europe than in America, and the teeth of all the extinct forms are somewhat simpler than those of the species now living.

WHEN will Palæontologists cease to elaborate theories and try to amend long-respected "laws" on the evidence of mere scraps of animals? In a letter we publish elsewhere, Professor Hutton says that the supposed evidence of the occurrence of the New Zealand Moa in Queensland proves to be worthless—that it consists solely

in a thigh bone which is much more like that of an Emu or a Cassowary than that of a Moa. "There is thus no longer any reason for supposing a late migration of Struthious birds between New Zealand and Australia." At the same time (*Proc. Linn. Soc. N. S. Wales* [2], vol. viii., pp. 7–10) Mr. De Vis discusses a second Queensland fossil—the upper jaw of Owen's great extinct wombat, *Phascolonus*—and points out how another guess has been exploded. Some have supposed that the huge broad chisel-like teeth named *Sceparnodon* by Owen were the upper incisors of this animal, but Mr. De Vis's discovery now disproves the supposition. *Phascolonus* has very stout upper incisors, and the animal to which the broad chisel-like teeth belong is as mysterious as ever. It is a distinct advantage to have accurate descriptions and figures of fragmentary fossils, and it is often desirable to assign to them provisional names; but literature is already sufficiently burdened, and may well be spared the additional incubus of elaborate guesswork and prosy prophecy.

In reference to general principles touched by this kind of Palæontology, we desire to add a word of warning to students who come across Professor H. G. Seeley's note on a supposed reptilian tooth with bifurcated root in the last number of the *Ann. and Mag. Nat. Hist.* (ser. 6, vol. xii., p. 227). It is a generally accepted principle that the teeth of reptiles are distinguished from those of mammals by having invariably a simple—not bifurcated—root. There is, it is true, not much difference between the longitudinally grooved root of certain reptiles and the double-rooted premolars of typical mammals; but there is no certain exception to the rule as yet on record. Now Professor Seeley describes what he terms an abnormal tooth of *Nuthetes*, an extinct reptile from the Purbeck formation; and if the determination be correct, the discovery is of great interest. We must, however, point out that there is no evidence whatever that the tooth described does belong to *Nuthetes* or to any other reptile; it was found isolated, and in a deposit where mammalian remains occur. Moreover, it appears to us much like the upper canine tooth (with bifurcated root) of the little insectivorous mammal *Triconodon* found at the same place. There is thus so much uncertainty in this determination that the good old law remains unaffected, and we only regret that such comparatively worthless fragments should add further to the literature of palæontological guesses.

In the same paper Professor Seeley incorporates an interesting observation which could not be suspected to occur under the published title, and we therefore extract it. He remarks how the little Purbeck reptiles, *Nuthetes* and *Echinodon*, seem to be dwarfed representatives respectively of the Megalosaurian (carnivorous) and Stegosaurian (herbivorous) Dinosaurs—animals which in other formations are many feet in length. Sir Richard Owen long ago pointed out how

almost all the crocodiles from the division of the Purbeck Beds in question were dwarfs, and discussed the curious association of these predaceous creatures with the well-known dwarf mammals of the period. The further recognition of dwarf Dinosaurs existing at the same time is thus of especial interest. All of these are land- or freshwater-animals forming one fauna at one period in one and the same region (present Isle of Purbeck), and it seems very difficult to explain so remarkable an occurrence. The dwarfing of fishes is much more comprehensible from the circumstances of their environment; and it is curious to note that in one Purbeck stratum in Wiltshire all the fossil fishes are diminutive representatives of types that are comparatively large elsewhere.

It is curious to note how widely distributed are some of the great reptiles of the Jurassic period. The marine Ichthyosaurs, for instance, have been found in most parts of the world. Remains of one species were brought home by Sir E. Belcher from the islands between West Cornwall and North Devon in the Arctic Regions; and twenty years ago vertebræ and paddle bones of two other species were discovered by Baron Nordenskiöld in Spitzbergen. Throughout Europe and in North America remains are abundant; and detached vertebræ have been found near Mombasa in East Africa. Other evidence of these saurians has been discovered in Australia, and even in New Zealand as far south as the 45th parallel of south latitude; and now comes the welcome announcement of the occurrence of an Ichthyosaur at two localities in South America (W. Dames, *Zeitschr. deutsch. Geol. Gesell.*, 1893, pp. 23–33, pl. i.). Some dorsal vertebræ with associated fragments from the Tithonian Formation of Cienegita, in the Argentine Republic, are described by Dr. Dames as *Ichthyosaurus bodenbenderi*; and he records the discovery of two vertebræ and a paddle bone of the same genus from the Andes of Chili.

It is remarkable how new fossils are continually being discovered even in the most thoroughly explored strata. The fossil fishes of the Caithness flagstones have been diligently collected for seventy years, and yet it is only during the past summer that a specimen of the typically Lower Devonian genus *Cephalaspis* has been met with in these rocks in the neighbourhood of Thurso. The fossil was described by Dr. R. H. Traquair at the recent meeting of the British Association, and is now in the Edinburgh Museum. A full account of it is promised in a forthcoming volume of the *Proc. Roy. Phys. Soc. Edinburgh*.

It is interesting to note that two species of Ostracoda found in the Bear-River Formation (Upper Cretaceous) of Wyoming, have been identified, by Professor T. Rupert Jones, with forms discovered in this country—the one being *Cypris purbeckensis*, Forbes; the other

a variety of *Cypridea tuberculata*, Sow., named *wyomingensis* by Professor Jones (*Geol. Mag.*, Sept., 1893).

A. Hollick claims to have discovered a fossil palm-leaf in the Cretaceous formation at Glen Cove, Long Island, a discovery of some significance, as "other fossil leaves associated with it show the geological horizon to be the equivalent of the Amboy Clays of New Jersey and other middle Cretaceous strata in America and Europe, from none of which have palms been definitely recorded." The leaf, which is small, fan-shaped, and about three inches across, is made to rank as a new genus, *Serenopsis*, the name indicating its supposed ancestral relation to the Southern Palmetto (*Serenæa*).

The genus was established in the April number of the Bulletin of the Torrey Botanical Club, and the August number of the same journal contains some further information obtained from a second specimen recently found, and in a better state of preservation. It may be well to mention that Professor Lester Ward, whose opinion on the specimens was asked, does not think it a palm, "but suggests that it is more likely to be allied to the organisms which have been called *Williamsonia*, whose affinities are exceedingly problematic."

It has long been uncertain whether the typical Carboniferous plant, *Lepidodendron*, occurs so low as the Devonian rocks in Australia. Messrs. Pittman and David now seem to have settled the question by its discovery on Mt. Lambie, New South Wales (*Proc. Linn. Soc. N.S. Wales* [2], vol. viii., p. 121).

The idea that there were high alps with glaciers in the neighbourhood of central France when the Coal-measures of that region were deposited, finds favour with another geologist who has been studying the curious breccias and barren strata in the series (A. Julien, *Comptes Rendus*, vol. cxvii., 1893, pp. 344–346). The result is very interesting, and the conclusions seem far from improbable; but we are pleased to observe that Mr. Mark Stirrup, in his latest paper on the boulders from the Lancashire Coal-measures, does not attempt to infer the occurrence of glacial conditions from the comparatively insignificant phenomena in his region (*Trans. Manchester Geol. Soc.*, vol. xxii., 1893, pp. 321–331). Mr. Stirrup confines himself to the careful observation of the facts of the case, and Professor Bonney has minutely examined many of the pebbles from the petrologist's point of view. Besides the well-known quartzites, Mr. Stirrup has now discovered examples of granite, gneiss, and probably felsite, but all these rocks are seen to be much decomposed when examined microscopically. Professor Bonney thinks they could all probably be "matched" in Scotland, though nothing definite can be stated as to their derivation. It is now evident

that extraneous pebbles occur in the coal-seams and associated shales in many coal-fields both in Europe and America.

It seems to be proved that the Northern European sea of the Eocene period was separated from that of the Mediterranean area by a land barrier extending across France and Northern Germany. Hence it has been noticed that the fossil marine organisms to the south bear witness to highly favourable conditions for growth, while those in the north seem to have been stunted by the chilling influences of cold currents. This phenomenon is still further exemplified by a recent study of the British Eocene Bryozoa (J. W. Gregory, *Trans. Zool. Soc.*, vol. xiii., pp. 219–279, pls. xxix.–xxxii.), which are numerically small, both in species and individuals, compared with the wealth of forms that inhabited the contemporary seas of the Mediterranean basin. The northern species are also remarkably dwarfed. The land barrier seems to have been destroyed in middle Eocene times, but the conditions were not seriously modified until later. Incidentally, Dr. Gregory remarks that the species of Bryozoa are not so long-lived as some palæontologists suppose; he has no faith in the identification of Cretaceous species with any of those of existing seas.

The "parallel roads" of Glen Roy are still inspiring those who have the opportunity of studying glacial lakes, and Dr. Robert Munro last year kept the much-discussed Scottish phenomenon in mind while visiting Norway. He found a remarkable example of a glacier lake, formed by a branch of the Hardanger-Jökul, near Eidfiörd, and has now communicated a valuable detailed account of it to the Royal Society of Edinburgh. The paper appears in the last part of the Society's *Proceedings* (vol. xx., pp. 53–62), accompanied by a map. Dr. Munro concludes that the evidence of the existence of glacier lakes furnished by the so-called "parallel roads" of Glen Roy, corresponds, in its minutest details, with the facts observed in his study of the recent lake in Norway.

Various hypotheses have been started with reference to the Dartmoor Granite, but the old view of De la Beche, that it was intruded among the Devonian rocks and Culm-measures, into which it sent veins, is supported in a recent paper by Lieut.-General C. A. McMahon (*Quart. Journ. Geol. Soc.*, vol. xlix., pp. 385–395). Such veins have been observed by the author near Lydford, on the western flank of Dartmoor. He also drew attention to the pseudo-bedding of the granite, remarking that this conforms closely to the present slope of the surface of the hills. In his opinion it may be attributed to the action of the sun's heat and frosts, in rupturing the superficial portions of the granite, and thereby producing the pseudo-bedding and joints.

A new Cornish mineral has been determined by Mr. H. A. Miers (*Nature*, August 31). It is a hydrated sulphate and chloride of copper and aluminium, and it occurs in brilliant and translucent crystals of a deep emerald-green colour. In external characters and composition it is identical with Spangolite, of which only one example was previously known, and that was described in America, and obtained from a man living in Arizona. Mr. Miers hopes that search will be made among old collections, and especially upon copper ores from the St. Day Mine, near Redruth, for this is the locality whence this new specimen was obtained.

The report of the Director-General of the Geological Survey for the year 1892 occupies twenty-nine pages in the last Report of the Science and Art Department, and it presents a much fuller statement of the scientific work of the staff than has hitherto been made. Many new facts are thus for the first time announced, so that the volume is one to be noticed by compilers of geological records. There are fresh facts in reference to enormous boulders in the Drift, and on the occurrence of Eskers or Kames in the West of England. There are notes on the Hampshire Tertiary strata, on the Chalk and other Cretaceous rocks of England, and on the Jurassic rocks of England and Scotland. There is the latest official classification of the Devonian rocks, and many remarks on the volcanic and metamorphic rocks of Britain generally. Folds and thrust-planes are noticed, not only in the Highlands, but in the regions of Purbeck and Weymouth.

The Report of the Royal Commission on London Water Supply has been presented to the House of Commons. The Commissioners are strongly of opinion that the water, as supplied to the consumer in London, is of a very high standard of excellence and of purity, and that it is suitable in quality for all household purposes. With respect to the quantity of water which can be obtained within the watersheds of the Thames and Lea, they are of opinion that a sufficient supply to meet the wants of the metropolis, for a long time to come, may be found. From the River Lea, with adequate additions to the present storage, 52½ million gallons may be taken daily; and by the construction of storage-reservoirs in the Thames Valley, at no great distance above the intakes of the companies, it will be possible to obtain an average daily supply of 300 million gallons, without taking in any objectionable part of the flood-water. At present not half that amount is taken, but the scheme suggested can be carried out progressively to meet the increasing demands for water. A large supply of water might also be procured from the Chalk area in the basin of the Medway. In the opinion of the Commissioners a sufficient quantity of water to supply 35 gallons a head to a population of 12 millions could be obtained; and the population of Greater London

is not likely to exceed that amount during the next 50 years. The Report will therefore be read with satisfaction by those interested in London Water Supply; it is signed by Lord Balfour of Burleigh (chairman), Sir George Bruce, Sir Archibald Geikie, Professor James Dewar, Mr. George H. Hill, Mr. James Mansergh, and Dr. W. Ogle.

We learn from the *Zanzibar Gazette* and recent letters that in his recent ascent of Mt. Kenia, Dr. J. W. Gregory was stopped at a point 2,000 feet below the summit by a cornice of snow, which could not be surmounted except by a properly-equipped party of climbers. He established a baggage camp at the foot of the mountain, a reserve camp at 11,000 feet, and an upper camp close to the snow-line. The greatest difficulty was experienced in passing through the zone of bamboos, where advantage had to be taken of elephant paths. The succeeding forests proved very dense, damp, and cold, and it was a relief to emerge into the higher pastures. Dr. Gregory collected plants and insects, examined some large glaciers and determined their former much greater extension, and mapped the S.W. side of the mountain.

We regret to hear that during his exploration of the now active volcano, Adjuma-yama, near Fukushima, in Japan, a member of the Japanese Geological Survey lost his life, and that of his assistant, owing to a sudden explosion. Mr. S. Miura, the deceased geologist, was formerly science master in the Normal School at Saga in S. Japan, and had only recently joined the Survey. The mountain can now be visited with safety, as the explosions occur at regular intervals, morning and evening.

One could scarcely accuse a writer so well-informed as Mr. Joseph Hatton of carelessness but that under the heading "Cigarette Papers" in the *People* of September 3 appeared an extraordinarily absurd account of a gigantic squid. This is referred to as an "up-to-date fish of the cephaloptera species," with "long scaly arms," or as the writer puts it in another paragraph, "forty-foot antennæ." The remarks are made with regard to an account of the appearance of one of these gigantic molluscs to some sailors in the Gulf of Mexico, and whether or not the nonsense occurs in the American newspaper account, a reference to any encyclopædia would have enabled the English writer to avoid repeating so strange a tissue of absurdities.

The Anthropological Institute issued in August an "Index to the Publications of the Anthropological Institute of Great Britain and Ireland (1843-1891), including the Journal and Transactions of the Ethnological Society of London (1843-1891): the Journal and Memoirs of the Anthropological Society of London (1863-1871): and

the Anthropological Review." The volume consists of 302 pp., and is priced at ten shillings. In the main one alphabetical arrangement is followed, but why this is not carried out under the headings "Exhibitions," "Reviews of Books," etc., passes our comprehension. These items are apparently arranged in the order of publication, and the inconvenience of having to wade through 16 pages of index when hunting up the review of a book is obvious. The publication is extremely valuable, and is an example to several other London societies (*e.g.*, Geological and Geographical), whose publications have in part been indexed by Americans. The neglect to provide a general index for journals like the *Annals and Magazine of Natural History* and the *Geological Magazine* too, which has been running now since 1864, speaks little for the enterprise of publishers. In the last instance, we are informed, the work of preparation has been offered gratuitously, but the outlay on printing has been refused. The consequence is that many valuable papers are overlooked or forgotten, as a serious expenditure of time is entailed in searching through the indexes of thirty or forty volumes. They manage these things better abroad, for such publications as the *American Journal of Science* and the *Neues Jahrbuch für Mineralogie* are provided with general indexes at intervals.

An excellent portrait of the late Mr. James William Davis appears in the current number of the *Geological Magazine*.

The sixtieth anniversary of the birthday of Baron F. von Richthofen has been celebrated by the issue of a "Festschrift," a volume of 418 pages, illustrated with a portrait and maps. It consists of a series of essays, chiefly on geographical subjects, by the Baron's pupils. The seventieth anniversary of the birthday of Professor J. Victor Carus was marked by the issue of a beautiful portrait with the *Zoologischer Anzeiger* for August 14, 1893.

The *Mediterranean Naturalist* is now issued only at intervals of two months. The August number contains, among other matter, a plea for the establishment of a Museum of Natural History and Archæology for Malta.

We are always glad to note the success of any scheme that has for its object the infusion of a scientific spirit into collectors. It is thus a matter for gratification to observe that the small "monthly medium for collectors and students of Natural History" known as *The Naturalists' Journal* has just been enlarged. One enthusiast in the "exchange column" wants some Natural History books or specimens in exchange for a pile of literature of the "railway accident insurance" type. This looks like a conversion.

I.

The Effect of the Glacial Period on the Fauna and Flora of the British Isles.

THE question of the extent to which the flora and fauna of Britain was driven out of the country, or exterminated during the Glacial period is intimately connected with that of the southern limit of the ice. Geologists in general seem to favour the view that plants and animals were for the most part—if not entirely—expelled, and that our country had to be restocked from the continent. But there are several considerations which point to the possibility of the survival of some part at least of the pre-Glacial inhabitants of the land.

The ice during the Glacial period may have extended as far south as the latitude of London; very possibly it did not extend so far. Our opinion as to the survival or otherwise of our flora and fauna will be influenced by our view as to the southern extension of the ice. Let us take the former view first. This would leave the counties of Kent, Surrey, Sussex, Hants, Dorset, Somerset, Devon, Cornwall, and part of Wiltshire free from ice.

As far as *space*, then, is concerned, we have an area capable of affording an asylum to a considerable number of our plants and animals.

If, however, the Boulder Clay was not formed beneath the ice, then the latter probably did not extend so far south as the latitude of London, and the area fitted to form an asylum for our pre-Glacial flora and fauna increased.

As regards climate, again, it is to be remembered that ice-sheets and glaciers terminate in temperate latitudes when they do not reach the sea—if the latitudes were not temperate they would not terminate; and in connection with existing glaciers we find a temperate flora and fauna in close proximity to the ice, as in Switzerland, the Himalaya, and America. Further, the limits of the ice overlap the limits of temperate life; the latter ascends beyond the termination of the ice, while the former extends into the domains of the latter. Note, for example, how the lines marking the northern limit of forests and the southern limit of the ice overlap in North America.

Such facts lead us to admit the probability that the climate of the south of England during the Glacial period was such as to permit

the continued existence of temperate life and also to enlarge our conception of the area free from ice.

There are other considerations which strengthen this view. The southern limit of the ice, when found, marks its extreme winter extension; during summer a very much larger area of country would be free from ice; for it is to be remembered that, whatever theory of glaciation we accept—since those involving an absolute diminution of solar heat are almost universally discarded—we have to reckon with a summer heat-supply as great as that which we receive to-day. Consider, then, what would be the effect of setting our present summer to work on an England in which the ice reached as far south as London. We may surely assume the melting back of the ice for a considerable distance.

During the most intense Glacial period possible, according to the astronomical theory, our present heat-supply was concentrated into 166 days, but this would probably affect very little its melting powers; and according to Sir Robert Ball's data, the average temperature of the Glacial summer would be 42° F. higher than at present, or about 102° F. Such a considerable retreat of the ice during summer would, of course, tend to make temperate life more possible in the country.

Again, there is evidence which appears to show that the Gulf Stream warmed our western and southern shores as it does to-day.

(1) It has been noted that more southern forms of shells occur in the Drift of the west of England and of Ireland than in that of the east of England.

(2) Glaciation extended further south in North America than in Western Europe.

Therefore, as at the present day there are places considerably north of London with milder climates, so during glaciation there may have been areas fitted to preserve temperate forms of life besides the southern counties before mentioned.

But do the fossils of our Glacial deposits favour the view of the survival of any pre-Glacial plants and animals in our country?

Intercalated with the Boulder Clay are certain sands and gravels containing occasionally organic remains, which those who accept the view of interglacial warm periods claim as evidence of such.

If, as I have endeavoured to show,[1] the evidence for such periods is inconclusive, these organic remains may indicate survival; and it is well-known that such mingling of arctic and southern forms does not necessarily indicate alternations of warm and cold periods. Professor Forbes pointed out an interesting example in his "Fauna and Flora of the British Islands." Cape Cod, in North America, he remarked, forms the line of demarcation between a fauna as northern as that of England during the Glacial period, and another of an aspect perhaps even more southern than that existing at present

[1] *Geological Magazine*, August and September, 1891.

on the coasts of Portugal; and he adds: "For a short space, and but a very short space, the two faunas intermingle."[2]

Take, again, certain parts of the Atlantic Ocean at the present day. A mingling of arctic with temperate forms of life must occur among the deposits there accumulating by means of currents flowing south from colder regions, and the Gulf Stream flowing north from temperate regions.

A similar mixture must occur in the seas north of Siberia, since the great rivers flowing from the far South will carry temperate forms of life to be interbedded with the natives of those icy seas.

An interesting example of the mingling of temperate and arctic forms is recorded from the New Siberian Islands. Here in one deposit were found bones of a long-haired variety of the tiger, along with those of the musk ox, mammoth, etc.[3]

Leaving, however, the doubtful evidence of the so-called "inter-glacial" beds, are there any indications of survival in undoubted Glacial deposits?

In the Glacial fresh-water beds which occur at the base of the Glacial series in Norfolk, *Salix polaris* and *Betula nana* occur along with *Planorbis complanatus*, *Succinea putris*, and *S. oblonga*. The first of these species of mollusca occurs everywhere in England, Wales, and Ireland; the second is fairly common, and the third rare and local *at the present day*. Thus the cold which brought *Salix polaris* did not drive away these temperate forms of mollusca.

In the Boulder Clay of Norfolk, again, the following species are recorded:—*Cardium edule*, *Tellina lata*,* *Cyprina islandica*, *Littorina littorea*, *Mya arenaria*, *Pleurotoma turricula*,* *Pholas crispata*. With the exception of those marked thus *, all occur in the succeeding Glacial gravel; they all, without exception, occur in the preceding Pliocene, while only *Tellina lata* is extinct as a British shell.

Cardium edule is now common round our coasts; *Cyprina islandica* ranges as far south as Boulonnais and Cherbourg; *Mya arenaria* occurring in South Greenland, ranges as far south as Rochelle; *Pholas crispata* extends to the North of France; *Littorina littorea* ranges between Greenland and Lisbon; *Pleurotoma turricula* occurs on every part of the British coasts.

As regards Mammalia, *Elephas primigenius* is recorded from the Boulder Clay of the same district, and doubtfully from the Glacial gravel; *Cervus elaphus* is doubtfully recorded from the Boulder Clay.

When we turn to the West Coast of England we find in the organic remains of the Boulder Clay still stronger indications of the possibility of survival. Thus the great majority of shells found in the Boulder Clay of Liverpool are such as are living in British seas to-day; only two, *Astarte borealis* and *Saxicava rugosa*, are distinctly

[2] *Mem. Geol. Surv.*, vol. i., p. 377.

[3] *Mém. Acad. Imp. Sci. St. Pétersbourg*, vol. xl., no. 1. Referred to in "Notes and Comments," NATURAL SCIENCE, March, 1893, p. 170.

arctic, while others—*Venus chione*, *Cardium tuberculatum*, and *Dentalium tarentinum*—are generally restricted to more southern regions.

As far, then, as these particular cases can be taken as typical, we seem to have indications that certain temperate forms of life were able to survive glaciation.

The present distribution of animals and plants in the British Islands has an interesting bearing on the question.

That England only possesses a portion, and Ireland a still smaller portion, of the animals and plants of the Continent, is held to indicate the gradual severance of the land connection by which these countries were restocked after glaciation. Ireland is supposed to have been severed from England while the latter was still connected with France and Belgium. It may possibly be, however, that the evidence of these facts of distribution is rather that England was not restocked by a land connection from the Continent after glaciation. If it can be shown that the missing forms are those most likely to have been exterminated by the cold, or least likely to cross the separating sea, and if, in addition, they are forms calculated to migrate as quickly as those which are common to our country, then there will be an argument against the restocking by a land connection.

The argument of the fifty absent species of mammalia is against the hypothesis of such a land connection, unless it can be shown that there was some other hindrance than lack of time; for the supposition that they had not time will not pass muster: such geographical phases as that implied by the union of the East Coast of England to the Continent by a great plain are usually of vast duration.

Supposing the connection to have lasted 10,000 years,[1] then, to accomplish the journey, they would only need to travel 15, 14, or 3 yards a year, according as they crossed from the Netherlands to Norfolk, from Belgium to Kent, or by the Straits of Dover. It is difficult to think of any species of mammal which, under favourable climatic and other conditions, would not be able to spread more than from 3 to 15 yards per annum. Individuals of most of the species might cross in less than a year. Nor is it easy to believe that any species of reptile would be unable to spread at a much quicker rate than 15 yards a year; and it is to be noted that if the process by which England was united to the Continent was a very gradual one—as such geographical changes usually are—then many species would be half-way across the dividing area by the time the connection was completely established.

With the plants the case is somewhat different, for they are not able to migrate as individuals like animals. Yet when we consider the numerous means at their disposal—rivers running from the Continent towards the central plain, seeds, like those of the dandelion

[1] According to Mr. Jukes-Browne, "it lasted long enough for Palæolithic man to be supplanted by Neolithic man, and for a large number of mammalia to become extinct."—"The Building of the British Islands," p. 297.

and hawkweeds, wafted by the wind, others, like goose grass, burdock, and common Avens, adhering to the fur and wool of animals, others carried by frugivorous birds, or adhering to the beaks and feet of others, etc.—it would be strange if most of them did not migrate more than 15 yards a year.

There is another consideration. The plants which must have crossed the plain on the hypothesis of a restocking from the Continent after the Glacial epoch, include apparently some of the most slowly spreading forms.

While, then, the above considerations seem to indicate that our present flora and fauna did not necessarily migrate across an eastern plain after the Glacial period, they need not be taken as an argument against the existence of such a connection, if that is supported by other facts. There *may* have been such a connection, and yet our plants and animals may have existed pretty nearly as they are now before its existence. If it can be shown, on the one hand, that the absent species are such as ought to have migrated across a land connection, then the present distribution favours the view that our country was not restocked from the Continent after the Glacial period; if, on the other hand, they are species whose line of geographical distribution did not extend so far west as our east coast, or so far north as our south coast, then their absence is no argument either way; and if our country was not restocked from the Continent in post-Glacial times, then the pre-Glacial flora and fauna must have survived to a large extent.

The existence of several species of South European plants in the south-west of Ireland which are absent throughout the rest of Britain also furnishes an argument in favour of survival; for in the absence of a direct land connection between Ireland and the north of Spain since glaciation—for which there is no independent evidence—it is difficult to understand how they can have got there. It seems on the whole a more reasonable supposition that in the warm period preceding the Glacial they were widely dispersed over Britain, and that they were exterminated everywhere, except in the warmest corner, viz., the extreme south-west of Ireland, by the cold. Such was the opinion expressed by Professor Forbes.[5] At the same time the possibility of a post-Glacial migration by other means than a land connection—as, for example, by the carriage of seeds by migratory birds—must not be lost sight of.

A recent discovery which seems to hint that a part of our flora, at least, is a survival, is that of the seeds of *Naias marina* in the Cromer Forest Bed. The only British locality for this plant is Hickling Broad, Norfolk, and hitherto the Forest Bed is the only fossil locality. It would be, to say the least, a curious coincidence if it had been exterminated by the ice and had then re-migrated from the Continent

[5] *Mem. Geol. Survey*, vol. i.

to one spot in Norfolk only. The species is at present a native of temperate and tropical regions of the Old World.

A further indication of the possibility of survival is afforded by the flora of Cornwall and Devon. Like the Iberian element in the Irish flora, the Norman element in that of Cornwall and Devon may have existed there before the Glacial epoch. At least, the precarious foothold of many of the species, and the apparent dying out of some, are not suggestive of species migrating northwards in response to a continually ameliorating climate.

In a paper on "The Climate of Europe during the Glacial Epoch,"[6] Mr. Clement Reid discusses the question of the temperature of Britain during the Glacial Period. Some of his results seem to have an important bearing on the present enquiry. If, as he concludes, the temperature increased *rapidly* towards the south, there would be a greater possibility of survival south of the limits of glaciation. At the same time, a southern England bounded by an ice-foot similar to that of the Arctic regions, as pictured in the same article, does not appear a favourable shelter for any but somewhat hardy forms of plant and animal life. Mr. Reid also arrives at the conclusion that "extensive regions must have been quite uninhabitable for their present fauna and flora." Whether, however, this remark is to be applied to the South of England or not does not appear.

The solution of the problem which forms the subject of this article seems to me an important one, since almost the only evidence of a post-Glacial connection with the Continent is the *supposed* necessity of such to account for our present fauna and flora. To those geologists, indeed, who, on the strength of the evidence of a submerged forest, infer an elevation of the country *en masse*, such a connection—even repeated many times in accordance with the view of many Glacial and inter-Glacial periods—is no difficulty at all; they are as prodigal in respect of earth-movements as those naturalists who would "create a continent to account for the migration of a beetle." To those, however, who shrink from such assumptions, unless supported by strong evidence, and who feel that such oscillations of the land *en masse* are inconsistent with the physics of the earth's crust, the conclusion here indicated may perhaps be a welcome solution of a difficulty.

G. W. Bulman.

[6] Natural Science, vol. ii., August, 1892, p. 427.

II.

Some Recent Researches on the Habits of Ants, Wasps, and Bees.

BY most zoologists, the aculeate or sting-bearing Hymenoptera are accorded the highest place among insects; a position warranted not only by the extreme specialisation of their structure and development, but also by the perfection of their family and social instincts, which, from ancient times, have been held up for the admiration and imitation of mankind. Those families of the group in which the social life has been most completely adopted—the ants, and the social wasps and bees—are probably better known to persons not zoologists than are any other insects. Interest in their habits and economy has been greatly aroused by Sir J. Lubbock's well-known work (1), though the leading facts of these insect-commonwealths were long ago carefully observed by Huber.

The orderly and purposeful collective work of ants has often led observers to speculate upon the means of communication between the insects, and they have been believed to converse with each other by motions and contact of their antennæ. Experiments on this subject must be familiar to readers of Lubbock's book. He also investigated the ability of ants to make sounds and to hear. Although supposed organs of hearing have been found in the antennæ and tibiæ of ants, no impression could be produced upon the insects by any sound audible to human ears. Lubbock supposed, therefore, that they can only appreciate notes of a pitch too high to be heard by us. He suggested that certain ribbed areas on the abdominal segments of various species might be organs for producing sound, but he was unable to detect any sounds which these may have made.

These sound-producing areas have recently been specially studied by Dr. Sharp (2). He finds that some of the structures observed by Sir J. Lubbock and others are nothing but the ordinary sculpture of the surface of the segments. In other cases, however, he has no doubt that stridulating organs really exist. At the middle of the base of the dorsal surface of the third abdominal segment, in most species of the Myrmicides and Ponerides examined, he finds a special area traversed by excessively fine lines. In the Ponerides these are modified from the ordinary sculpture of the segment which, on these

spots, becomes finer, closer, and more regular. Indeed, in comparison with the rest of the surface, the stridulating area looks smooth, and a high power and appropriate light are required to make the lines apparent. In the Myrmicides, the lines appear to be not always mere modifications of the general sculpture; in some cases they are described as being "developed on a glassy surface poured out on the ordinary surface." In a species of *Sima* from Australia, the area was found to be divided into two parts, the lines on the one part being much coarser than those on the other, and doubtless enabling the insect to modify the sound produced.

The delicate instrument formed by the fine sculpture is played upon by the specially modified hinder margin of the segment in front, which projects downwards to form a very sharp and even edge. This, drawn across the fine ridges, as the ant moves her abdominal segments, must give rise to an excessively high note. In some cases, at least, the sound is audible to human ears. By moving the appropriate parts, Dr. Sharp has produced sounds with species of leaf-cutting ants (*Atta*). Mr. Wroughton (3) has described the action of a colony of Indian ants (*Crematogaster*) when disturbed; they wag their abdomens, and emit a sound "as if a red hot cinder had been plunged into water." The Camponotides, to which most of our common British species of ants belong, do not appear to possess stridulating organs.

Leaf-cutting ants are familiar to readers of books of tropical travel, and the object of the insects in collecting leaves has been differently explained by various observers. The opinion advocated by Belt, that the ants tear up the leaves in order to make beds on which a crop of fungi may be grown, was confirmed by Mr. Cook, and has recently received more detailed confirmation from the researches of Herr Möller (4), who has studied the habits of these ants in Brazil. Within their nest is a soft, spongy mass, consisting of the remains of leaves, cut into excessively minute fragments and gathered into small heaps to serve as "mushroom gardens," in which a fungus (*Rozites gongylophora*) is cultivated to furnish food for the colony. Four species of ants (genus *Atta*) are found to grow the same fungus. In all cases a small space is left between the "mushroom garden" and the outer wall of the nest, and the beds are never formed in an exposed position. The material collected by the ants naturally contains spores of other fungi than their special kind, and in cultivations prepared by Möller these appeared and developed. In the nests, however, but the one kind is allowed to grow; the ants appear to weed out all but their special food. Over the upper surface of the "garden" are numerous small white bodies, formed by masses of swollen ends of fungus hyphæ, which the ants appear to produce by some special culture. These masses are what the ants feed upon, they never appear to eat the fragments of leaves, which form the fungus-beds. When they take a journey they carry with

them the material of the "garden," even to the smallest fragments, and rebuild the structure in their new abode.

Herr Möller finds that ants of the genera *Apterostigma* and *Cyphomyrmex* are also "mushroom gardeners," but these do not cut leaves to form their fungus-beds. They use instead, wood, grain, or dung. Four species of ants of the former genus cultivate one fungus, and two species of the latter another. Although these fungi all belong to the same group, the ants, even when hungry, are found to refuse disdainfully the food of another genus. Even the species of ants of the same genus, which cultivate the same species of fungus, produce by different methods of culture a difference in the nature of their special food bodies. Those grown by the species of *Atta* are adjudged the prize by their patient observer in the fungicultural contest.

Observations by Herr Möller on another genus of ants (*Acanthognathus*) have been recently recorded by Professor Forel (5). Besides teeth at the extremity of their mandibles, the workers possess, at the base of each of those appendages, a long, curved tooth, directed inwards and downwards. When at work building, or carrying eggs, these ants have their mandibles widely apart, sticking out on either side at right angles to the body. When in this position, the ends of the basal curved teeth just touch each other, and are used by the insect to carry the egg, or a piece of earth for building. When frightened, the ant brings her mandibles sharply together, and the basal teeth are then, of course, crossed.

A most interesting summary of what is known of the various kinds of ants' nests has, also, lately been published by Professor Forel (6), in which he gives the result of observations by himself and other naturalists. Nests made in holes or under stones, nests dug in or built of earth, wood, etc., are described and illustrated from the work of European and exotic ants. The transition between nests built of a papery substance formed of fragments of earth, wood, etc., joined by a secreted cement, to those composed of fine threads drawn out from such cement, and used for binding leaves together, is traced through a succession of species.

Lubbock, in the work already referred to, remarked that it was strange that the exact manner in which new colonies of ants are founded should remain uncertain. He suggested that a young queen, after the nuptial flight, might join an old nest, or found a new colony, either by herself, or with the assistance of a number of workers. Professor Forel, in the paper just mentioned, considers it established that new colonies are founded by solitary females, or by several associated together. Mr. W. W. Smith (7) has lately studied the rise of ant-colonies in New Zealand. The nuptial flight there takes place in March, and the rise of societies was studied through the winter months following. A pair (male and female) or several pairs were found to be associated to form the new colonies; and these always selected sites, under stones or elsewhere, already occupied by

aphids and coccids—the "domestic animals" of the ants. If, through any cause, the aphids left the place, the ants went away too.

To modern naturalists the stages by which the more perfectly social insect communities have arisen furnish a question of great interest. Some facts and suggestions towards the solution of this problem have recently been furnished by Herr Verhoeff (8). He points out that the economy of each social group must be compared with that of the "solitary" genera most nearly akin to its presumed ancestral stock. The affinities of the ants are considered by Verhoeff too doubtful for profitable consideration: he therefore confines his attention to the wasps and bees. The social wasps (Vespidæ) are believed to have arisen from the Eumenidæ, and these from a group of fossorial hymenoptera nearly allied to the Trypoxilidæ. The bees are supposed to have originated from some other fossorial family. Hence the study of the fossorial genera may be expected to throw much light upon the economy of the higher groups. From simply laying the egg in the body of some other insect—after the manner of modern ichneumon flies—was developed the present habit of the fossorial wasps of digging a hole, in which are placed both the egg, and the insect which serves as a prey for the grub when hatched. The habit of catching the prey, and then digging a hole to bury it in, must clearly have preceded that of first preparing the hole and then catching the prey to put in it. As in primitive man, the hunting habit was at first stronger than the home-making. But the simple digging of a hole, once adopted, led on by degrees to the construction of nests of a more and more complicated design. The simple unicellular nest was improved into the linear arrangement of cells, with a common opening, constructed either in the earth or in twigs which may now be observed in species of *Crabro*, *Hoplopus*, *Tropoxylon*, *Colletes*, *Osmia*, *Anthophora*, etc. From this latter, or from the simple cell, is derived the branched style of nest in which the cells open off from the entrance or from a passage leading thereto; such nests are made by many of the solitary wasps and bees, species of *Hoplopus*, *Halictus*, etc. In *Rhopalum clavipes*, which, making its nest in twigs, constructs some of the cells at a very slight angle with the main series, we see how the branched system could arise from the linear. A further advance is seen in the nests not placed in cavities but built of lime or sand upon rocks or walls. The highest forms of nests are the paper structures of the social wasps, and the waxen cells of the bees.

Herr Verhoeff does not believe that the social communities of the wasps and bees have arisen from casual colonies, such as are occasionally observed among solitary species; the bond of the community is to be found in the members being all the offspring of one mother. As in human society, the family has given rise to the state. For the development of a social community three conditions are necessary:—A nest large enough for a number of insects, a close

grouping of the cells, and an association between the mother and her offspring in the perfect state (not simply as larvæ); the last condition will be brought about by the emergence of the older insects of the brood while the mother is still occupied with the younger larvæ or their cells. In a single species of solitary bee (*Halictus quadristrigatus*) these conditions are almost fulfilled, but the earliest young insects to appear are males, and when the females are developed the mother dies. For the formation of a community, of course, the mother must co-operate with her female offspring, and this *Halictus*, therefore, just fails to develop the social habit. It gives, however, a sufficiently strong clue to the origin of that habit.

The difference in habit between bees and wasps gives rise to another interesting suggestion by Herr Verhoeff. The bees, being vegetable-feeders, are more disposed to live peaceably in communities than the wasps. But there is no contact between the mother and larva; a supply of honey and pollen is placed in the cell, the egg laid, and the cell closed up. The wasps, on the other hand, are mostly insect-feeders, and might be expected, therefore, to be of too warlike a disposition to form well-ordered republics. But their mode of life is such as to develop family affection; the mother, after laying the egg, goes in search of prey, and from the fossorial habit of placing alongside the egg a paralysed insect to serve as food for the larva, and then closing the cell, has arisen the custom of feeding the larva throughout the preparatory stage, and only closing the cell during the pupal period.

The nests and habits of many species are described by Herr Verhoeff in detail. Some notes on the wintering of Hymenoptera are of special interest. The winter is passed by various species in all stages, except the egg and the feeding-larva. Curiously enough, insects of the social species are generally found singly in the winter, while individuals of the solitary species gather together in the cold season, as do also some ichneumon flies. A number of males and females of a species of *Ceratina* were found in a bramble stem which they had hollowed out to serve as winter quarters. A winter colony of females of *Halictus morio* (the males of this species die in autumn) was found in a forked tunnel opening under a large stone. In was in early spring that these insects were observed, and, while a number of the bees were gathered together at the end of one branch of their burrow, others were apparently making their way out of the winter retreat.

The habits of some bees (*Trigona*) at Trinidad have been lately described by Mr. Hart (9). These insects have no sting, and consequently adopt special means for defending their nest. The only entrance is by means of a tube which is constricted at intervals so that only one bee can pass. Each of these constrictions would serve as a fortification where a foe might be held at bay. Moreover,

at night the bees entirely close up the tube with a sheet of wax, leaving only a few minute openings for ventilation.

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Geo. H. Carpenter.

III.

The Recent Plague of Wasps.

THE remarkable summer just past has been signalised by such a profusion of what is perhaps the most generally unpopular insect pest, that a brief review of the facts may be of interest. Of the seven British species of *Vespa*, no fewer than five appear to have been more than usually abundant. So early as the beginning of June both personal observation and reports from reliable witnesses showed *V. sylvestris* and *V. novegica* to be well established and in possession of strong nests. During the earlier portion of the summer these two tree-wasps were, in S.W. Surrey, far more abundant than the ground-wasps, *V. vulgaris* and *V. germanica*, which, however, were in full force by the middle of July, while *V. rufa* was found more commonly than in previous years, though it could not be said to be numerous, at least from my own information. It is much to be regretted that the letters and complaints in the daily newspapers contain no hint as to the species of wasps abounding in various parts of the country, though the context would in most cases point to ground-wasps. Nevertheless, Mr. Lowe (*Nature*, vol. 48, p. 437) speaks of "the tree-wasp" having had many nests in Monmouthshire, so that we may reasonably conclude that the season has been favourable to tree- and ground-wasps alike throughout England. Of the somewhat mysterious *V. arborea* I have heard nothing, excepting the capture of two females in Ireland (*Entom. Monthly Mag.*, July, 1893, p. 166). It is very singular the hornet, *V. crabro*, has hardly been reported abundant anywhere. I cannot help thinking that this species is decidedly on the wane and disappearing from our islands. Popular observation on the point is well-nigh worthless, for many so called "hornets" prove on inspection to be large female wasps. It would be of great interest to ascertain from all counties whether the "red hornet" has or has not become appreciably scarcer during the last fifteen or twenty years.

Turning now to the causes of this "plague," I have no hesitation in ascribing it almost entirely to a remarkable series of meteorological conditions advantageous to wasp life. Speaking of observations made by myself, which in the main agree with those taken elsewhere, it is noticeable that after March 23 there was no frost registered by the screened thermometer, excepting slight ones on the nights of April 13, 14, 15. Again, with regard to rainfall, from March 1 to June 30 the total amount registered was only 2·38 inches on 27 days,

and on no one day throughout the four months did the fall exceed 0·34 inch.

It is not easy to conceive weather better suited than the above to the well-being of wasps. The impregnated females emerge from winter quarters early in the year, generally making their first appearance during March. The occurrence of frost, snow, heavy rains, and the like during April or even May, cannot fail to destroy enormous numbers of females and their first few grubs lying in the small nests built of delicate "wasp-paper" by the queens themselves. Severe weather setting in after the commencement of the nests must be fatal to tree- and ground-wasps alike. This year after the severe weather of early spring had once gone it never returned. Hence the females, tempted from hibernation by warmer days, met with no disaster either in their persons or their habitations, and thus an unusually large number of nests became successfully established. Further than this, the mean temperature for the summer months has been decidedly above the average, and many animals and flowering plants made their appearance several weeks sooner than is customary, thus affording a plentiful supply of food material to the omnivorous worker wasps. The drought appeared to affect the wasps in two ways, advantageously by causing great and rapid increase of Aphides whose secretions the wasps keenly appreciate, in addition to favouring the multiplication of flies, earwigs, etc., on which wasps to a great extent subsist; detrimentally by rendering it difficult to supply an adequate amount of moisture to the growing grubs. In some nests which were taken by chloroforming, I found numerous grubs shrivelled up and many of the wasps captured in the open were unusually small. This lack of water was very clearly shown by the way in which wasps swarmed round plants which were regularly watered so that the leaves and earth were constantly moist; in such cases wasps could be seen at all times of day greedily sipping up drops of water, or getting it from the wettest earth beneath the leaves.

From an economical point of view, a word in favour of the much-abused wasp is due. As scavengers, wasps undoubtedly confer great benefit upon us; the rapidity with which they remove the flesh from dead animals requires to be seen to be believed. On several occasions I exposed dead mice in order to notice the ravages produced by the wasps, and in each instance every particle of soft material was completely removed from the bones in the course of two days.

It is never wise to prophesy, but there are indications of what may be expected next year; the drones and young females have now been on the wing for some time, having appeared earlier than usual. This renders it probable that many females will be destroyed before they search out convenient spots for hibernation, so that already the balance is probably being readjusted, and unless the spring and early summer of 1894 prove repetitions of this year, there is no cause to anticipate a recurrence of the plague.

Charterhouse, Godalming. OSWALD H. LATTER.

PHOTOGRAPH OF LEFT WING OF *Archæopteryx*, from the specimen in the Natural History Museum, Berlin. *(Two-thirds nat. size.)*

IV.

Biological Theories.

VII.—THE DIGITS IN A BIRD'S WING: A STUDY OF THE ORIGIN AND MULTIPLICATION OF ERRORS.

THE plate accompanying this essay is a photograph from nature of the dorsal aspect of the left wing of the specimen of *Archæopteryx*, which is now in the Museum of the Berlin University, and which was found, in 1877, in a stratum belonging to the Upper Jurassic series, near Eichstätt in Bavaria. This photograph is now published because the best known figure of this specimen is characterised not so much by carelessness or by inadvertent error as by wilful falsification. That figure has, however, been copied into some of the best palæontological and geological treatises, and has received the authoritative sanction which insertion in Zittel's "Palæontologie" involves. In controverting views supported by such an authority, and views to be found even in the careful writings of Professor Huxley, and of almost every lesser light who has ever expressed an opinion on the subject, it is hardly probable that I should gain credence if I had given merely a new drawing. The photograph, however, will carry conviction.

In the plate, the humerus, the straight radius, and *curved* ulna, a carpal bone, and three long slender fingers may be seen at a glance. The fingers are all clawed, though the claw itself is not easy to make out: still the form of the ungual phalanx can leave no doubt upon the subject.

The fingers are recognised by everybody as being the equivalents (or homologues) of the digits I, II, and III of the normal pentadactyle sauropsidan fore-limb: this view is fully supported by the relative positions of the three fingers and by the numbers of phalanges—two, three, and four respectively—which they possess.

To the post-axial side (*left* in the plate) of the ulna and of the fingers is seen a wing in a marvellously perfect state of preservation In the original, even the barbules of some of the quills are recognisable, and the barbs are easily seen even in the photograph. Of the quills, there are sixteen or seventeen (possibly even one more). The first seven are *primary* quills, and these are of chief importance to us for the present purpose. To merely state that the fourth is straight, and

that the remainder are very nearly so, would not be sufficient. I must beg the reader to lay upon the plate the *straight* edge of a piece of paper, or a rule, and to see for himself that this is true, and that the concavity of the curvature of every primary quill is turned towards the middle one, *i.e.*, the fourth. The hinder ones are curved—but only *very* slightly—in such way that the concavity of the curve is turned forwards. If the line of the rhachis of each individual primary quill be traced onwards, a pretty accurate idea may be obtained as to where these *primary* quills were inserted.

In the Steinmann-Döderlein figure, which is spreading like a plague in modern books, those hinder primary quills which the photograph shows to be curved slightly *forward* (*i.e.*, with the concavity forwards), are represented as curving *strongly backwards*—curving, that is, through an angle of more than forty degrees! The distinction between primary and secondary quills is abolished, and all but the first two or three are represented as attached to the ulna! The second (or ? first) quill is represented as the longest, and the fourth, which is seen in the photograph to be the longest, is drawn much shorter than this, or rather as falling short of it at the tip. The continuation of the quills in a wrong direction as far as the ulna involves, in the case of the third one, the representation of the feather of nearly double its true length.

Nor is it difficult to guess how the originators of the figure came to draw it thus falsely. Inadvertent error on the part of the engraver is out of the question. The drawing is not to be described simply as erroneous, but as deliberately falsified. The uselessness of *strongly*-curved feathers for flight may not have occurred to the authors, but the absurdity of supposing (as many do!) that the three long, *slender*, and especially *weak-jointed* fingers could bear the torsional stress to which they would be exposed during flight if they supported those large quills, appears to have occurred to them and to have led them to avoid the absurdity of this, the everyday view, by the falsifications to which I have referred.

If the dissected wing of a common bird, such as a pigeon—the left wing—be laid on the table and 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, and this conclusion receives ample support from the weakness of the vertebral column and of the hind limbs, and from the small size of the pelvis and sacrum.

In the dissected wing, or in any fairly good figure showing both feathers and bones in the wing of an ordinary bird (1, 2), the *ala spuria* will be seen to correspond to one or more of these free fingers of *Archæopteryx*. I cannot at present see any way of deciding whether it is a vestige of one, or of two, or of all three of them, and I cannot satisfy myself as to whether certain slender feathers seen to lie upon the primary quills in the plate, and making a considerable angle with them, are really coverts as usually described, or whether they were attached to the free fingers. If they are coverts, as seems probable, their position (lying *across* the primary quills) may be due to the action of a current of water flowing over the recently dead bird and bringing with it the mud which, being deposited—apparently rather quickly—effected the preservation of the specimen in the perfect state in which it was found.

Dames (3), in his detailed description of the specimen, states that the primary quills ("6 to 7") were attached to the longest finger (II). If while we look at the photograph 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. In other words, both wing and finger would be rendered useless by such an attachment.

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 that 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.

That the views which have gained such wide acceptance should have been possible when Owen (4) 30 years ago recognised and figured the very bones in question, is a striking testimony to the credulity of modern zoologists!

We read in any text-book that happens to be at hand that the three digits of a bird's wing answer to the "pollex and second and third digits of the pentadactyle fore-limb." Such a statement, though found in almost all modern books which treat of the subject at all, is in no case, so far as I am aware, supported by any evidence whatever. I believe the statement originated in a mistranslation of the

pre-Darwinian statement that the ala spuria is "analogous to the thumb," while the other two digits are called simply "second" and "third," that is *second and third digits* not of the pentadactyle but *of the tridactyle fore limb*. Such phrases in works written on the then undoubted hypotheses of special creation and of fixity of species, could, obviously, not mean that the three digits called "thumb" and "second" and "third" digits had been evolved from the digits I, II, and III of the pentadactyle fore limb of an ancestor: the authors did not believe birds had ever had such ancestors. The transcription of such phrases into post-Darwinian treatises, without consideration of the new meaning which they would thus gain from the new context, appears to have been the origin of the error.

And yet Professor Huxley (5) has relied upon the unfounded statement that these digits are I, II, and III in constructing a theory of the phylogeny of birds. On the strength of this assumption he has denied the descent of birds from Pterodactyles. They may, of course, not be so descended, but we may not reasonably believe that his argument from the wing-structure makes their descent one jot more improbable than it was before.

It may at least be said that, in the matter of pelvis and of wing, *Archæopteryx* is much more like a Pterodactyle than it is like *Compsognathus*, or any other Dinosaur.

Those "ornithic" characters of the Dinosaurs which distinguish them from Pterodactyles, distinguish them also from the undoubted bird *Archæopteryx*; and if the "ornithic" characters of the Dinosaur pelvis generally, and of the foot of *Compsognathus*, are due to any blood-relationship with birds at all, it seems probable that the phylogenetic tree will have to be turned upside down so as to express the descent of Dinosaurs from Carinate birds through a series of "ratite" birds which had lost the power of flight through isolation and consequent suspension of the action of a selection dependent on powers of flight, much as the existing "Ratitæ" have probably descended from Carinates of very various orders.

It is, however, not to put forward new phylogenies that I now write, nor merely to correct a particular error in biological theory, but to point to one of the most fruitful sources of error.

To attempt to expose one by one all the current biological errors would be very much like going to Mecca, armed with a microscope, in order to eradicate cholera by finding the bacilli, and killing them one by one as they were found.

With reference to cholera epidemics, it has been said, "Cholera is a filth-disease: abolish filth, and cholera will vanish."

Uncritical credulity plays the same part in the spread of error as filth plays in the spread of cholera. The standard text-book of the anatomy of the Vertebrata (6) says, "In all Carinatæ there are three digits in the manus, which answer to the pollex, and the second and third digits of the pentadactyle fore limb"; and the statement is not accompanied

by any evidence of any kind in support of it. In a healthy community of students, such statements would do no harm, for only a morbidly-credulous person unfit for scientific work would accept the statement as true, *on the authority* of even the greatest anatomist living. For an honest record of mere fact, we are often compelled to rely upon the accounts given by others—we do not all have the opportunity of dissecting *Nautilus* and other rare animals, and we none of us have time to dissect one-hundredth part of the animals of which specimens are easily obtainable. We are therefore compelled to rely upon each other, and so glaring a falsification as that shown in the Steinmann-Döderlein figure is, fortunately, not common. The discrepancy between the description and the figure (in Zittel, for instance) would, moreover, show any careful reader that either description or figure was false, if not both.

For facts, then, we must rely upon others, but must exercise judgment in doing so.

For opinions it is otherwise. To take another man's opinion and accept it untested—as has been done in the case of the bird's wing—is a sure way to something worse than mere error. Darwin was one of the greatest men who have lived, and his opinion is worth more than that of most other men. To accept even his opinion, however, except after examination of the argument upon which it rests, is evidence that the man so accepting it is unfit for scientific work. He will probably gain applause from "a popular audience" (just as a good encyclopædia may gain approval), but if he holds opinions otherwise than as the result of conviction he is only a walking encyclopædia, and not nearly so good a one as the "Britannica." So long as teachers continue to regard, as many do now regard, the "passing" of their students as the object of their work or of their students' work, so long will the "cramming" system continue to convert men into walking encyclopædias, stocked with second-hand ideas, and incapable of either creating new ideas or of judging of the value of the old ones.

In a paper by Morse (7) the view that the bird's *wing*-digits are II, III, and IV is put forward on the strength of the existence of a supposed vestige of another digit on the radial side. Granting his facts, such a vestige might be a vestige of a pre-pollex, so his contention does not prove even that the usual view is erroneous, though it lessens its apparent probability. It of course does not in the least affect the view I have put forward, for he makes no attempt to prove that the digits *are* II, III, and IV, but only that they are *not* I, II, and III. The possibility of their being III, IV, and V seems not to have occurred to him or to those who have adopted his view. What W. K. Parker (8) regarded as a vestige of the digit IV appears to me to be *os pisiforme*.

Zittel (9) gives a figure described as "nach Owen." Owen's figure shows *four* digits. Zittel has eliminated the innermost. It

would not otherwise fit in with the orthodox view. Owen was a strong opponent of the Dinosaurian-ancestry theory, and his figure was valuable evidence in favour of the Pterodactyle-ancestry view which he maintained. Is it right that Zittel should so falsify that figure as to make it tell the other way, and then describe the mutilated figure as "nach Owen," thus ascribing to Owen a view which he strongly opposed?

The falsification of the figure is a typical example of the inevitable results of "pinning one's faith to a creed," the creed being, in this case, the orthodox view that the three digits in an ordinary bird's wing are I, II, and III.

SUMMARY.

Archæopteryx was a winged quadruped, probably arboreal in habit.

The photograph accompanying this essay 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.

This justifies the belief that the two large digits in an ordinary bird's wing are IV and V, and that the *ala spuria* is a vestige of one or more of the other three digits of the pentadactyle fore limb.

The argument against the view that birds are descended from Pterodactyles is, therefore, worthless so far as it rests upon the assumption that the large digits of the bird's wing are II and III.

The "ornithic" characters of Dinosaurs do not justify the view that birds are descended from these reptiles: for the oldest known bird is devoid of those "ornithic" characters.

Compsognathus may form a connecting link (but not, of course, in the direct line) between some unknown "ratite" birds on one hand, and the great Dinosaurs on the other, but only if these be regarded as "degenerate" and overgrown descendants of birds which had lost the power of flight.

Careless transcription of a pre-Darwinian statement (which was perfectly true) into post-Darwinian treatises has given the statement a new and false meaning.

Uncritical plagiarism has rendered the false statement an integral part of almost every modern treatise on the subject.

The false idea expressed in that statement after transcription has dominated the minds of even those (10, 11) who wrote with *Archæopteryx* before their eyes, and the absurdity of a corollary of that idea has induced some (12) to issue a figure so falsified

as to hide the absurdity: and the falsity of that figure has gained for it admission into modern text-books written also under domination of the same false idea, while Owen's figure has been mutilated so as to make it reconcilable with a view which it was intended by Owen to disprove (9).

The spread of error is largely due to the domination of ideas (not invariably false!), and is possible only in a world of uncritical compilers and credulous readers and hearers.

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C. HERBERT HURST.

V.

The Problem of Variation.

IT is certain that, in order to get a deeper insight into the causes and modes of organic evolution, we must investigate the problem of variation. The first requisite for such an investigation is a definite grasp of the problem, an analysis of its complications. Much that is written at the present time on ultimate biological theories is more or less barren from the absence of a definite and comprehensive conception of the phenomena which have to be explained. It is easy to expand the proposition that, given all kinds of hereditary variations in individual organisms, selection will accumulate those which lie in a particular direction and result in special adaptations of structure to function. But what causes the variations? It is more important and more difficult to construct a theory of heredity as due to the continuity and conservatism of the germ-plasm, but we have to explain, not merely conservative, but progressive and retrogressive heredity.

It is necessary always to bear in mind that the phenomena to be dealt with are presented by individual organisms. There is no such thing as the origin of species apart from the origin of individuals. It has been maintained by philosophers that a horse can be locked in a stable, but the species horse cannot, being a mental abstraction formed from the perceptions of individuals; but it seems to me more correct to say that we recognise a number of individuals with such a degree of similarity to one another that we class them together and give them all a specific name. It would not be impossible to put all existing horses together into one enclosure.

Variations then, of course, must be variations of individuals; but a most important distinction, which is constantly ignored, exists between contemporary individual differences and the differences between consecutive generations, which form, so to speak, the units of evolution. The nature and degree of individual differences in a single species, the relations of varieties and doubtful species, are important enough as observed facts; but Darwin, Wallace, and others present them as the material on which selection acts, without adequately discussing the question of the relation they bear to the difference between one generation and its successor. Galton and Weldon have recently applied the higher mathematics to the study of individual differences, but it does not seem to me that in this way

we shall make much advance towards an explanation of the variations which make evolution. The differences between a large number of individuals, without reference even to the ontogeny of these differences, are recorded with greater accuracy and completeness than before, but that is all. The same differences might have been present in every generation of the species if it had been created in the form in which we see it.

In order that evolution may take place, individuals must be generated which are different to any that ever existed before. If we take any particular organ or character, one or more individuals in a generation must have possessed this organ or character in a more advanced (or less advanced in the case of retrogression) condition than any individual which had previously existed. This is the kind of variation which we must account for, and find the cause of, in order to have a complete theory of evolution or of heredity. This must occur over and over again to produce the evolution which, according to the evidence, has taken place.

Now the actual appearance of new characters has been observed, for instance, in the case of the Ancon sheep, and in wild plants when cultivated; and, on the other hand, Weismann has proposed the theory that variations of the kind defined above are due to the union of two individuals in sexual reproduction; but I have not space to discuss, on this occasion, either of these two subjects, because, with regard to the second, Weismann has abandoned the theory, and, with regard to the first, such variations occur at the beginning of the development of the individual. Nothing is more certain than that no theory of variation is worthy of attention unless it takes into account the phenomenon of recapitulation. In a very large number of cases an organ, or a character, or an individual, passes through in development stages which reproduce, more or less exactly, an ancestral condition. In other words, new characters are added usually at the end of ontogeny. We have convincing evidence, then, that the modified individual first resembled its parent, and afterwards became different. The ancestor of the flat-fish was symmetrical; but, at some time or other, individuals of this pedigree, after developing into symmetrical fish, became more or less asymmetrical, and every flat-fish *at the present day* develops first into a symmetrical fish, and then turns into a Pleuronectid.

It is certain that no combination of invariable parental germ-plasms could result in this change. How, then, does Weismann's new theory account for it? The determinants, the units of the germ-plasm corresponding to particular cells, or group of cells, in the adult, are modifiable ultimately by external influences. On this new theory Weismann attempts the explanation of recapitulation in the following words:—

"The determinants of the id of germ-plasm become endowed with a greater power of multiplication, so that each one of them causes the

addition of one or more cell-generations to the end of the ontogeny. At the same time, the determinants in the germ-plasm increase in number, and each of them becomes differentiated in a fresh manner. As, however, every two new determinants always follow the same course, from the id of germ-plasm to the final stage in ontogeny as was taken by the single original determinant, they will pass through the same determinant figures as before, and only lead to the formation of new structures in the final stages when they become separated from one another. The ontogenetic stages will be repeated less accurately the nearer development approaches its termination."

Now, for my part, I cannot see that this explanation in any way covers the phenomena. According to the theory, the specialisation of every cell and every organ at every stage depends on determinants. The problem to be explained is, therefore, why the determinants for the organs of the early stages of ontogeny are not modified. Weismann supposes that in the differentiated cells of any stage determinants are used up, while in the undifferentiated multiplying cells unconsumed determinants are held in reserve to supply later generations of differentiated cells. The determinant figure is merely an attempt to show why the consumption of the determinants follows a definite course. It does not afford any reason why the determinants which are reserved to the last should be modified, while those which pass into differentiated cells belonging to early stages remain unaltered.

As to the cause of changes in the determinant, Weismann now attributes them to the direct effect of external influences, which influence the nutrition of the biophors and determinants during their continual reproduction through countless generations; but in his chapter on variation he mentions no reason why such changes should occur rather in the determinants belonging to the final stages of ontogeny, than in those belonging to the earlier stages.

It may be truly said, therefore, that we have no theory of variation which necessarily involves the recapitulation of phylogeny, except the theory of the inheritance of acquired characters. It is important to notice that recapitulations occur chiefly in connection with adaptive modifications. The young flat-fish swims with its principal plane in a vertical position, and its body is symmetrical about that plane; the adult flat-fish, on the contrary, is adapted by its asymmetry to living in a position in which the principal plane of the body is horizontal. The tadpole is essentially a fish adapted to live in water, while the frog is adapted to breathe air and lead a terrestrial existence. These obvious and striking facts point, at all events apparently, to the conclusion that the change of structure was produced by the change of conditions.

Another consideration which points in the same direction is that the actual ontogeny of these changes of structure in the individual has been shown in some instances to be largely dependant on

conditions. For instance, the metamorphosis of tadpoles can be prevented for a long time by certain appropriate conditions, which would not be the case if the changes were entirely determined by modification of the germ-plasm.

A case which I have myself recently investigated experimentally seems to me to support very strongly the theory of the inheritance of acquired characters. I have shown that in normal flat-fishes, if the lower side be artificially exposed to light for a long time, pigmentation is developed on that side; but when the exposure is commenced while the specimens are still in process of metamorphosis, when pigment-cells are still present on the lower side, the action of light does not prevent the disappearance of these pigment-cells. They disappear as in individuals living under normal conditions, but after prolonged exposure pigment-cells reappear. The first fact proves that the disappearance of the pigment-cells from the lower side in the metamorphosis is a hereditary character, and not a change produced in each individual by the withdrawal of the lower side from the action of light. On the other hand, the experiments show that the absence of pigment-cells from the lower side throughout life is due to the fact that light does not act upon that side, for when it is allowed to act, pigment-cells appear. It seems to me the only reasonable conclusion from these facts is that the disappearance of pigment-cells was originally due to the absence of light and that this change has now become hereditary. The pigment-cells produced by the action of light on the lower side are in all respects similar to those normally present on the upper side of the fish. If the disappearance of the pigment-cells were due entirely to a variation of the germ-plasm, no external influence could cause them to reappear, and, on the other hand, if there were no hereditary tendency, the colouration of the lower side of the flat-fish, when exposed, would be rapid and complete.

Weismann admits that hereditary colour-changes, such as a darker colour in butterflies, are produced by climatic influences, but explains this on the supposition that the climatic influences have simultaneously modified in an individual the determinants of the pigmented cells in the integument, and the determinants in the germ-cells of that individual which belong to the corresponding pigmented cells of its offspring. Now it is impossible to suppose that the light acting on one side of a flat-fish would modify the determinants belonging to one side in the ova of that individual, and even if it did it would not cause the pigment-cells to develop symmetrically on both sides of the larva, and then to disappear from one side of the perfect fish.

It is impossible or inconceivable on Weismann's theory that changes of somatic cells should affect the determinants of the germ-cells contained in the soma; there is no connection between the two; but, on the other hand, it is equally inconceivable on his

theory that the presence or absence of the germ-cells in an individual should affect the ontogeny of the soma. He assumes that the determinants for the soma are separated off and determine a certain ontogeny, which is not affected by the fate of the determinants of succeeding generations contained in the germ-cells and germ-tracks; and yet we know that male secondary sexual characters are not developed if the young male is castrated. The development of these characters depends on the normal presence of the testes and their germ-cells in the individual. Weismann mentions this fact, but how does he connect it with his theory? He simply concludes that in such cases the secondary sexual characters of both sexes are present in each individual, and one or the other set develop according to circumstances. He makes no attempt to show why the particular circumstance which determines the development is the presence or removal of the generative organs. The fact of the course of ontogeny in this case being affected in so radical a manner by the mere presence of the generative organs, is alone sufficient to overthrow his fundamental assumption of the essential independence of the soma and the germ-cells contained within it.

Degeneration of organs is a phenomenon in which recapitulation is particularly well exhibited. Scarcely any case is known of an organ which has disappeared or become rudimentary in the adult condition, and which is not more perfectly developed or developed to some extent in earlier stages of the life-history. Even the limbs of snakes are indicated to some extent in the embryo, and the teeth of the right whale are present in the fœtus; but the loss of the eyes in blind subterranean animals is the instance I prefer to bring forward here. On the Natural Selection theory it is no advantage to such animals that the larvæ or the young should have eyes better developed than the adults. Yet this is known to be the case in the blind amphibian Proteus of the caves of Dalmatia, the blind crayfish of the caves of Kentucky, and in other cases. This case has a superiority over others in my argument, because there is no change of conditions as in the frog and the flat-fish: the whole development takes place in the dark. Weismann considers this case in relation to his theory, but does not succeed in showing any logical connection between his assumptions and the phenomenon. He supposes that the determinants for the degenerating organ first begin to lose their power of multiplication, and then a continually increasing number disappear. This means, presumably, that a succession of determinants is necessary to supply the successive generations of cells which keep an organ in proper condition, and that the reserve determinants in the soma are continually multiplying when degeneration does not take place; but we know that in the life of the individual this "multiplication of determinants" is checked or stopped by disuse, and we do not know how the "power of multiplication" is diminished in the germ-plasm. In spite of all that has been discovered or conceived up

to the present, the hypothesis that the properties of the germ-plasm are modified by changes effected in the adult organs by external influences still holds the field.

It must be conceded, even by his opponents, that Weismann has done a very great service to biological science by insisting upon and forcing upon the general attention the actual relations of the germ-cells to the soma. We cannot now overlook the fact that germ-cells are not descended from the modified and specialised cells of the soma, nor can we any longer use a hypothesis of the construction of the germ-cells from gemmules emitted by the somatic cells. The germ-cells, like the cells of a muscle or a gland are derived directly from the ovum by a succession of multiplying cells forming the germ-track; but we have still to discover how the ontogenetic properties of these germ-cells are modified. In the present state of knowledge it is a more irresistible conclusion that changes produced in the soma ultimately affect the germ-cells, than that variations in the latter are due to variations of nutrition in some manner which no one has attempted to describe.

J. T. CUNNINGHAM.

VI.

The Nearctic Region and its Mammals.[1]

IT would appear that the Munroe doctrine of "America for the Americans," is little heeded by the biologists of that Continent. Although the best European authorities on the geographical distribution of animals have long ago conceded to the northern half of the New World the rank of one of the six primary divisions of the earth's surface, under the name of the "Nearctic Region," our American friends will have none of it. Two recent writers of deservedly great authority on the Mammal-life of North America, though, as we shall show presently, they differ much in minor details, agree in repudiating the Nearctic Region altogether. They refer the northern parts of North America to the "Arctic Realm" or "Boreal Region," of the Old World, and the southern portion to the "American Tropical Realm" or "Tropical Region," leaving only the intermediate area separate and apart. This intermediate area is included by Mr. Allen in his "North Temperate Realm," which embraces the whole of the northern hemisphere on both sides of the Atlantic, between the annual isotherms of 32° and 70°, but is allowed to rank independently as the "North American Region," while Dr. Merriam, following Cope, calls it the "Sonoran Region." Thus both these authorities agree in splitting up the Nearctic Region into three constituent parts, and in repudiating the views of Sclater and Wallace that it should form one of the main zoo-geographical divisions of the earth's surface. While, however, it must be allowed that both Mr. Allen and Dr. Merriam have studied the distribution of Mammal-life in North America to some effect, and are well acquainted with its details, it will be easy to show that their general views on geographical distribution are not entitled to acceptance. At the same time, we admit that there is something to be said on their side of the question.

Taking a general view of the distribution of Mammal-life over the terrestrial portion of the earth's surface, we see at once that Australia and the adjacent islands stand strongly apart from the rest

[1] i. "The Geographic Distribution of Life in North America, with Special Reference to the Mammalia." By C. Hart Merriam, M.D. *Proc. Biol. Soc. Washington*, vol. vii., p. 1 (April, 1892).

ii. "The Geographical Distribution of North American Mammals." By Joel Asaph Allen. *Bull. Amer. Mus. Nat Hist.*, vol. iv., no. 1 (Dec., 1892).

of the world as being principally tenanted by Marsupials, and as being the sole home of the existing Monotremes. Again, South and Central America together form a division easily separable from the remaining portion of the world, after Australia has been subtracted. The "Neotropical Region," as it is usually called, has a family of Marsupials peculiar to it; it has almost no Insectivores, and it is the home of all the three typical families of the great Order Edentata. Thus we have a very obvious threefold division of the earth's surface, taking Mammals as our text, into what may be called *Notogæa*, *Neogæa*, and *Arctogæa*. The vice of this division is that it leaves the great bulk of the earth's surface (*Arctogæa*), which remains after taking off the Australian and Neotropical Regions, of rather unmanageable size, consisting as it does of North America and the whole of the Old World except Australia and its islands. This large area, however, readily falls into four sections—North America, Europe and Northern Asia, Africa, and Southern Asia, which are denominated by Sclater and Wallace the "Nearctic," "Palæarctic," "Æthiopian," and "Oriental" regions respectively. No one pretends to say that these four regions are exact equivalents in zoological value to the two regions first spoken of—namely, the "Australian" and "Neotropical" regions. As Mr. Wallace ("Geogr. Distr.," vol. i., p. 66) has well put it:—

"It is admitted then that these six regions are by no means of precisely equal rank, and that some of them are far more isolated and better characterised than others; but it is maintained that, looked at from every point of view, they are more equal in rank than any others that can be found; while as regards geographical equality, compactness of area, and facility of definition, they are beyond all comparison better than any others that have yet been proposed for the purpose of facilitating the study of geographical distribution."

Mr. Allen and Dr. Merriam seem to be of opinion that when we come to scientific questions we are bound to throw away considerations of convenience altogether and to stick to matters of fact. This is a beautiful doctrine, but if the matters of fact are in dispute, as is the case in many of the details of geographical distribution, owing to our as yet imperfect knowledge of the subject, it is far better to adopt some easily comprehensible system, of which the leading features are obviously correct, than to use a more elaborate plan, based upon details that are more or less open to question. Contrast, for example, the six divisions of the world already given, their simple names and their easily-defined boundaries, with Mr. Allen's seven "Primary Life-Regions," their complicated titles and their uncertain limits. Where are we to draw the line between the "American Tropical" and the "South American Temperate Realm," or between the "Arctic" and the "North Temperate," both of which embrace portions of the Old and the New Worlds? Than Africa, as regards its Mammals at least, no part of the world, except Australia, has a better set of characteristic types, such as *Hippopotamus*, *Camelopardalis*, *Orycteropus*, *Hyrax*, and many others. Yet Mr. Allen proposes to

unite Africa with India as the "Indo-African Realm," while he would actually split up South America between two different regions. In these proposals, at least, very little consideration is shown to the much-lauded principle of the equivalence in value of the realms or regions.

While, however, we insist upon the propriety of retaining the Nearctic Region as a whole, and as one of the primary zoo-geographical divisions of the earth's surface, we are quite disposed to attend to the views of Messrs. Merriam and Allen as to the best mode of subdividing the large area, and to admire the maps and tables in which they have set it forth. Take, for example, Mr. Allen's first map of his so-called "Realms" of North America (*Bull. A. M. N. H.*, vol., iv., pl. v.). Here we find a threefold division of the contiment proposed into "Arctic," "Temperate," and "Tropical" Realms. The "Arctic Realm," which consists merely of the land bordering the Polar Ocean and Hudson's Bay and the great peninsula of Greenland, and is "beyond the limit of arboreal vegetation," Mr. Allen unites to the similar arctic portion of the Old World, stating, no doubt quite correctly, that it is really a part of a "homogeneous hyperborean fauna of circumpolar distribution." But looking to the extreme proverty of life in these inclement latitudes, as Mr. Allen well puts it, we think it quite unnecessary to elevate this wretched fraction of the earth's surface to one of its principal constituent life-regions, and must prefer the plan adopted by Sclater and Wallace, of regarding it as a borderland between the Nearctic and Palæarctic Regions.

We now come to Mr. Allen's "North American Region," which is regarded by the author as a subdivision of the "North Temperate Realm," corresponding in value to what is usually called the "Palæarctic Region" of the Old World, but what Mr. Allen prefers to denominate by the horrible compound term "Eurasia." Mr. Allen's "North American Region" embraces, as will be seen by a glance at the map already referred to, by far the largest portion of that continent. In fact, it embraces the whole, except the extreme arctic portion already referred to, and what Mr. Allen terms "Tropical North America," which consists of the southern end of the peninsula of Florida and a narrow strip of the coast of Mexico on both sides, extending on the Atlantic side up to the Rio Grande and on the Pacific side up to Mazatlan. This "Tropical North America" of Mr. Allen, however, is, in fact, merely the borderland between the Nearctic and Neotropical Regions of Sclater and Wallace, and may be left out of account when a general view of the great life-regions of the world is being taken. We thus see that Mr. Allen's "North American Region" is, for all practical purposes, identical with the Nearctic Region of Sclater and Wallace. Let us now see how Mr. Allen proposes to divide it—according to its Mammal-life.

"The North American Region," Mr. Allen tells us (*op. cit.*, p. 221), "falls into two sub-regions, namely (1) a 'Cold-Temperate Sub-

region' extending southward to about the mean latitude of the Great Lakes, with outlying portions extending further southwards along the principal mountain-system of the continent, and (2) a 'Warm-Temperate Sub-region' occupying the remaining area." This mode of division (which is shown on Mr. Allen's plate vi.) corresponds pretty nearly with that proposed by Dr. Merriam (see his Bio-geographic Map), who calls those two sub-regions "Boreal" and "Sonoran"; but upon comparing the maps of the two authors together, it will be seen at a glance that the minor subdivisions do not exactly correspond, and indeed such particulars must be always more or less matters of opinion.

In his carefully drawn up tables of North American Mammals, Mr. Allen shows that fourteen genera occur in his Cold-Temperate Sub-region, which do not range to any extent into the Warm-Temperate Sub-region. On the other hand, 33 genera found in the Warm-Temperate do not occur in the Cold-Temperate, while 27 genera are, to a greater or less extent, common to both their sub-regions. He points out that the 42 genera of the Cold-Temperate Sub-region are "either obviously of boreal origin, or find their nearest relationship with boreal types," while of the 62 genera which occur in the Warm-Temperate Sub-region, "about fourteen are wide-ranging southern or subcosmopolitan types, 24 may be regarded as indigenous, and about thirteen are of southern origin."

Neglecting, therefore, as we have already proposed to do, Mr. Allen's strip of the "Arctic Realm" in the North American Fauna as merely borderland, and, in a similar way, treating the area south of the Mexican Tableland and the extreme southern parts of the peninsulas of Lower California and Florida as merely debatable land between the Nearctic and Neotropical Regions, we find Mr. Allen's "North American Region of the North Temperate Realm" practically identical with the Nearctic Region of Sclater and Wallace. Following the guidance of Mr. Allen and Dr. Merriam, we recognise two sub-regions only in the region, namely, a Northern and a Southern one.

The Northern Sub-region is composed of Mr. Allen's "Cold-Temperate Sub-region," and the adjacent district on the north which he assigns to the "Arctic Realm." Here the general facies of the Mammal-fauna is much more decidedly similar to that of the Palæarctic Region than in the Southern Sub-region. Such genera as *Cervus*, *Alces*, *Rangifer*, *Ovis*, *Castor*, *Lagomys*, *Gulo*, and *Putorius*, betray at once a very strong Palæarctic element. At the same time, a decidedly endemic element is shown by such types as *Haploceros*, *Fiber*, *Condylura*, *Procyon*, *Mephitis*, and *Taxidea*, which are absolutely unknown in the Palæarctic Mammal-fauna. For this Northern Sub-region of the Nearctic Region perhaps "Canadian" would be the best term, as it embraces the whole of the Dominion of Canada, and the area of the sub-region is therefore at once recognisable by the name. Mr. Allen's term "Boreal" is much too vague.

The Southern Sub-region is composed of Mr. Allen's "Warm Temperate Sub-region," and the fragmentary bits of borderland to the south, which he assigns to the "American Tropical Realm." For this Sub-region "Sonoran," as used by Dr. Merriam, seems to be a good and appropriate term, much preferable to Mr. Allen's "Warm-Temperate," which is quite indefinite. The Sonoran Sub-region of the Nearctic Region, as we propose to call it, contains a much smaller Palæarctic element than the Canadian Sub-region. Nevertheless, *Bison*, *Tamias*, *Spermophilus*, *Arvicola*, and others, are undoubtedly Palæarctic types. On the other hand, *Antilocapra*, *Cynomys*, *Sigmodon*, *Oryzomys*, *Neotoma*, *Thomomys*, *Dipodomys*, *Scalops*, *Urotrichus*, and *Bassaris*, with their allies, constitute a strong endemic force. But what principally differentiates the Sonoran from the Canadian Sub-region is the presence in it of a lot of intruders from the Neotropical Region—such as *Didelphys*, *Dicotyles*, *Cariacus*, *Tatusia*, *Nasua*, and *Molossus*, which have nothing to do with the autochthonous fauna of North America. On the whole, the Nearctic Mammal-fauna may be defined as having a strong Palæarctic basis mixed up with endemic elements, and invaded largely on its southern frontiers by Neotropical immigrants. The presence of these Neotropical immigrants serves chiefly to distinguish the Sonoran from the Canadian Sub-region, though, as Mr. Allen has shown, there is also a material difference in the endemic forms peculiar to the respective Sub-regions.

VII.

The British Association Addresses, 1893.

SUCH authoritative expressions of opinion on current questions of scientific interest as always characterise the Presidential Addresses delivered to the British Association and its several Sections, are of importance in indicating the tendency of research; but it is not often that these general discourses contain much new matter. We are, therefore, not expressing any feeling of disappointment, when we remark upon the conspicuous lack of new facts and unfamiliar ideas in the addresses relating to Natural Science delivered last month at Nottingham. Everyone interested in Physiology, Petrology, Field Natural History, Arctic Exploration, and Anthropology, will have read the carefully-prepared digests of current thought and research with profit; and if there is little in them that is absolutely new, there are at least some personal expressions of opinion of the deepest significance.

Dr. Burdon Sanderson, the President, dealt with the present aspect of the problems of Physiology; and the whole address, full of the history of progress in the science, seems to have been designed to lead up to the climax—a plea for the establishment of a "British Institute of Preventive Medicine."

"It is possible that many members of the Association are not aware of the unfavourable—I will not say discreditable—position that this country at present occupies in relation to the scientific study of this great subject—the causes and mode of prevention of infectious diseases. As regards administrative efficiency in matters relating to public health, England was at one time far ahead of all other countries, and still retains its superiority; but as regards scientific knowledge we are, in this subject as in others, content to borrow from our neighbours. Those who desire either to learn the methods of research or to carry out scientific inquiries, have to go to Berlin, to Munich, to Breslau, or to the Pasteur Institute in Paris, to obtain what England ought long ago to have provided. For to us, from the spread of our race all over the world, the prevention of acute infectious diseases is more important than to any other nation. May I express the hope that the effort which is now

being made to establish in England an Institution for this purpose not inferior in efficiency to those of other countries, may have the sympathy of all present ?"

Physiology not Physics.

The final expression of Dr. Sanderson's opinion of the nature of physiological processes is of great interest. There is a certain "specific energy of cells" that neither physics nor chemistry can explain ; and physiology can never become a mere branch of applied physics or chemistry. Nevertheless, there are parts of physiology wherein the principles of these sciences may be applied directly. "Thus, in the beginning of the century, Young applied his investigations as to the movements of liquids in a system of elastic tubes, directly to the phenomena of the circulation ; and a century before Borelli successfully examined the mechanism of locomotion and the action of muscles, without reference to any excepting mechanical principles. Similarly, the foundation of our present knowledge of the process of nutrition was laid in the researches of Bidder and Schmidt, in 1851, by determinations of the weight and composition of the body, the daily gain of weight by food or oxygen, the daily loss by the respiratory and other discharges, all of which could be accomplished by chemical means. But in by far the greater number of physiological investigations, both methods (the physical or chemical and the physiological) must be brought to bear on the same question —to co-operate for the elucidation of the same problem. In the researches, for example, which during several years have occupied Professor Bohr, of Copenhagen, relating to the exchange of gases in respiration, he has shown that factors purely physical—namely, the partial pressures of oxygen and carbon dioxide in the blood which flows through the pulmonary capillaries—are, so to speak, interfered with in their action by the 'specific energy' of the pulmonary tissue, in such a way as to render this fundamental process, which, since Lavoisier, has justly been regarded as one of the most important in physiology, much more complicated than we for a long time supposed it to be. In like manner Heidenhain has proved that the process of lymphatic absorption, which before we regarded as dependent on purely mechanical causes—*i.e.*, differences of pressure—is in great measure due to the specific energy of cells, and that in various processes of secretion the principal part is not, as we were inclined not many years ago to believe, attributable to liquid diffusion, but to the same agency. I wish that there had been time to have told you something of the discoveries which have been made in this particular held by Mr. Langley, who has made the subject of 'specific energy' of secreting-cells his own. It is in investigations of this kind, of which any number of examples could be given, in which vital reactions mix themselves up with physical and chemical ones so intimately that it is difficult to draw the line between them, that the

physiologist derives most aid from whatever chemical and physical training he may be fortunate enough to possess."

Uniformitarianism in Geology.

Mr. Teall's address to the Geological Section is the most remarkable for startling expressions of opinion, and may be regarded as indicating another reaction in the ever vacillating body of enquirers into the history of the earth. Speaking as a petrologist, Mr. Teall remarks that although enormous progress has been made in this science during the last hundred years, there has been comparatively little advance so far as broad, general theories relating to the origin of rocks are concerned. He even declares his belief, that those who have deserted the old school of Uniformitarians for a certain modern creed of Evolution, will ere long discover their mistake. "The uniformitarian hypothesis, as applied to the rocks we can examine, has assimilated and co-ordinated so many facts in the past, and is assimilating and co-ordinating so many new discoveries, that we should continue to follow it, rather than plunge into the trackless waste of cosmogonical speculation in pursuit of what may after all prove to be a will-o'-the-wisp." " . . . The good old British ship, 'Uniformity,' built by Hutton, and refitted by Lyell, has won so many glorious victories in the past, and appears still to be in such excellent fighting trim, that I see no reason why she should haul down her colours either to Catastrophe or Evolution. Instead, therefore, of acceding to the request to 'hurry up,' we make a demand for more time. The early stages of the planet's history may form a legitimate subject for the speculations of mathematical physicists, but there seems good reason to believe that they lie beyond the ken of those geologists who concern themselves only with the record of the rocks."

Crystalline Schists.

The earliest sedimentary rocks in Britain, Mr. Teall admits, present some differences from the later rocks in this country, but he doubts whether any conclusions of universal application can be drawn from the fact. The common idea, however, that crystalline schists belong exclusively to the earliest periods of the earth's history, and therefore are witnesses of cosmic evolution, is combated in a vigorous manner. "The crystalline schists certainly do not form a natural group. Some are undoubtedly plutonic igneous rocks showing original fluxion; others are igneous rocks which have been deformed by earth-stresses subsequent to consolidation; others, again, are sedimentary rocks metamorphosed by dynamic and thermal agencies, and more or less injected with 'molten mineral matter'; and lastly, some cannot be classified with certainty under any of these heads. So much being granted, it is obvious that we must deal with this petrographical complex by separating from it those rocks about the origin of which

there can be no reasonable doubt. Until this separation has been effected, it is quite impossible to discuss with profit the question as to whether any portions of the primitive crust remain. In order to carry out this work it is necessary to establish some criterion by which the rocks of igneous may be separated from those of sedimentary origin. Such a criterion may, I think, be found, at any rate in many cases, by combining chemical with field evidence. If associated rocks possess the composition of grits, sandstones, shales, and limestones, and contain also traces of stratification, it seems perfectly justifiable to conclude that they must have been originally formed by processes of denudation and deposition. That we have such rocks in the Alps and in the Central Highlands of Scotland, to mention only two localities, will be admitted by all who are familiar with those regions. Again, if the associated rocks possess the composition of igneous products, it seems equally reasonable to conclude that they are of igneous origin. Such a series we find in the North-West of Scotland, in the Malvern Hills, and at the Lizard. In applying the test of chemical composition, it is very necessary to remember that it must be based, not on a comparison of individual specimens, but of groups of specimens. A granite and an arkose, a granitic gneiss and a gneiss formed by the metamorphosis of a grit, may agree in chemical and even in mineralogical composition. The chemical test would therefore utterly fail if employed for the purpose of discriminating between these rocks. But when we introduce the principle of paragenesis it enables us in many cases to distinguish between them. The granitic gneiss will be associated with rocks having the composition of diorites, gabbros, and peridotites; the sedimentary gneiss with rocks answering to sandstones, shales, and limestones. Apply this test to the gneisses of Scotland, and I believe it will be found in many cases to furnish a solution of the problem."

"... The origin of gneisses and schists, in my opinion, is to be sought for in a combination of the thermal and dynamic agencies which may be reasonably supposed to operate in the deeper zones of the earth's crust. If this view be correct, it is not improbable that we may have crystalline schists and gneisses of post-Silurian age in the North-West of Europe formed during the Caledonian folding, others in Central Europe of post-Devonian age due to the Hercynian folding, and yet others in Southern Europe of post-Cretaceous age produced in connection with the Alpine folding. But if the existence of such schists should ultimately be established, it will still probably remain true that rocks of this character are in most cases of pre-Cambrian age. May not this be due to the fact, suggested by a consideration of the biological evidence, that the time covered by our fossiliferous records is but a small fraction of that during which the present physical conditions have remained practically constant?"

The Polar Basin.

The address of Mr. Seebohm to the Geographical Section is perhaps the most readable, but it contains nothing new beyond six beautiful maps, illustrating the River Basins, Temperature, Rain and Snow, Heights and Depths, and Vegetation, prepared under the direction of Mr. E. G. Ravenstein. The general reader, however, will perhaps obtain a clearer broad view of the Arctic Regions from this carefully-prepared address, than from any previously-published description.

Zoo-geographical Regions.

Incidentally Mr. Seebohm expresses his views on the possibility of dividing the earth's surface into regions, and then makes some interesting remarks on the special case of the Polar Basin. "The fact is that life areas, or zoo-geographical regions, are more or less fanciful generalisations. The geographical distribution of animals, and probably also that of plants, is almost entirely dependent upon two factors, climate and isolation, the one playing quite as important a part as the other. The climate varies in respect of rainfall and temperature, and species are isolated from each other by seas and mountain ranges. The geographical facts which govern the zoological provinces consequently range themselves under these four heads. It is at once obvious that the influences which determine the geographical distribution of fishes must be quite different from those which determine the distribution of mammals, since the geographical features which isolate the species in the one case, are totally different from those which form impassable barriers in the other. It is equally obvious that the climatic conditions which influence the geographical range of mammals, must include the winter cold as well as the summer heat, while those which determine the geographical distribution of birds, most of which are migratory in the Arctic Regions, is entirely independent of any amount of cold which may descend upon their breeding grounds during the months which they spend in their tropic or sub-tropic winter quarters. Hence all attempts to divide the Polar Basin into zoological regions or provinces are futile. Nearly every group of animals has zoological regions of its own, determined by geographical features peculiar to itself, and any generalisations from these different regions can be little more than a curiosity of science. The mean temperature or distribution of heat can be easily ascertained. It is easy to generalise so as to arrive at an average between the summer heat and the winter cold, because they can be both expressed in the same terms. When, however, we seek to generalise upon the distribution of animal or vegetable life, how is it possible to arrive at a mean geographical distribution of animals? How many genera of molluscs are equal to a genus of mammals, or how many butterflies are equal to a bird? If there be any region of the world with any claim to be a life area, it is that part of the Polar

Basin which lies between the July isotherm of 50 deg. or 53 deg. F. and the northern limit of organic life. The former corresponds very nearly with the northern limit of forest growth, and they comprise between them the barren grounds of America and the tundras of Arctic Europe and Siberia. The fauna and flora of this circumpolar belt is practically homogeneous; many species of both plants and animals range throughout its whole extent. It constitutes a circumpolar Arctic region, and cannot consistently be separated at Behring Strait into two parts of sufficient importance to rank even as sub-regions. The mean temperature of a province is a matter of indifference to some plants and to most animals. The facts which govern their distribution are various, and vary according to the needs of the plant or animal concerned. To a migratory bird the mean annual temperature is a matter of supreme indifference. To a resident bird the question is equally beside the mark. The facts which govern the geographical distribution of birds are the extremes of temperature, not the means. Arctic birds are nearly all migratory. Their distribution during the breeding season depends primarily on the temperature of July, which must range between 53 deg. and 35 deg. F. It is very important, however, to remember that it is actual temperature that governs them, not isotherms corrected to sea-level. If an Arctic bird can find a correct isotherm below the Arctic circle by ascending to an elevation of 5,000 or 6,000 ft. above the level of the sea, it avails itself of the opportunity. Then the region of the Dovrefield above the limit of forest growth is the breeding place of many absolutely Arctic birds; but this is not nearly so much the case on the Alps, because the cold nights vary too much from the hot days to come within the range of the birds' breeding grounds. Here, again, the mean daily temperature is of no importance. It is the extreme of cold which is the most potent factor in this case, and no extreme of heat can counterbalance its effect."

The Field Naturalist.

Canon Tristram's address to the Biological Section, as might be expected, is devoted to the interests of the Field Naturalist. It is a matter of gratification, for once, to turn from the modern professor and his laboratory to a distinguished biologist whose work has been chiefly accomplished in the field. Observations of organisms in their natural surroundings are not merely essential for the understanding of such broad questions as geographical distribution, variation, mimicry, migration, and so forth; but they are also, as the Canon points out, often indispensable even in researches for which museum cabinets are commonly deemed to furnish adequate material.

"The closet systematist is very apt to overlook or to take no account of habits, voice, modification, and other features of life which have an important bearing on the modification of species. To take one instance, the short-toed lark (*Calandrella brachydactyla*) is spread

over the countries bordering on the Mediterranean; but, along with it, in Andalusia alone is found another species, *Cal. bætica*, of a rather darker colour, and with the secondaries generally somewhat shorter. Without further knowledge than that obtained from a comparison of skins, it might be put down as an accidental variety. But the field naturalist soon recognises it as a most distinct species. It has a different voice, a differently shaped nest; and, while the common species breeds in the plains, this one always resorts to the hills. The Spanish shepherds on the spot recognise their distinctness, and have a name for each species. Take, again, the eastern form of the common song-thrush. The bird of North China, *Turdus auritus*, closely resembles our familiar species, but is slightly larger, and there is a minute difference in the wing formula. But the field naturalist has ascertained that it lays eggs like those of the missel-thrush, and it is the only species closely allied to our bird which does not lay eggs of a blue ground colour. The hedge accentor of Japan (*Accentor rubidus*) is distinguished from our most familiar friend, *Accentor modularis*, by delicate differences of hue. But, though in gait and manner it closely resembles it, I was surprised to find the Japanese bird strikingly distinct in habits and life, being found only in forest and brushwood several thousand feet above the sea. I met with it first at Chinsenze—6,000 ft.—before the snow had left the ground, and in summer it goes higher still, but never descends to the cultivated land. If both species are derived, as seems probable, from *Accentor immaculatus* of the Himalayas, then the contrast in habits is easily explained. The lofty mountain ranges of Japan have enabled the settlers there to retain their original habits, for which our humbler elevations have afforded no scope."

Migration of Animals.

In the long-studied subject of the migration of birds and other animals, the field naturalist can also still find continuous and profitable occupation; and Canon Tristram calls attention especially to an exceptional phenomenon, "Not the mere wanderings of waifs and strays, nor yet the uncertain travels of some species, as the crossbill in search of food, but the colonising parties of many gregarious species, which generally, so far as we know in our own hemisphere, travel from east to west, or from south-east to north-west. Such are the waxwing (*Ampelis garrula*), the pastor starling (*Pastor roseus*), and Pallas's sandgrouse, after intervals sometimes of many years, or sometimes for two or three years in succession. The waxwing will overspread Western Europe in winter for a short time. It appears to be equally inconstant in its choice of summer quarters, as was shown by J. Wolley in Lapland. The rose pastor regularly winters in India, but never remains to breed. For this purpose the whole race seems to collect and travel north-west, but rarely, or after intervals of many years, returns to the same

quarters. Verona, Broussa, Smyrna, Odessa, the Dobrudscha, have all during the last half-century been visited for one summer by tens of thousands, who are attracted by the visitations of locusts, on which they feed, rear their young, and go. These irruptions, however, cannot be classed under the laws of ordinary migration. Not less inexplicable are such migrations as those of the African darter, which, though never yet observed to the north of the African lakes, contrives to pass, every spring, unobserved to the lake of Antioch in North Syria, where I found a large colony rearing their young, which, so soon as their progeny was able to fly, disappeared to the south-east as suddenly as they had arrived."

The Perfection of the Human Frame.

The last of the addresses requiring notice, that of Dr. Munro to the Anthropological Section, will scarcely bear abstracting. It discusses, in an interesting manner, the direct and collateral advantages conferred on man by the erect position of his frame; and there are some noteworthy remarks on the theory of Natural Selection as applied to man. Dr. Munro asks Dr. A. R. Wallace to explain why, in his philosophy, he dispenses with the operation of a "higher intelligence" in the early stages of man's evolution, and finds its assistance only requisite to give the final touches to humanity. We shall be interested to learn the reply.

SOME NEW BOOKS.

NATURALIST'S MAP OF SCOTLAND. By J. A. Harvie-Brown and J. G. Bartholomew. Edinburgh: John Bartholomew & Co., 1893. Price 2s. 6d.

THIS is a beautifully-executed map of Scotland, on the scale of 10 miles to an inch, full of information of value not only to the naturalist, but also to tourists and sportsmen. The various "Faunal Areas" are regarded as coincident with the river basins, and the lines of demarcation are accordingly marked along the watersheds. The areas of cultivated land, patches of woodland, bare moorland and other uncultivated land, with the deer forests, are all indicated by different colours. The areas above 1,000 and 2,000 ft. in elevation respectively are marked by special shading; and the depth of the sea is shown by various tints of blue. As the authors remark, much of their work is tentative and liable to modification by future research; but no stronger incentive to this important research could have been devised than the result of their painstaking labours now before us.

A CONTRIBUTION TO THE GEOLOGY AND NATURAL HISTORY OF NOTTINGHAMSHIRE. Edited by J. W. Carr, M.A., F.G.S. Small 8vo. Pp. 90. Nottingham: James Bell, 1893. Price 2s. net.

THIS little work was specially prepared for the use of members of the British Association during the recent meeting, and is a valuable compendium of the Geology, Zoology, and Botany of the district of which it treats. Contributions to the geological section are made by Messrs. J. Shipman and R. M. Deeley, and the Rev. J. M. Mello; the notes on birds were written by Mr. F. B. Whitlock, and the list of mollusca was compiled by Mr. B. Sturges Dodd. The list and notes on the vascular plants are contributed by Mr. H. Fisher. Nottinghamshire is a rather monotonous county of cultivated land, almost entirely within the drainage area of the Trent; and the greater part of its surface is occupied by the New Red Sandstone. The fauna and flora, however, is far from meagre, and much still remains to be done in recording the distribution of many groups both of animals and plants.

A BIOGRAPHICAL INDEX OF BRITISH AND IRISH BOTANISTS. By J. Britten and G. S. Boulger. 8vo. Pp. xvi., 188. London: West, Newman & Co., 1893. Price 5s.

WE are glad to welcome the re-issue, in one volume, of Messrs. Britten and Boulger's Index of Botanical Biography. It has been promised for some time, but the authors assure us that the amount of corrections and amplifications necessary have justified the delay. The

record has been carried down to the end of 1892, and the extent of the revision "may be gauged from the fact that, whereas the issue in the *Journal of Botany* comprised 1,619 names, occupying 148 pages, an average of over ten names to the page, in its present form our little book contains 1,825 names, and occupies 188 pages, an average of less than ten names to the page." Even had there been no additions we should have commended a re-issue, for a bibliography scattered through the pages of three volumes of the *Journal of Botany* compares unfavourably with a compact little volume like that now before us. As the title implies, the work is an index, "intended mainly as a guide to further information," and as such is admirable; references to the chief sources of further information being freely given.

The term botanist is held to include all who have in any way contributed to the literature of botany, who have made scientific collections of plants, or have otherwise assisted directly in the progress of botany, exclusive of pure horticulture. Mere patrons have, as a rule, not been included, or those known only as contributing small details to a local flora. The authors have done their work well and made a valuable contribution to botanical literature. Bibliography is an endless task and, doubtless, one might by toil find additions even to the long roll of 1,825 names, among which, by the way, we do not find that of Gilbert White, of Selborne, who surely might claim to be inserted equally with some of those whose names are included.

The book is nicely printed, and remarkably free from typographical errors.

A Descriptive Account of the Mammals of Borneo. By Charles Hose, F.R.G.S., F.Z.S. Pp. 78, with map and three plates. Diss, Norfolk: Edward Abbot, 1893.

The excellent exploring work which Mr. Charles Hose, one of the Residents in the service of the Rajah of Sarawak, has been doing in Borneo has been for some time known to most English scientific men, partly through the receipt by many different museums of zoological specimens of his collecting, and partly by the constantly-recurring descriptions in scientific periodicals of new vertebrates and invertebrates discovered by him in that wonderful island, the riches of which still seem so far from being exhausted.

During a recent visit to Europe, Mr. Hose has found time to compile, originally merely for his own and his friends' use in the jungle, the descriptions of all the mammals known to occur in the island; and at the request of some of his scientific friends he has added to the rough notes thus made some short original remarks drawn from his own knowledge of the species. This being the origin of the unpretentious little book above quoted, any detailed criticism of it would be out of place, and it should be accepted simply as the first rough basis on which, as we may hope, the author will found such a complete work as may be worthy of his intimate knowledge of the Bornean wild beasts and their ways.

The descriptions in this book are adapted almost or quite *verbatim* from the technical writings of recent workers on the subject, and mainly from Blanford's "Mammals of India," and Anderson's "Zoology of the Yunnan Expedition"; those of the species more recently discovered being, of course, taken from the original notices. Following the descriptions, Mr. Hose has given his own notes, which we hope will, in the future, be largely amplified, on the habits,

comparative rarity, ranges, native names, and other such particulars of the animals as can only be learnt on the spot, and then only by an enthusiastic lover of the subject.

In all 144 species are recognised as inhabitants of Borneo, and of these about twenty of all sorts, from monkeys to shrews and mice, have been added to the fauna by Mr. Hose and his friend and co-worker, Thos. A. H. Everett, to whose advice and encouragement zoological science is largely indebted for the enrolment of such an ardent recruit as our author has proved himself to be.

Such work as Mr. Hose's is a type of what ought to be going on all over the world, wherever Englishmen with their national love of sport and natural history are living in the midst of a fauna as yet undisturbed by the onward march of civilisation. As European settlements and colonies gradually cover the earth the native fauna is inevitably killed out, either directly for sport or food, or indirectly by the introduction of domestic and parasitic animals. Now, and for the next twenty years, is, therefore, the time that our museums should be filled with carefully-preserved specimens of all sorts, so that our successors may have some chance of actually seeing examples of the animals which will soon no longer exist in nature. There can be no greater absurdity than the recent clamour made by certain "nature-lovers" in decrying the killing of specimens for collections, since such collecting can have not the slightest appreciable effect in exterminating species in comparison with many other processes now going on, notably the barbarous slaughter of birds for their feathers; a method by means of which millions of individuals are killed and their spoils thrown away after a year or two's administering to someone's vanity and thoughtlessness. How great is the destruction thus worked is to be gathered from the fact that the British Museum collection of birds, the largest in the world, does not consist of so many individual skins as have been sold for the purposes of fashion at a single day's sale at a City warehouse. But, protest as we may, the sad fact has to be faced that many of the most interesting and beautiful species now existing are doomed to destruction, and the liberal storage of specimens in museums for careful and long preservation appears to be the only means to give our successors the advantage we ourselves enjoy; and to decry such accumulations is a form of sentimental selfishness of which we hope no reader of NATURAL SCIENCE will be guilty. Such work as Mr. Hose's, therefore, combining, as it does, both observation and collection, is worthy of the highest commendation, and we may well wish him success in his future studies of the rich Bornean fauna.

O. T.

THE AMPHIOXUS AND ITS DEVELOPMENT. By Dr. B. Hatschek. Translated and Edited by James Tuckey, M.A., Lecturer in the University of Durham. Pp. 181, with nine full page illustrations. London: Swan Sonnenschein & Co., 1893. Price 6s.

IT is difficult to know exactly what to say about this book. It is a bald translation of Dr. Hatschek's researches on Amphioxus with marvellously incapable reproductions of the beautiful figures. The excuse of the "translator and editor" for the book is that hitherto "these investigations have not been accessible to that portion of the scientific world which does not read German." It may be laid down dogmatically that that portion of the scientific world which does not read German has no business with original memoirs. The results of

memoirs are collated and simplified for them in a multitude of excellent text-books, and the perusal of translations of isolated researches can but bloat them up with unassimilated materials, and give them a lopsided view of the animals they wish to study. Take this particular case. Dr. Hatschek has written more and discovered more about Amphioxus than any other investigator; but many others have written on it, and our total knowledge of Amphioxus is based on a very large number of original papers. About these others the "editor" says no word, and anyone judging by this book would conclude that he had exhausted the subject, whereas by a little laboratory work and a perusal of "Marshall and Hurst" or Parker's "Elementary Biology" he would have learned much more. The most astonishing thing about this publication is that it forms one of a series of "Introductory Science Text-Books," for original papers come at the end, not at the beginning of biological work.

Report on the European Methods of Oyster-culture. By Bashford Dean. Bull. U.S. Fish Commission for 1891. Pp. 357-406, pls. lxxv.-lxxxviii. 1893.

This report is drawn up on the author's personal observations made during a tour through Europe for the special purpose in 1891. Previous reports by Dr. Dean have been noticed in this Journal for Feb., 1893. Detailed descriptions of oyster-culture as carried on in Italy, Spain, Portugal, Germany, Holland, Belgium, and England are given, that of France forming the subject of one of the previous reports. Dr. Dean gives an account of the actual processes of oyster-culture, shows in a general way the influence exerted upon the industry by Government concessions or restrictions, and deals with a variety of questions relating to the living conditions of the oyster. The report is full of useful and practical remarks, is illustrated by photographs taken by the author, and we can only repeat what we said on the previous occasion, that it should be furnished to every grower around our coasts.

Ethnographie Nordost-Afrikas: Die materielle Cultur der Danâkil, Galla und Somâl. By Philipp Paulitschke. 8vo. Pp. 338, with 25 plates containing 100 figures and map. Berlin: 1893. Price 20 marks.

This volume commences with a geographical sketch of the district, notes on the peoples, the pure and mixed races, and then deals with their culture. Clothing, ornament, weapons, building construction, utensils, foods, physiology and hygiene, family and social life, are all taken in turn and fully dealt with; these are followed by chapters on economics, such as production and sale of valuables, imports, value of the natives and their work. The plates represent the natives themselves and their industries; and the map, on the scale of 1 : 4,000,000, shows the distribution of the various races.

Guide to the Siebengebirge. [Führer durch das Siebengebirge.] By B. Stürtz. Bonn: A. Henry. 1893.

Naturalists visiting the Rhine will welcome this valuable little guide-book as affording not merely the usual information for tourists, but also a concise and interesting notice of the Geology, Zoology,

and Botany of the region immediately to the south of Bonn. It is written in popular style by Mr. Stürtz, the well-known dealer in geological specimens at Bonn, and is accompanied by a frontispiece giving a general view of the Siebengebirge. There are also lists of maps and works of reference.

C

A Classed and Annotated Bibliography of the Palæozoic Crustacea, 1698–1892, to which is added a Catalogue of North American Species. By Anthony W. Vogdes. Occasional Papers IV., California Academy of Science. 8vo. Pp. 416. San Francisco: June, 1893. Price 10s.

This valuable book, as its title indicates, is divided into parts. The bibliography (part 1) occupies 252 pages, and is followed by a catalogue of Trilobites (part 2), arranged firstly under the families, and secondly in alphabetical order of genera. The third part (pp. 361–414) treats of the non-trilobitic genera and species, which follow precisely the same convenient arrangement as do the Trilobites. The volume appears to be compiled in the author's usual careful manner.

The Clarendon Press announces the issue of part i. of Mr. B. D. Jackson's "Index Kewensis," a monumental index to the names (with authorities) of all known flowering plants, with an indication of their geographical distribution. The part comprises 728 pp., 4to, and is issued to subscribers at two guineas net. Sir Joseph Hooker, who has supervised the work, thus explains its origin:—"Shortly before his death, Mr. Darwin informed me of his intention to devote a considerable sum in aid or furtherance of some work of utility to biological science; and to provide for its completion, should this not be accomplished during his lifetime. He further informed me that the difficulties he had experienced in accurately designating the many plants which he had studied, and ascertaining their native countries, had suggested to him the compilation of an Index to the Names and Authorities of all known Flowering Plants and their Countries, as a work of supreme importance to Students of Systematic and Geographical Botany, and to Horticulturists, and as a fitting object of the fufilment of his intentions. I have only to add that, at his request, I undertook to direct and supervise such a work; and that it is being carried out at the Herbarium of the Royal Gardens, Kew, with the aid of the staff of that establishment."

The third part of the new edition of the Molluscan portion of "Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs" is just out, and permits one to judge of its merits. So far the work is in every way a worthy successor to the previous edition, and the fact that it is being brought out under the able editorship of Dr. Simroth, of Leipsic, is a guarantee that this standard of excellence will be maintained to the end.

The first two lieferungen, issued last year, were devoted to an historical summary; the new part, which ranks as lief. 3 to 6, contains a general bibliography and the beginning of the Aplacophora, with the first four plates.

By some curious oversight, while many of the minor papers on

molluscan adults are duly noted in the bibliography, the standard work by Troschel, "Das Gebiss der Schnecken," has been omitted.

The treatment of the subject matter of the work, as evinced by the portion before us, leaves little, if anything, to be desired, and the plates are most excellent. It is to be hoped that the work will not take as long to complete as some other Teutonic productions that could be named.

Mr. H. A. Pilsbry has completed and issued the last part of vol. xiv. of Tryon's "Manual of Conchology." The volume is illustrated with 68 plates, and is prefaced by a general account of the structure and classification of the Chitons. An index to these molluscs will appear with the next part, which completes their description.

The last part (fasc. v.) of Professor A. de Lapparent's "Traité de Géologie" (pp. 1281 to end) has been issued by F. Savy, Paris. Messrs. Macmillan & Co. announce the preparation of a third edition of Sir Archibald Geikie's well-known Text-book.

Messrs. W. H. Allen & Co. will shortly publish a new edition of "Our Reptiles and Batrachians," by Mr. M. C. Cooke, which has long been out of print. The work has been revised and corrected to date by the author with some additional matter. The coloured plates are reproduced in chromolithography.

The same publishers have also nearly ready for publication a "Handbook of British Hepaticæ," by Mr. Cooke, with introduction and full description of all the genera and species hitherto found in the British Isles, illustrated by 200 woodcuts and seven plates. The work will be the only complete guide to the subjects which has been published in this country for upwards of a quarter of a century.

OBITUARY.

EDUARD SCHNITZER.

BORN MARCH 28, 1840. DIED DECEMBER, 1892.

NEWS has been received during the past month of the sad murder of Dr. Eduard Schnitzer, better known as Emin Pasha. The event took place at the close of last year.

Eduard Schnitzer was born on the 28th of March, 1840, in Oppeln, in the Prussian province of Silesia. He was the son of Ludwig Schnitzer and Pauline his wife. The family removed to Neisse in 1842. After being educated in the Gymnasium of Neisse, Eduard commenced the study of medicine at Breslau in 1858. He completed his studies in Berlin, where he attended the University during 1863–1864 and graduated.

A strong desire for travel led the young doctor to look for a sphere of work in a foreign land, and leaving Berlin at the end of 1864, he went to seek a practice in Turkey. Chance led him to Antivari and Scutari, where he obtained the confidence of the Vali Mushir Divitji Ismail Hakki Pasha, from whom he received a post on his staff, and whom he accompanied on his official journeys throughout the various provinces of the extensive district under his jurisdiction. In this way Schnitzer became acquainted with Armenia, Syria, and Arabia, and at length Constantinople, where Hakki Pasha died in 1873.

In 1875, Schnitzer returned to Neisse, devoting his leisure to Natural History. Suddenly he started off for Egypt, and in 1876 we find him entering the Egyptian service as Dr. Emin Effendi. He was ordered to join the Governor-General of the Soudan at Khartoum, and from there was sent to act as chief medical officer in the Equatorial Provinces, of which Gordon was the Governor.

In 1878, on Gordon accepting the Governor-Generalship of the Soudan, he appointed Emin Governor of the Equatorial Provinces. Immediately setting to work to restore matters, his success was unbounded, and by 1879, Felkin says, a most wonderful change had taken place. "Stations had been rebuilt, discontent was changed to loyal obedience, corruption had been put down, taxation was equalised, and he had already begun the task of clearing his province from the slave-traders who infested it." All this, with the exception of a few months' help from Lupton

Bey, he did entirely alone and unassisted. He received the title of Bey in 1879, and by the end of 1882 he was able to report that not only was his province at peace and contented, but that he had entirely banished the slave-traders from his borders. He was also able to show a profit of £8,000 in that year, whereas when he took up the work there was a deficit of £32,000 per annum.

The reader is referred to Felkin's life of Emin, which forms the Introduction to "Emin Pasha in Central Africa," London, 1888, from which much of our information is taken.

EDWARD CHARLESWORTH.

A REMARKABLE man in many respects was Mr. Charlesworth, whose death, at an advanced age, took place recently at Saffron Walden. Educated at Guy's Hospital, he gained a good general knowledge of Comparative Anatomy, and while still a student, he came prominently into notice by the publication in 1835 of a masterly paper on the "Crags of East Anglia." He then pointed out that the Crag of Suffolk was divisible into two portions, termed respectively the Coralline and the Red Crags. Those divisions were accepted by Lyell, and their names have become permanently established. Later on he pointed out that in Norfolk a newer division occurred, and this he named the Mammaliferous Crag, now generally known as the Norwich Crag.

In 1836, Charlesworth was temporarily employed in the British Museum, but, having busied himself with the invention of an "elevator gun," he soon retired to join an expedition to Mexico.

In 1837, he succeeded Loudon as Editor of the *Magazine of Natural History*. He commenced a new series and edited four volumes, terminating his connection in 1840, when the *Magazine* was united with the *Annals of Natural History*, that had been started two years previously.

About the year 1843, Charlesworth planned the publication of the *London Geological Journal*, but the first number did not appear until September, 1846, owing chiefly to the fact that in 1844 he was appointed Curator of the York Museum. The *Journal* was profusely illustrated with plates, and it contained valuable contributions from the leading palæontologists of the day. Charlesworth himself gave an account of the occurrence of flint in the pulp-cavity of a tooth of *Mosasaurus*. Three numbers only of this *Journal* were published, the last being issued in May, 1847.

In 1858, Charlesworth gave up his Curatorship at York, where he was succeeded by the late W. S. Dallas, and eventually settled for a time in London. From this date, he appears to have made a somewhat precarious livelihood, chiefly by the sale of specimens. He formed the "British Natural History Society," which consisted only of himself, and undertook the disposal more especially of the Tertiary

and Recent Mollusca. He prided himself also on being the first to introduce glass-topped boxes for the preservation of delicate specimens. He would appear occasionally at a scientific meeting, when there was a discussion on flints, in which he took an especial interest; and for several years he successively attended the anniversary meetings of the Geological Society, then held at Somerset House, and argued at length, with great fluency, and in loud tones, about the management of the Society's affairs. While his speeches contained, at times, matter well deserving of discussion, his manner too often was aggressive and needlessly offensive; and this want of tact was displayed in some of the critical remarks he introduced into the journals he edited. From these and other causes Charlesworth came gradually to lose position and friends. Starting, as he did, so full of promise, with ability of a high order and much enthusiasm, it is sad to think how in later years he neglected his talents and misused his opportunities, for the work of his early years, though not great, is sufficiently important to form an enduring contribution to the literature of geology.

LEONARD BLOMEFIELD (JENYNS).

BORN 1800. DIED SEPTEMBER 1, 1893.

THE last of Darwin's associates in the preparation of "The Zoology of the Voyage of H.M.S. 'Beagle,'" has passed away in the person of the Rev. Leonard Blomefield (formerly Jenyns), who contributed the section on the fishes. He was the youngest son of the late Rev. George Leonard Jenyns, of Bottisham Hall, Cambridgeshire, and for many years had resided at Bath. He graduated at Cambridge, and his first paper on the Ornithology of Cambridgeshire, was read before the Cambridge Philosophical Society in 1825. Between that year and 1833 he also contributed to the Transactions of the same Society papers on the reptiles of Cambridgeshire, the habits of the natterjack toad, a mite parasitic on slugs, a swarm of flies, and on the British species of *Cyclas* and *Pisidium*. In 1834 he presented a report on "The Recent Progress and Present State of Zoology" to the British Association, and in the following year he published at Cambridge his well-known "Manual of British Vertebrate Animals." Eleven years later he also published another small work, entitled "Observations in Natural History: with an Introduction on habits of observing, as connected with the study of that Science. Also a Calendar of Periodic Phenomena in Natural History." He was especially devoted to field observations, and during the last fifty years made many contributions to scientific literature, his last being a Presidential Address to the Bath Natural History and Antiquarian Field Club in 1892. Both in this Club and in the Bath Institution, Mr. Blomefield took the deepest interest, and the Institution is indebted to

him for a fine scientific library of nearly 2,000 volumes, besides a good herbarium.

ALEXANDER STRAUCH.

THE death is announced of the eminent herpetologist, who has for many years been Director of the Museum of the Imperial Academy of Sciences at St. Petersburg. Though only in his sixty-second year, Dr. Strauch had been in feeble health for some time before his death, and had been unable to make much progress in his favourite studies. His more important memoirs were published by the Academy, and comprise, among others, an essay on the Herpetology of Algeria (1862), studies of Chelonia (1862, 1865, and 1890), a synopsis of the Crocodilia (1866), a synopsis of the Viperidæ (1869), a revision of the Salamanders (1879), on the Snakes of Russia (1873), and on the Geckos (1887). Dr. Strauch's memoir on the Russian snakes is especially valuable, and it is a misfortune that he was unable to complete similar memoirs on the other reptiles and batrachians of the empire. His contemplated description of the reptiles collected by Przewalski remains unfinished.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

PROFESSOR C. H. TYLER TOWNSEND, of Las Cruces, New Mexico, has been appointed Curator of the Museum and Institute, Kingston, Jamaica.

MR. L. J. SPENCER, B.A., of Sidney Sussex College, Cambridge, is the successful candidate in the recent examination for the vacant Assistantship in the Mineralogical Department of the British Museum. Mr. Spencer was awarded the Harkness Scholarship last June, and before entering upon his appointment will spend the remainder of this year in visiting the Museums and Universities of the Continent.

THE Scientific Library of the late Professor J. S. Newberry, has been presented as a memorial to the Geological Department of Columbia College, New York.

A VALUABLE concise report on the Marine Biological Laboratories of Europe, by Dr. Bashford Dean, appears in the July and August numbers of the *American Naturalist*.

ACCORDING to *Indian Engineering*, a wealthy citizen of Calcutta has offered a sum of money for the purpose of building a library in the Zoological Gardens of that city. The new laboratory at the Gardens is completed, and will shortly be ready for use.

THE first of a series of Stratigraphical Memoirs, published by the Geological Survey, was on the Pliocene Deposits of Britain, by Mr. Clement Reid, and was issued in 1890. Now a further instalment has been published, in two volumes, on the Jurassic Rocks of Yorkshire, by Mr. C. Fox-Strangways. These works summarise our knowledge on the various formations and their subdivisions, and they contain full lists of the fossils.

THE Annual Report of the Madras Museum, 1892–93, records, among other additions, the mounting of a stuffed specimen of the rare shark, *Rhinodon typicus*, cast ashore at Madras in 1889. A figure is given. During the year the Superintendent has visited the corundun deposits, and collected a series of Cretaceous fossils near Pondicherry. A collection of ammonites has been sent to Vienna, for study by Dr. W. Waagen.

THE Annual Report of the British Museum for 1892 has been issued. The Director of the Natural History Departments states that the building of a temporary room for the accommodation of the Cetacea is under contemplation; and special allusion is made to the inconvenience caused by the want of a lecture room. The Swiney Lectures on Geology, by Professor Nicholson, will be delivered this year in the Lecture Theatre of the neighbouring South Kensington Museum. The subject is the Bearing of Geology on the Geographical Distribution of Plants and Animals, and the course commences on October 2.

A beautiful new example of protective resemblance in animals has been added to the collection in the hall of the Natural History Branch of the British Museum. It is a beetle (*Lithinus nigrocristatus*) from Madagascar, living on lichen, and adorned in such a manner as to precisely resemble the latter. A very fine collection of madreporarian corals from Western Australia has just been sent to the Zoological Department by Mr. W. Saville Kent, and a nearly complete skeleton of the extinct New Zealand rail, *Aptornis*, has been added to the exhibited collection in the Geological Department.

The political disturbances in Bohemia are seriously retarding the arrangement of the Natural History collections in the new Royal Bohemian Museum at Prague. The annual grant of money this year has been greatly reduced. The cases for the Barrande collection of fossils are still unfinished, and probably will not be ready until next spring; while there is little prospect of the completion of the new cases for the other fossils within the next two years. Considering the lavish expenditure on the architectural features of the Museum, this niggardliness in providing fittings is much to be deplored. For the student of Palæozoic fossils, the Bohemian collection is perhaps the most important in Europe, and any delay in rendering it of service is a great misfortune.

The Proceedings of the meeting of the German Anatomical Society, held last May, have just been issued as a supplement to the *Anatomischer Anzeiger*, forming a small volume of 224 pages.

The Norfolk and Norwich Naturalists' Society has established a "Yarmouth Section" at Great Yarmouth, with the Rev. C. J. Lucas as Chairman, and Mr. Arthur Patterson as Hon. Sec.

The Haslemere Natural History Society is interesting itself in the lectures on Technical Education provided by the Surrey County Council. It has organised a "Teaching Committee" to supplement, by class work, the ordinary lectures.

The ninth part of vol. i. of the *Journal of the Trinidad Field Naturalists' Club* has been issued. Besides the reports of meetings, it contains papers on Composite Plants, Mosquitoes, Ticks on an Iguana, etc. Mr. F. W. Urich's account of the mosquito pest is of special interest, and the Club is doing wisely in restricting its operations to local research. Several members are occupied in collecting the mammals of Trinidad, in preparation for a forthcoming work on the subject by Mr. O. Thomas, of the British Museum.

Timehri, the journal of the Royal Agricultural Society of British Guiana, is nearly always the most interesting of Colonial publications. The latest part (vol. vii., pt. i.) is especially readable, and contains some valuable contributions. Mr. James Rodway, the editor, discusses the seasons in Guiana; an account of the Indians of Guiana is translated from an old Dutch work, published in 1770; Mr. H. I. Perkins contributes notes on a journey to the Cuyuni Gold-Mining District; and Mr. H. C. Swan records his experiences of insect-collecting in British Guiana.

The newly-issued part of the *Proceedings of the Liverpool Geological Society* (vol. vii., pt. ii.) contains Mr. W. Hewitt's Presidential Address on the New Red Sandstone with reference to its mode of formation, and several other papers of much interest. Mr. Mellard Reade gives an elaborate and well-illustrated account of Glacial Deposits of North Wales, discussing their possible origin; and Dr. C. Ricketts treats of the conditions under which the older Carboniferous rocks of N.W. England were formed. Messrs. P. Holland and E. Dickson discuss the formation of clay, and there are some descriptive papers on local geology.

THE recent meeting of the British Association at Nottingham (Sept. 13 to 20), though receiving scarcely any communications of absolute novelty, was full of interest; and the arrangements of the local committee were all that could be desired. The attendance, also, though not remarkably large, was at least equal to that of most recent meetings. In addition to the Presidential Addresses, which we notice elsewhere, there were several interesting discussions to attract widespread attention, notably the debates on the nature of life (opened by Dr. J. S. Haldane), on coral reefs (opened by Professor Sollas), and on the place of geology in education (led by Professors Cole and Lebour). A more futile discussion than the attempt to settle the limits of Geology and Geography, we do not remember to have heard.

THE section for Geology seems to have been favoured with the greatest number of papers, and the Petrologists, as might have been expected, mustered in considerable force. There were several contributions to local geology, Mr. A. T. Metcalfe's description of the gypsum deposits being the most elaborate. Professor Brögger (of Christiania) discussed some eruptive rocks in Norway, and Professor Iddings (of Chicago) described a dissected old volcano in Wyoming. Professor Johnston Lavis presented his usual report on Vesuvius, and Professor Sollas explained the origin of intermediate varieties of igneous rocks by intrusion and admixture. Mr. Walcot Gibson gave a general sketch of the geology of Central East Africa, and Mr. R. D. Oldham exhibited two new geological maps of India. Dr. H. Hicks reiterated his views on the base of the Cambrian Formation; and Mr. Goodchild showed how the "eyes" in gneiss give a clue to its origin. In Glacial Geology, Prince Kropotkin attempted the widest theme in contributing a paper on the glaciation of Asia; Professor Sollas exhibited a map of the eskers of Ireland; Mr. De Rance traced the pre-Glacial form of the ground in Lancashire and Cheshire; and Messrs. Abbott and Kendall had some more remarks on the "heretics" who believe that North Wales was submerged to a great extent in the Glacial Period. Professor Bonney and Mr. Lindvall also spoke on theoretical matters in Glacial Geology. In Palæontology, Mr. E. T. Newton gave the latest information about his Triassic reptiles from Elgin; and Dr. Traquair recorded a cephalaspidian fish from Caithness. In addition to the papers, the usual reports on Underground Waters, Erratic Blocks, Photographs, Palæontology, etc., were presented.

THE Biological Section had to lament the absence of the President on account of ill-health. Among the more fundamental questions, Professor J. B. Farmer treated of some new features in the division of the nucleus in plant-cells, and Messrs. Hartog and Dixon discussed the digestive ferments of a large protozoon. Messrs. Cattle and Millar read a paper on Gregarines and the possible connection of allied forms with tissue-changes in man, notably with the production of cancer. Mr. W. E. Hoyle described the luminous organs of Cephalopoda, and Mr. F. T. Mott discussed the origin of organic colour. Mr. F. Enock spoke on insect parasites, and Mr. G. B. Rothera on vegetal galls. Mr. W. S. Bruce gave an account of the seals and whales seen during his recent voyage to the Antarctic regions, and Dr. C. H. Hurst made the observations on birds' wings which we print elsewhere. The most generally interesting botanical paper was Miss N. Layard's description of the root of the duckweed (*Lemna*). Mr. J. Clark treated of lime in its relation to some physiological processes in the plant, and Mr. H. Brown had a paper on starch. There were numerous interesting reports of committees, that dealing with the legislative protection of the nests of wild birds leading to the warmest discussion.

THE Geographers were treated to some entertaining exhibitions of lantern slides, Mr. W. M. Conway bringing his views of the Karakoram Mts., and Professor John Milne showing some pictures of Japan with especial reference to earthquakes. Mr. Murdock also exhibited his paintings lately made in the Antarctic regions. Besides

numerous addresses by travellers, there were papers by Mr. J. Y. Buchanan on the influence of land on the temperature of the air; by Mr. H. N. Dickson on the sea between Scotland and the Faroe Isles; and by Dr. H. R. Mill on the Clyde Sea Area and on the English Lakes.

The Anthropologists began with Mrs. Grove's paper on the Ethnographic Aspect of Dancing, and had numerous interesting discussions both on Ethnographical and Antiquarian subjects. The event of the meeting seems to have been Mr. A. Bulleid's description of the ancient British village recently explored by him near Glastonbury. Mr. E. W. Brabrook communicated the first report of the committee for undertaking an ethnographic survey of the British Isles; and Professor Hans Hildebrand, of Stockholm, discussed Scandinavian antiquities. Dr. J. H. Gladstone also started a somewhat indecisive discussion on the possibility of recognising a copper age between the ages of stone and bronze.

The General Committee of the British Association awarded grants of money for investigations in Natural Science as follows:—Erratic blocks, £15; Fossil Phyllopoda, £5; Geological photographs, £10; Shell-bearing deposits, £5; Eurypterids, £5; New sections of Stonesfield Slate, £25; Earth tremors, £50; Exploration of Calf Hole Cave, £5; Table at Naples Zoological Station, £100, and at Plymouth Station, £15; Zoology of Sandwich Islands, £100; Zoology of Irish Sea, £40; Mammalian heart, £10; Climatology and hydrography of Tropical Africa, £10; Observations in South Georgia, £50; Exploration in Arabia, £30; Anthropometric Laboratory statistics, £5; Ethnographical survey of United Kingdom, £10; The Lake Village at Glastonbury, £40; Anthropometrical measurements in schools, £5; Mental and physical condition of children, £20; Corresponding societies, £25. The total amount expended in grants was £705.

The British Association meets next year at Oxford early in August, under the presidency of the Marquis of Salisbury, Chancellor of the University. A new section for Physiology will then be inaugurated. The invitation of the town of Ipswich has been accepted for 1895.

The nomination of the Marquis of Salisbury for the Presidency, which has met with general approval, was proposed by Sir Frederick Bramwell and seconded by Sir William Flower. It is rarely that one sees gratitude for past favours so tactfully expressed as in the remarks of Sir William Flower, reported in the *Nottingham Daily Guardian* of September 19. Sir William said: "Lord Salisbury had shown general sympathy with all branches of science for a long time, and he was the first Prime Minister to recognise that men of science might sometimes aspire to honours and distinctions which had hitherto been reserved for successful soldiers or barristers and others. He had made a scientific man on his own merits a member of the peerage, and had appointed another purely scientific man a member of the Privy Council."

The Toynbee Hall Natural History Society, Whitechapel, is still actively pursuing its good work in the East End of London. During the winter, meetings are regularly held at the Hall, and during the summer cheap excursions, of long and short duration, are organised to various districts of interest. The longest excursion of the past season was a visit of fifteen days to the island of Jersey. The party, numbering seventeen, started on Friday, July 28, and returned Saturday, August 12. The headquarters during the whole of the time were at Gorey, on the S.E. coast of the island. Those taking part in the expedition represented three sections—Botany, Geology, and Zoology. For the purposes of systematic work, a portion of the island was allotted to each day, so that the whole of the coast and much of the interior was covered during the visit.

The effects of the drought in Jersey were not so evident as they are in England, and most of the plants for which the Channel Islands are noted were secured for the Society's herbarium. Many of them were in great abundance, and included—*Matthiola sinuata*, R. Br.; *Polycarpon tetraphyllum*, L.; *Lythrum Hyssopifolia*, L.; *Hypericum linarifolium*, Vahl.; *Gnaphalium luteoalbum*, L.; *Centaurea aspera*, L.; *Armeria plantaginea*, Willd.; *Microcala filiformis*, Link.; *Echium plantagineum*, L.; *Scrophularia Scorodonia*, L.; *Sibthorpia europæa*, L.; *Scilla autumnalis*, L.; *Cyperus longus*, L.; *Scirpus pungens*, Vahl.; *Fibichia umbellata*, Koch.; *Bromus maximus*, Wesf., and *Briza minor*, L.

It is worthy of note that in the north-east part of the island, at least, *Hypericum humifusum*, L., could not be found, but the form *H. decumbens*, Petermann, was abundant on the roadsides. It is, therefore, not very evident that *H. decumbens* is a hybrid between *H. humifusum* and *H. linarifolium*.

The ferns were not good or abundant except in the north-west. Those members inclined for shore work received valuable aid respecting the best hunting grounds, etc., from Mr. Sinel.

The Geologists were able to collect specimens of nearly all the rocks described from the island, including very fine examples of the Pyromerides of Bouley Bay. Owing to the complexity of structure of the island, the relations of the rocks to one another were in many cases not well made out, but the intrusion of the Granite series into the Diorite was well seen on the north. The great series of dykes were examined all along the coast, and the results of weathering, in the formation of hollows and caves, where they cut the harder granites, attracted general attention. A good series of rocks was secured for the Society's museum, and the micro slides, which are in course of preparation, will be exhibited at one of the winter meetings.

For the last two years, two long excursions, in addition to the usual Saturday and Sunday excursions, have been successfully carried out by Toynbee Hall. The plan of camping has been found to answer well, since the expenses are small; and this is a point of considerable importance, for one great object of the Society is to bring these Natural History outings within the reach of a large number. In the early summer of this year a camp was pitched for eight days at Thursley, Godalming, within a short distance of Hind Head. The cost for eight days' camp, including railway fare, was 24s., and the cost per member for a fortnight in Jersey, including return ticket, was 68s. The Society dispensed with tents in Jersey, as a farm-house was found sufficiently large to accommodate the whole party.

CORRESPONDENCE.

Phylogeny and Ontogeny.

I REGRET that Mr. Bather should have troubled himself to reply to me while he had not access to my original paper, and while he was subject to the inevitable distractions and inconveniences of travelling. Under such conditions it was probably impossible for him to avoid inadvertent misrepresentation.

I did not "deny that the past history of its race has *any* influence on the growth-stages of an individual," but only "that the phylogeny can *so* control the ontogeny as to make the latter into a record of the former—even into an imperfect record of it." This, however, is probably what Mr. Bather meant. But there is a very real misrepresentation two or three sentences further down on the same page (238). I have never "maintained that the development of any [=every] individual was a regular progress from the embryo to the adult." Not only am I pretty familiar with Weismann's work on the Diptera, but I have myself worked out in some detail the development of *Culex*, in which the alimentary, respiratory, muscular, and nervous systems and, to a smaller extent, other systems and organs also, exhibit phenomena (histolysis, for instance) which could not by any stretch of the imagination be called "regular progress from embryo to adult." The phenomenon called "metamorphosis" is too familiar for any zoologist to make any such statement as that ascribed to me.

I have already answered Mr. Bather's question elsewhere, but as he says "it is needless to reply" to my "other remarks while the above question remains unanswered," I will answer it again; merely premising that he has so far not replied to any of *my* "remarks" on the subject under consideration, but only to "remarks" which he has mistakenly ascribed to me.

His question is this:—"What cause can have produced these deviations of ontogeny from the path of simple development?" the deviations in question being those which he described in NATURAL SCIENCE, vol. ii., pp. 275 *et seqq*. My answer is that the species have so varied under the guidance of Natural Selection that the *later* stages of development have come to differ more widely from the *corresponding* stages of the ancestors than the *early* stages have come to differ from the *corresponding* stages of the same ancestors. The *vestiges*—I fully admit that they *are* vestiges—still to be found in the embryos of the existing species in the case of *Antedon*, are surviving remnants of the rudiments which existed in the ancestral embryos. They are so modified that, though still recognisable, they do not now any longer follow the same course of development as they did in those ancestors. Mr. Bather will remember that this explanation was given with reference to the "gill-arches" of embryonic birds and mammals (vol. ii., p. 198).

As the *Philosophical Transactions* and the *Annals and Magazine of Natural History* are practically inaccessible to a large proportion of the readers of NATURAL SCIENCE, I will again express the hope that Mr. Bather will take an early opportunity of putting the more interesting and tangible results obtained by Dr. Carpenter and himself before those readers, with such figures as may put them beyond the possibility of being misunderstood. (*Cf.* Mr. Smith Woodward's article on "The Fore-runners of the Back-boned Animals," vol. i., p. 596). I feel sure that by so doing he will facilitate the solution of our present problem.

C. HERBERT HURST.

Manchester, Sept. 10, 1893.

[We shall welcome any such concise statement of facts as is suggested by Dr. Hurst; otherwise this correspondence must now cease.—ED.]

THE MOAS OF NEW ZEALAND.

NATURAL SCIENCE has honoured me with two reviews of my paper on "The Moas of New Zealand," one in October, 1892, by Mr. R. Lydekker, the other in May, 1893, by Mr. H. O. Forbes, and by these I am gratified. I hope, however, I may be allowed a little space for a few additional remarks, because, in the first review, Mr. Lydekker has, unintentionally, I am sure, misrepresented my opinion on an important point; while in the second review there are two misstatements of facts which I ought not to leave unnoticed.

Mr. Lydekker in his criticism (NAT. SCI., vol. i., p. 594) says that as I admit that the Moas probably passed from New Zealand to Australia during the Pliocene (? Pleistocene) period without the passage of any mammalia in the opposite direction, my arguments as to the impossibility of flightless birds, as such, having reached Australia and New Zealand at an earlier period, are by no means convincing. But in my paper I say that the migration from New Zealand to Australia could not have been later than the Eocene period (*Trans. N. Z. Inst.*, vol. xxiv., p. 147); and I point out how, by an isolation of a part of the New Zealand area and its subsequent connection with Australia, this latter country could have been inoculated with Struthious birds without any possibility of mammalia passing in the opposite direction. But it would be highly improbable to suppose that on a part of Australia the Struthious birds had been isolated from the mammalia and then transferred alone to New Zealand. The two cases are quite different. No doubt a little further on in my paper I say that *if Mr. De Vis is right* in referring a femur from Darling Downs to Dinornis, *then* we should have to allow a second migration from New Zealand to Australia in the Pliocene; and this may have misled Mr. Lydekker. But it is, I think, evident that I was very doubtful about this, and now, having examined a cast of the Queensland bone, I find that it differs considerably from any genus belonging to the Dinornithidæ, and resembles more closely the femur of the Emu and Cassowary, especially that of the young Emu,[1] so that there is no longer any reason for supposing a late migration of Struthious birds between New Zealand and Australia.

Turning now to the second review. In my paper I have a footnote regretting that I had not been able to see Mr. Lydekker's "Catalogue of the Fossil Birds in the British Museum," of which "no copy has as yet been received in Christchurch" (*l.c.*, p. 98). Nevertheless Mr. Forbes says, "It is greatly to be regretted by all workers on this most difficult subject that Mr. Hutton did not defer the publication of this valuable paper, in which has been brought together almost all the known information on the Moa, till he had found time to compare his nomenclature with that of Mr. Lydekker's catalogue—a volume which had already reached the Colony before the reading of his paper" (NAT. SCI., vol. ii., p. 377).

Now the first part of my paper "On the Classification of the Moas" was read on October 1, 1891, and the second part, "On the History of the Moa," on November 4, 1891 (both dates appearing on the face of the published paper); while in the books of the Christchurch Museum I find an entry, *in Mr. Forbes's own handwriting*, that Lydekker's "Catalogue of the Fossil Birds in the British Museum" was received on November 18, 1891; and as this is the first copy that reached Christchurch, it is evident that both parts of my paper had been read, and had left my hands some time before the book was received here. As a matter of fact, I was not aware, when I read the first part of my paper, that Mr. Lydekker had been working at the same subject. I saw a notice of his book a few days afterwards, and went to the Museum to ask Mr. Forbes, who was then curator, if he had got it; and on his replying in the negative, I added the footnote to my paper. I mention this because I do not wish Mr. Lydekker to think that I treated in so cavalier a fashion his very valuable work, from which I have learnt a great deal. I could, of course, have written to Wellington and asked leave to withdraw my paper, but it did not seem to me worth while to do this, because none of the new species described by me are identical with those described by Mr. Lydekker, and the only changes I could

[1] On *Dinornis* (?) *Queenslandiæ*, by Captain F. W. Hutton, F.R.S., in *Proc. Linn. Soc. N. S. Wales* [2], vol viii. (1893), pp. 7–11.

have made were a few in the genera, owing to Mr. Lydekker having convinced me that I had put too much reliance on Sir R. Owen's plates.

The second point in Mr. Forbes's criticism to which I wish to refer relates to the age of th ebeds in which *Anomalopteryx antiqua* was found. Mr. Forbes says (Nat. Sci., vol. ii., p. 378), "I shall, however, most willingly admit that I am mistaken as to the age of the strata in which these *Dinornis* remains have been found as being other than "newer Pliocene, or even Pleistocene," so soon as the officers of the New Zealand Geological Survey—who are really the only competent referees in the case—shall have assigned to these gravels a different age." But it was the officers of the Geological Survey whom Mr. Forbes contradicted. It was Sir Julius von Haast, Provincial Geologist of Canterbury, who referred the Timaru lava stream, which overlies the gravels, to the Eocene; and it was Mr. A. McKay, Assistant Geologist to the Geological Survey of New Zealand, who said that these beds are of Miocene age. All that I did in the matter was to point out that Mr. Forbes differed from the officers of the Geological Survey without examining the evidence on which their opinions were founded.

Canterbury Museum, Christchurch, N.Z. F. W. Hutton.
25 June, 1893.

I have to thank the Editor for the perusal of the above observations of Mr. Hutton. In regard to the date of the arrival in New Zealand of Mr. Lydekker's "Catalogue of Fossil Birds in the British Museum," I beg to say that the date entered in the Museum Additions Book does not *necessarily* indicate the exact date of its receipt. As Mr. Hutton says that he came to me for the volume after the reading of the *first* part of his paper, I leave the question there. I handed him Mr. Lydekker's volume, however, immediately on its reaching my hands, *within a day or two* of his asking me for it. At all events, the point I raised remains unaltered, that as Mr. Hutton's paper could not be published for at least five or six months after it was read, and since Lydekker's classification and the descriptions of his new species were in Mr. Hutton's hands before he corrected his proofs, he ought not to have added to the confusion already existing in the classification and synonymy of the Dinornithidæ.

In regard to the second point, I quoted in my paper the latest opinion of the Director of the Geological Survey, which I have now before me, and if I recollect aright the opinion verbally expressed to me by Mr. A. McKay on the last occasion on which he visited me at the Museum.

Henry O. Forbes.

On *Anomalopteryx antiqua*, Hutton, and other New Species of Moa from Enfield, New Zealand.

In the *Transactions of the New Zealand Institute*, vol. xxv. (1892), just received, Mr. Hutton gives a further account of this species founded by him in the previous volume on fragments of a tibia embedded in two blocks of laterite found near Timaru, N.Z., and on two other fragments of a metatarsus he had seen in a photograph, which he assumes to belong to the same species. The photograph is reproduced on plate xvii. of volume xxiv. The species is founded on the tibial fragments marked *a* and *b*; and on those of the metatarsus lettered *c* and *d*. In the same volume, page 125, he states the length of the latter to be "5·5 ? inches," and of the former to be "12 inches." In the new volume he now, on plate iv., figures the two metatarsal fragments from their casts made by me, which he had previously overlooked in the Christchurch Museum. The interest of the specimens rests in their being the oldest—Newer Pliocene or Pleistocene—known portions of *Dinornis*; but the value of *Anomalopteryx antiqua* as a species, and necessarily the value of the deductions drawn from these fragments of bone, will be apparent when I state that fragment *b* of the tibia was obtained at one time (the first find) and the fragment *a* on a subsequent occasion (the third find), and that they came from different "drives" made for blasting purposes, under distant parts of the dolerite

sheet. In like manner, the fragments *c* and *d* also were found, as I personally know, in different drives far apart from each other. The chances, therefore, of these fragments all belonging to the same bird are infinitely little

Each may represent a different species, or even a different genus. Mr. Hutton has, however, boldly deduced the length of the *tibia* of his new species from the one set of fragments, and its metatarsus from the other set. On this compound structure he has built conclusions as to the early forms of *Dinornis*, and as to the pedigree of others. The author observes, on p. 16, "It will be seen that my inference that the Moa bones from Timaru belonged probably to the genus *Anomalopteryx* was a correct one."

If we now turn to his own diagnosis of the genus (tom. cit., xxiv., p. 123) we find it to be based on certain characters of the skull, sternum, and pelvis—bones which, in the present species, are all unknown; and on the metatarsus being shorter than the femur (here also undiscovered), and on its length being 2·0–2·3 times the girth of the shaft, which again is lost in both fragments. *Anomalopteryx* is further defined by its tibia being "2·1 to 2·2 times the length of the metatarsus, and its breadth between 4·2–5·1 times the girth." As already pointed out, the tibial fragments are "much hidden in the matrix," and imperfect at both ends, and Mr. Hutton now admits that "the length of the metatarsus is unknown." There is left, therefore, not a single character by which these fragments can be referred to *Anomalopteryx*, as defined by him, or to any other genus or species.

Of the additional species described in this volume as new, nearly all are based on bones exhumed at Enfield, and examined with considerable care by myself. When arranged in a series of several yards in length, the one bone merged into its neighbour so gradually that it was quite impossible to draw any line and say the bones to the right ought to belong to one species, those on the left to another. The bones associated together by Mr. Hutton to form a limb are purely guess-work, and may or may not belong to one species (not to say individual) and the comparative dimensions of its different parts can have only conjectural value. Indeed, most of the separate bones might easily be fitted to different existing species, for it is well-known that the bones of birds of ancient pedigree vary greatly in their dimensions. From descriptions such as are given by Mr. Hutton, unaccompanied by figures, it is impossible to identify the species of *Dinornis* he has established. They merely increase the already almost inextricable confusion which those who are working on this subject are compelled laboriously and unnecessarily to unravel.

HENRY O. FORBES.

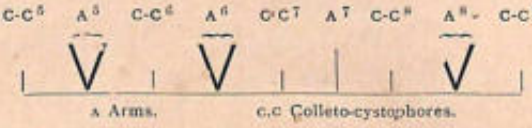
LATENT CONGENITAL VARIATION IN A LUCERNARIAN?

IT seems to be the fact that the Lucernarians examined by Mr. Hornell, and described in his note in the July number, were abnormal when gathered. On January 6 of this year I received from Messrs. Sinel & Hornell two specimens of *Haliclystus octoradiatus*, normal as to the number of arms, c-cystophores, and septa. They were put into a tank in good order, and containing a fair supply of food in the shape of small crustacea. On the 8th, no abnormality was perceived, and illness prevented my visiting the tank for some days; but when next seen, one specimen had eleven tentacular arms instead of eight. The other specimen continued normal till death.

The per-radial joining c-c^1 and c-c^5—which were slightly smaller than the others—divided the animal into an upper abnormal and a lower normal half. The latter calls for no observation. Between c-c^5 and c-c^6 were two arms well developed; between c-c^6 and c-c^7 were also two arms, but that next c-c^7 was smaller than its neighbour; there was a single arm between c-c^7 and c-c^8; and between c-c^8 and c-c^1 arm ^{8} was duplicated.

As I felt unable to deal with the problem, I offered the animal to Professor E. Ray Lankester, at whose wish it was sent to Mr. Garstang, at Plymouth, who, with regard to the last-mentioned arm, wrote to me thus:—"Arm ^{8} has now only one bunch of tentacles; but there is an abrasion on one of its edges, near c-c^8, and possibly one of the supernumerary arms has sloughed away from that spot."

Examination showed him that the septa in the upper half were four in number instead of two. The following diagram represents the abnormal half as it was when it reached Plymouth :—



Mr. Garstang was good enough to preserve the specimen, and it is now at Plymouth, so as to be available for anyone working there.

My first impression was that the supernumerary arms arose by fission of the normal ones (though it is difficult to account for the non-fission of arm 7). This was also Mr. Garstang's opinion; and though my rough notes state that the supernumerary arms were, when first observed, smaller than the others, and arose as outgrowths, the notes were made under such disadvantages that I willingly withdraw them in favour of his view.

Henry Scherren.

TO CORRESPONDENTS.

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

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ERRATUM.

Page 106. The lines under the heading "Abyssinian Sub-Region" should be transferred to the heading "South African Sub-Region" below.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 21. Vol. III. NOVEMBER, 1893.

NOTES AND COMMENTS.

Science—Educational and Professional.

At the recent opening of the new buildings for the Department of Human Anatomy at Oxford, a question of considerable interest came into prominence. As everyone knows, Science has two sides, and the two sides, except in the most elementary stages, have a very different aspect so far as study is concerned. All the natural sciences have their applied side. Geology is concerned with agriculture and with mining; Botany with agriculture, with pharmacy, forestry, and so forth; Anatomy and Physiology with medicine; Zoology with veterinary work, pisciculture, etc. As purely educational subjects and as ends in themselves leading to research and the advancement of knowledge, they are at least equally important. The one side appeals to the practical man—the follower in philosophy of Bacon; the other to the idealist, the follower of Plato. But it is a hard world, and as the practical side offers a livelihood, students are attracted to it. This rules especially in the department of Biology, as a medical career attracts so many men. What is a University to do? Its highest function certainly is the Platonic side of science, and the great endowments of the past would seem best used when directed to learning. But what are professors and laboratories without students? Cambridge effects a reconciliation by having a large medical school and by giving many fellowships to Biology men, who research and batten on the fees of medical students. Oxford has a small but growing medical school, but the few biological fellows she has had, have with all haste shaken the dust of Oxford from their feet.

Demonstrators at Oxford.

Another pressing problem at Oxford is the status of demonstrators. The habit of the University has been to consider the demonstrators as mere personal appanages of the Professors. The result of

that habit has been that almost every change in the professoriate has been followed by an alteration in the staff. As a general rule, that alteration has been such as to substitute for Oxford graduates, graduates of other universities. Even apart from this, very few demonstrators have stayed at their posts longer than a year or two. So far there are at once advantages and disadvantages in this custom. But what is an unmixed evil, and what is being agitated against at present, is the absolute want of University status possessed by demonstrators. Appointed and removed at the will of the professor, unrepresented on the Councils of the teaching staff, practically unrecognised by the University and totally unrecognised by the Colleges, the posts have nothing to compensate the meagreness of the stipends. If the leaders of Oxford University really want to stimulate Science at Oxford there is a full programme of reform before them. The planks in such a reform might well be:—

Increase of Scientific Fellowships.
Increase of Stipends of Demonstrators.
Improvement of Status of Demonstrators by
(1.) Recognition by the University.
(2.) Recognition by the Colleges.
(3.) Admission of Demonstrators, at least when these are Oxford graduates, to the Board of Faculties, and to the Museum delegacy.

In a word, demonstratorships should be University appointments: like other University appointments, they should be attached to colleges, so that by virtue of office a demonstrator should become a member of some senior common-room.

Science as She is Taught.

Being anxious to obtain some information about popular encyclopædias, we recently set our office-boy to study and report upon them. Our first selection—Saxon's "Everybody's Scrap-Book of Curious Facts," by Don Lemon—was apparently not a fortunate one, and the following extracts will show that our office-boy's report that the statements therein contained are more correctly described as "curious" than as "facts," was not without justification. We told him to commence with the Natural Sciences, and he was naturally startled by finding our respected contemporary, the *Zoologist*, made responsible for the statement that whalebone forms the substitute for teeth, and that sheep have no teeth in the upper jaw; we shall expect a libel action to result from this. The author, however, accepts himself the responsibility for making centipedes into reptiles (p. 189), and placing the teeth of the sea-urchin round the stomach (p. 300), and for revealing to geologists (p. 47) that granite "is from two to ten times as thick as the united thicknesses of all the other rocks," and that from granite all other rocks have been derived; if he stopped

here we might have been grateful for the information, but as he goes on to talk about *atoms* of lime, and that granite contains none, our faith is shaken. Of course, many fine old-crusted "Münchausenisms" are trotted out anew, such as the 5,000 year old baobabs (p. 173), and the raining of blood (p. 259); confirmation of the existence of the giants on the earth in those days is still derived from the bones of the Irish Elk (p. 260), odour is yet regarded as immaterial (p. 8), and the author seems to have no suspicion that the humming of insects is due to any other cause than the flapping of the wings. Perhaps it is hardly worth calling attention to so slight a mistake as not knowing the difference between Newfoundland and Labrador, which must have given rise to the statement on p. 128 that the interior of the former is still a *terra incognita*. Geography is not the author's strong point; but neither is mathematics, or he would not have left (pp. 302, 303), as an inscrutable problem, the "curious arithmetical puzzle" of the herring-and-a-half for three halfpence order. We are, therefore, not surprised when he tells us the atmosphere is saturated when it contains 84 per cent. out of a possible 80 per cent. of moisture; in these conditions we should think it as supersaturated as the compiler's brains. Nor ought one to grumble that, when he tries his hands on crystallography, he fails to distinguish between lapidaries and crystallographers, and in describing garnets mistakes polygons for polyhedra (pp. 131, 132).

His "facts" are not only at times inconsistent with the facts, but they are often inconsistent with one another. Thus on page 212 he tells us first that Sunken Lake is the deepest in the world, and next that Lake Baikal is "by far the deepest lake in the world." If he had only put these facts a few pages apart, the office boy would doubtless have done his best to believe them; the juxtaposition of contradictions in this case is the more surprising, as elsewhere the illustrations of the truth of a remark are often given in quite a different part of the encyclopædia: thus he heads one paragraph "statistics are funny" on p. 55, while his calculation as to the population of ancient Rome which proves it, is delayed till p. 80. In spite of all the vast range of the author's knowledge he is very modest and loves simplicity: thus his formulæ on p. 44 for determining ships' tonnage ought to charm the heart of Lloyd's with their exquisite ease and wide applicability; while his confession on p. 19 that he will never be able to understand the principle of the steam ejector shows that he does not rate his own powers very highly; but when he sums up "what's a flame?" in four lines beginning with "Combustion is in some way produced by the union of carbon and hydrogen with oxygen," and ends with "the best philosopher can tell little more," we cannot but excuse our office-boy's comment of "More things in heaven and earth can be explained than by your philosophy, Don Lemon." The author's use of the word "fact" is certainly strangely comprehensive, for we do not quite see how advice as to the selection of a wife (however sound in itself) can be

regarded as such; but we cannot grumble at this as, if it were excluded, so might have been the remarks of Professor Blackie, which ought, in future editions, to be rescued from the obscurity of p. 51, and printed in large type on the title page:—"Never force yourself to learn what you have no talent for, . . Be content to be ignorant of many things that you may know one thing well." This collection of curiosities has, however, we understand, an enormous sale, and its statements are doubtless accepted as "facts," whereby we are reminded of Carlyle's estimate of the population of the British Islands, "30,000,000 mostly fools!"

A Recent Submergence of Western Europe.

Professor Prestwich has communicated to the Royal Society a paper "On the Evidences of a Submergence of Western Europe and of the Mediterranean Coasts at the close of the Glacial or so-called post-Glacial Period, and immediately preceding the Neolithic or Recent Period." (*Proceedings*, vol. liii., pp. 80–89.)

He points out that certain superficial deposits, containing organic remains of a land-surface, and composed always of local materials, are found to radiate from independent centres and to show no signs of glacial action. They are grouped by the author under the general name of "Rubble Drift." This assumes various forms.

As "*Head*" it overlies raised beaches by the Channel, in France, Jersey and Guernsey, and on the North African Coast eastward to Tunis.

As *Osseous Breccia* it covers the lower slopes of the rock of Gibraltar, extends round the borders of the hills encircling Palermo's plain, and is found beneath the high escarped rocks of Malta and in Greece.

In *Ossiferous Fissures* it occurs on isolated hills along the Riviera, in Southern France, at Gibraltar and in Algeria.

As *Loess*, apart from that which occurs within valleys and is due to river floods, it is found on dividing watersheds and the high plains of West and Central Europe.

Differing though this Rubble Drift does in detail, the author considers it to be due to a common cause, and only explicable upon the hypothesis of a submergence, which was at once widespread and short in duration. It allowed no time for marine sedimentation, while the establishment of a marine fauna was prevented by the turbid character of its waters. The resulting deposits come from the wreck of the land-surface only. This submergence was followed by an upheaval, since which the configuration of the land has remained comparatively unaltered.

The ossiferous fissures, found often on isolated hills, in France, at Gibraltar, and elsewhere, contain the remains of carnivores, ungulates, and ruminants. After showing that these remains could not have been collected by beasts of prey, the author points out that

only a great and a common danger could have driven together such variously-natured animals. Such danger would be found in advancing waters, driving the animals to take refuge on isolated hills, or in other circumscribed areas, there to meet their deaths by drowning. It is noticed that while certain animals did not survive the rubble drift in the west, they existed to historic times eastwards in Egypt—of the submergence of which country there is as yet no evidence.

When the upheaval followed, portions of the surface *débris* were swept down, detached bones were carried away, other bones were crushed and splintered by rolling fragments of rock; and the drift deposits were dispersed from various centres by the diverging currents that resulted from the gradual elevation of the sea-bottom.

Nuclear Division in Branchipus and Apus.

Mr. J. E. S. Moore, in the *Quarterly Journal of Microscopical Science* for September (vol. 35, pp. 259–283), has published some results of great interest. In the male gland of Branchipus, the first stage in spermatogenesis consists of the enlargement of the cells lining the tube, and the arrangement of their nuclei into a spirem with the chromatic elements all on one side. During this time, a large number of bodies like centrosomes (pseudosomes), appears in the protoplasm at the cell-basis. The spirem breaks up into ten dumb-bell shaped chromosomes, and these arrange themselves in an equatorial plate. But in this process the chief interest is that the observer regards it in the light of Bütschli's froth theory of protoplasm. At first the whole nucleus appears to be made up of drops of a clear fluid, enclosed in films of staining material. These drops gradually run into each other, as the drops of a soap foam grow larger by smaller bubbles breaking into the larger. As this process goes on the strands or meshes of staining material become thicker and thicker, and ultimately break up into the chromosomes. As this running together of the nuclear bubbles progresses it breaks beyond the nuclear limits, and bursts into the cytoplasm. The chromosomes thus are left hanging in a clear plasm, supported by a few strands of the cytoplasm which have not yet been broken through. These irregular strands become reduced to fine threads, and the threads terminate in the pseudosomes. As a result of the tractions of the films of the bursting bubbles, the threads with the chromosomes become pulled into a spindle of which the poles are those pseudosomes which have "the best foothold," on the cell periphery. The other pseudosomes and their threads converge on these, and the result is a more or less regular spindle. The actual centrosomes are a fusion of some of these pseudosomes, and are thus in origin equivalent to the angular spaces in the network exterior to the nucleus. A progressive fusion of the bubbles in the cytoplasm sweeps together the scanty stainable material of the cytoplasm into

twenty bodies called *dictyosomes*, just as the fusion of the bubbles in the nucleus swept together the nuclear chromatics into the chromosomes. Thus pseudosomes, centrosomes, and dictyosomes, as well as chromosomes themselves, are all expressions of the froth-like activity of protoplasm.

It is of much interest to find an observer of actual miscroscopic details in karyokinesis connecting the actual observed processes with, on the one hand, Professor Bütschli's Foam Theory of protoplasm, and, on the other, Professor Weismann's idants and reducing divisions. It is truly bringing the phenomena of heredity into close relation with physical phenomena.

The Origin of Dicotyledons.

Botanists as well as geologists will welcome Professor Lesquereux's posthumous monograph on the Flora of the Dakota Group (*Monographs of the United States Geological Survey*, vol. xvii.). Few facts are so puzzling to the evolutionist as the sudden appearance in profusion in Middle Cretaceous times of highly-organised dicotyledons, often belonging to existing genera. The older strata, even the Wealden, give no hint of the existence of any plants of such high type; and yet the Cenomanian deposits of Dakota have already yielded no fewer than 429 species of dicotyledons, without including any herbaceous species, for little has been preserved except leaves of deciduous trees. When the discovery in Dakota of these remarkable deposits was first announced, there was a natural hesitation to accept their Cretaceous age; but, since the stratigraphical evidence has been made clear, botanists have been awaiting the appearance of figures and descriptions, to show to what extent the reference of so many of the plants to still living genera is justifiable. Now that this handsome monograph has been issued, we are better able to judge, and one cannot help being struck with the close resemblance of many of the leaves to those of existing plants; yet, on the other hand, one observes that nearly all of the species are described from leaves alone, and that the few detached fruit yet found have nearly all to be left in the indefinite genus *Carpites*, for they do not seem to be closely allied to recent genera. According to Professor Lesquereux's analysis, the flora of the Dakota group is composed of 460 species, of which 6 are ferns, 12 cycads, 15 conifers, 8 monocotyledons, and 429 dicotyledons. Such a proportion of dicotyledons is far more suggestive of a recent flora than of one of Cretaceous age, and it is singular how few characteristic Secondary genera have been obtained. Professor Lesquereux's suggestion, that we are enabled to refer the origin of the dicotyledonous plants to the beginning of the Cretaceous period, no doubt accords with the facts, as far as they are yet known; but both botanists and geologists will hesitate to adopt it, in face of the highly differentiated dicotyledons existing in the Dakota group.

A Remarkable Orchid.

Mr. F. W. Moore, the enthusiastic Curator of the Glasnevin Gardens, Dublin, has, according to the *Orchid Review*, recently flowered, for the first time in Europe, a plant of *Coryanthes Wolfii*. This remarkable genus attracted the attention of Charles Darwin, who gives a figure of the flower, and describes its strange method of fertilisation, in his work on the Fertilisation of Orchids. It will be remembered that drops of liquid are excreted and caught in a bucket-shaped development of the lip, that bees struggling for a place on the edges of the bucket get pushed in, and their wings being wetted by the liquid, have no means of escape except by a narrow passage which is arched over and temporarily closed by the anther and stigmatic surface. The present species comes from Ecuador, and was named by Mr. Lehmann, the German Consul in the United States of Colombia, well-known as an energetic botanist and orchidologist. He says, writing in the *Gardeners' Chronicle*—"It grows very sparingly, mostly on cacao trees, all over the littoral districts of the Guayas, where it flowers in February and March, when these level lands are mostly inundated. During this season it is beyond the power of man to penetrate the woods there—a circumstance that accounts for the plant not having been seen before. It produces thick upright flower-spikes, 40 to 50 cm. high, with three to six large, wonderfully-constructed flowers, which are yellow, mottled, and stained with brownish red. There are but few plants in the entire vegetable kingdom which are more interesting, and which afford such a varied amount of material for the student of vegetable physiology." The plant has a strong attraction for ants, numbers of which surround its root masses, and are apparently essential to its well-being, as in their absence it seems to do badly. This myrmecophily was observed not only in the tropical South American forests, but under cultivation. The ant is a small species of *Myrmica*, with a strong aromatic smell, and a bite so severe that it requires some courage to meddle with the plant.

Names of Orchids.

In the same journal, which, by the way, contains much to interest the orchid-lover, are descriptions by Mr. Rolfe of new species of *Masdevallia*, *Laelia*, and *Maxillaria* which have flowered under cultivation, and also a note of warning to those who are apt to trifle with nomenclature. *Cypripedium spectabile* is one of the best known and most beautiful of Cypripediums, but it has no right to this name, under which Salisbury described it in 1791, as in so doing he ignored two earlier names, *C. album* of Aiton (1789), which seems to have been discarded as inapplicable because the lip is rose-coloured, and *C. reginæ* under which it was described by Walter in his "Flora Caroliniana" in 1788. *Cypripedium reginæ*, as we must therefore call it, and none will deny the suitability of the name, has a remarkable

distribution. Hitherto known only as a native of the peat bogs of Canada and Eastern North America, it has recently been discovered in Western China, on the borders of Tibet, together with the remarkable little *C. arietinum*, also a native of the same parts of North America.

As a genus, *Cypripedium* has fared somewhat badly with the people who discuss nomenclature. In his review of Mr. Jackson's monumental "Index," the editor of the *Journal of Botany* refers to the omission of Ascherson's emendation of the Linnean name, to wit *Cypripedilum*, launched in the Flora of Brandenburg in 1864 to accord with the etymology (πεδίλον, a slipper), since *Cypripedium*, as the German doctor observes, "ist nicht zu erklaren." It is to be regretted that Pfitzer should have taken up Ascherson's name in Engler and Prantl's "Pflanzenfamilien" and elsewhere.

The practice of amending names the etymology of which is not clear to the emendator's mind is a reprehensible one, and apt to lead to still greater confusions. Thus, to correspond with Ascherson's alteration, Pfitzer writes *Selenipedilum* for *Selenipedum* of Reichenbach *fil.*, and also establishes a third genus—*Paphiopedilum*. The new genus must stand, but the other two return to their original form, and so the symmetry is destroyed. Another instance of the same intermeddling is supplied by Lestiboudois' *Heleocharis* for Robert Brown's *Eleocharis*, to explain its derivation from ἕλος a marsh, and dispel any idea of pity (ἔλεος) or a kitchen table (ἑλεός).

American Botanists in Congress.

The September number of the *Botanical Gazette* is filled with reports of meetings. The American Association for the Advancement of Science met at Madison, Wis., on Thursday, August 17, and sat till the following Tuesday. Professor C. E. Bessey presided over the botanical section, and the *Gazette* prints an abstract of his address, as well as an outline of the papers and discussions. In his address, entitled "Evolution and Classification," the Professor upbraids systematists for their unscientific conservatism in retaining the crude system of Jussieu and De Candolle for the arrangement of flowering plants for more than thirty years after the general acceptance of the doctrine of evolution. Evolution has taught us what relationship means, and from the new point of view a natural classification is not merely an orderly arrangement of similar organisms, but an expression of genetic relationship, in which primitive forms will precede those derived, and the relationship of the latter be positively indicated. Just as Carl Linné's artificial system was in general use among botanists long after the construction of a natural system by Jussieu, so in turn this natural system has persisted by the help of conservatism and reverence, till ceasing to be natural it has become a makeshift, and "is now as much a clog and hindrance to the systematic botany of the higher plants, as was that of Linné sixty years ago."

There is doubtless a great deal of truth in these remarks, but what has Professor Bessey to offer us in place of our present system? It was admitted from the first that the Apetalæ formed an artificial group; a residue, in fact, of families whose places in a natural system could only be determined by further study. This study is now progressing. Treub has shown, by his work on *Casuarina*, that one family at any rate has affinities with groups other than the Polypetalæ among which Professor Bessey would apparently distribute all the apetalous orders. Quite recent work on the Cupuliferæ has led to somewhat similar results, and we cannot but think that until other families of doubtful affinity have been subjected to the same close investigation, it will, on the whole, be better to keep them apart. Of course in the case of those forming the series of Curvembryeæ, and others, relationships are more apparent, and there is no objection to ranking them with their polypetalous allies; but until a more truly natural system has been satisfactorily elaborated, systematists will not be willing to drag to pieces their herbaria, repeating the process every six months to suit the peregrinations of a few orders of doubtful affinity.

Following the botanical proceedings of the Association is an account of the proceedings of the Madison Meeting of the Botanical Club connected with the Association, in which the check list ordered to be prepared by the committee on nomenclature occupied the chief place in the discussions. The Botanical Club met on four occasions during the session of the Association. Finally, we have the proceedings of the Madison Botanical Congress, which occupied two days immediately after the close of the two former functions. The desired international character of the assembly was not realised, the attendance of European botanists falling much below the expectations of the organising committee, and it was therefore decided that questions of nomenclature should not be discussed. Professor Greene was elected President, and M. Henry de Vilmorin, of Paris, one of the Vice-Presidents.

In the September number of the *Journal of Botany*, Mr. Britten makes some amends for the omission of Gilbert White's name from the "Index of British and Irish Botanists," by conclusively demonstrating his claims to be included. A copy of Hudson's "Flora Anglica" has recently come to light with White's autograph on the fly-leaf, and a note, also in his hand, that all plants occurring within the parish of Selborne are marked by a cross. Several corrections and a few MS. notes show that he used the book a great deal. Moreover, Mr. Bell, in his edition of Selborne, states that he had a catalogue of Selborne plants in White's handwriting. At the end of his note Mr. Britten prints a list of the 439 species indicated by the Selborne naturalist.

In our last number we briefly drew attention to the stirring address by Mr. Teall on Uniformitarianism: an antidote has already been provided.

In an article on "The Position of Geology" (*Nineteenth Century*, October), Professor Prestwich offers a vigorous protest against "the dwarfing influence of Uniformitarianism." He admits that the forces acting upon the surface of the globe in past times have remained the same *in kind*; but he strongly opposes the notion that they have remained the same *in degree*. "What (he asks) if it were suggested that the brick-built Pyramid of Hawâra had been laid brick by brick by a single workman? Given time, this would not be beyond the bounds of possibility. But Nature, like the Pharaohs, had greater forces at her command to do the work better and more expeditiously than is admitted by Uniformitarians." Professor Prestwich complains, and we think rightly, that many arguments with regard to geological time and the rate of denudation are based on very limited evidence; they furnish standards applicable to present changes, but they give no measure of the amount and rate of work that could be done. We must interpret phenomena by the light of the facts themselves. Those who claim vast periods of time for the Glacial and post-Glacial periods, give no explanation why such animals as the Reindeer, the Musk-ox, and the Glutton survive unchanged. He concludes that Uniformitarian measures of time "have probably done more to impede the exercise of free inquiry and discussion than any of the catastrophic theories which formerly prevailed"; they "hedge us in by dogmas which forbid any interpretation of the phenomena other than that of fixed rules which are more worthy of the sixteenth than of the nineteenth century."

"The Work of the Geological Survey" forms the title of a paper read, by Sir Archibald Geikie, before the Federated Institution of Mining Engineers (*Transactions*, vol. v., p. 142). After a short sketch of some early geological maps, the writer gives an account of the method of mapping, and of the various kinds of map issued by the Geological Survey. Other branches of the Survey work are dealt with, and it is stated in conclusion that "From the beginning of its existence the Survey has been continually referred to by all branches of the Government Service for information regarding questions in which a knowledge of geology is required. The sinking of wells, the choice of sites for forts and Government buildings, the placing of graveyards, the selection of materials for buildings or roads, the nature of soils and subsoils, with reference to matters of drainage—these and many other subjects have been reported on. Nor has the general public been backward in application for similar information. The offices of the Survey are always open, and every assistance which can be rendered to enquirers is placed freely at their service."

An important memoir on the Jurassic rocks of the Southern Jura by Dr. Attale Riche has lately been published (*Annales de l'Université de Lyon*, vol. vi., 1893). The beds described are the Bajocian, Bathonian, and Callovian, which the author includes as "Jurassique Inférieur." In this respect he departs from the ordinary grouping, which places the Lias in the Lower Jurassic. The local details furnished by the author are valuable; while his Comparative Table of formations, which places beds with *Ammonites parkinsoni* on a level with those yielding *A. arbustigerus*, is calculated to yield matter for discussion. In this way *A. parkinsoni* comes in both Bajocian and Bathonian formations. He ends the Lias with the zone of *A. opalinus*, and includes the representatives of our Cornbrash in the Callovian. The work shows the difficulties that attend all attempts at very minute correlation of strata in different areas.

A short paper just published by Mr. W. H. Dall is likely to be overlooked as of purely palæontological interest unless his last paragraph is read. Mr. Dall, writing on a subtropical Miocene fauna in arctic Siberia (*Proceedings of the United States National Museum*, vol. xvi.), remarks that "it is perhaps very late in the day to refer to the hypothesis which explained the warm water Old Miocene of the North Atlantic shores by assuming a shifting of the polar axis, so that the pole at that time would have been situated somewhere in central Siberia. That hypothesis has few, if any, friends at the present time; but it may not be amiss to point out that, if it were necessary to put a quietus on that moribund speculation, the presence of a warm water Old Miocene in eastern Siberia, such as our present fossils indicate, would be quite sufficient to prove that no polar conditions in the modern sense could have existed there during that epoch of geological time."

Subtropical, or at least warm-temperate, Miocene plants have already been obtained in Greenland and Spitzbergen, and the North Pole was evidently completely encircled by a warm belt such as no shifting of the axis will account for.

Authors should, if possible, give a clue in the titles of their papers to the subjects dealt with. In the *Geological Magazine* for October, we find the first part of a paper on "Some Cretaceous Pycnodont Fishes," by Mr. A. Smith Woodward. Imagine our surprise when we discovered in it accounts of Kimmeridgian, Portlandian, and Purbeckian Fishes! Some Neocomian and Cretaceous forms are also described.

I.

Geology in Secondary Education.[1]

THE need for a selection of subjects in modern education becomes pressingly apparent, and there is consequently every danger of specialisation at too early an age. The result of this will be that men and women will grow up as students of some branch of natural science, literature, or mathematics, with even less in common than is the case between the several groups of educated persons at the present day. While every thorough worker, knowing the influence of his own favourite study upon himself, is apt to put forward the claims of that study as the real essential in educational progress, it is with all seriousness that I assert that, in general secondary education, Geology should receive a recognised position.

Elementary Chemistry and Physics may be accepted as being now taught in all self-respecting institutions, such as the great day-schools, the endowed boarding-schools—which often usurp the title of "public schools"—and similar progressive seats of learning. In the following suggestions, everything that is put forward is intended to apply equally to girls as well as to boys, the time having long passed when it could be maintained that knowledge which enlarged the views of the one sex would unfit the other for the affairs of life. This paper is also aimed somewhat at the future, when secondary education will be brought within the reach of all classes, perhaps by a wider system of scholarships open to pupils of the board-schools.

Given a fair grounding in the facts of elementary chemistry and physics, the application of these facts to the great natural world on which we depend leads us directly to geology. The common minerals, the common rocks, their common modes of association, are capable of direct observation, and offer material for consideration wherever the future lot of the student may be cast. I am now taking the case of the ordinary individual, whose leisure-time is becoming in general increased. The correct understanding of one's surroundings is capable of adding a new pleasure to existence, and of providing a rational occupation—which is one of the great ends of education.

The amount of knowledge that the average schoolboy possesses about the Punic Wars, or the externals of Greek mythology, is some-

[1] An introduction to a discussion at the meeting of the British Association at Nottingham, September, 1893.

thing awe-inspiring, at the least; yet he comes up to a University in ignorance of what has made the features of the landscape which he has seen all his life from the door of his country home, and of what agents have brought together the pebbles of his summer bathing place.

If the utility of such knowledge be questioned, we open up an attack upon the vast body of information crammed into unsuspecting childhood. The only answer is to admit the utility of all knowledge whatsoever, and then to assert the special desirability of that branch under consideration.

At the present moment much attention is given to technical education, which has two main objects—the improvement of the workman and the improvement of the object produced; but there are many who look to such education primarily as a means of acquiring wealth—and also of competing with the Germans. In this paper I regard education as a means of acquiring happiness and of living peaceably with one's neighbours; so that the financial return may be disregarded until special cases are considered in the discussion. National secondary education, I take it, should insist on the dependence of the individual on his fellows rather than on the dependence of the masses on an individual; in other words, it should enable a man to realise his position as a member of a community, to live without grasping, and happily upon a moderate income.

Now Geology becomes indispensable for a correct appreciation of our relations to our surroundings and to the past. In a general course of instruction during, say, the last year of secondary education, when the pupils are of the age of 16 or 17, I would treat of the different minerals of which rocks are made, always bearing in mind the materials to be found in the locality in which the teaching is being given. I would deal as little as possible with the more refined aspects of these minerals—such as their crystalline systems—beyond an indication of the facts of symmetry and of how minerals, apparently similar, may be ultimately distinguished by their crystalline forms. I would lay much stress on the character of hardness, using the thumb-nail and the pocket-knife, and on simple chemical means of determination. It may be worth noting that I have always found girls most apt in matters requiring the elements of crystallography, while boys, as a rule, are indifferent to symmetrical beauty.

In dealing with rocks, I should exclude the use of microscopic sections. For our broad untechnical purposes, it would seem better to grind down a surface when structure has to be exhibited, and to pick a rock to pieces, with the aid of a hand-lens or a dissecting microscope, when its constituents have to be determined. The methods used in more elaborate work might, however, be hinted at; but in every week's lesson the teacher will have to remember that he is training men and women, and not miners or geologists.

The mineral analysis of some ten common rocks may thus be

discussed; the teacher will do well to avoid debatable terms, or those dependent on purely microscopic examination. Every encouragement should be given to the idea that rock-specimens are parts of the great masses round us, and that Geology is a study of the open air, natural in the highest sense, and not an exercise in more or less classical terminology.

The modes of origin of these rocks may be described side by side with their mineral characters, an amount of physical geology being thus introduced to enliven the bald statement of their constitution. Thus an account of modern marine shell-banks may precede the discussion of limestone, or may proceed naturally from the observation of fossils in a rock; while an account of the main features of an active volcano will add vast interest, and even dignity, to the details of a lump of basalt.

The structures of mountains, the carving of valleys in high plateaux, the rounding of pebbles, and the sifting of materials in the streams, can all be illustrated by local examples, or by reference to the customary excursion-resorts of the people. London and the south-east of England form a district less favoured than most parts in the matter of variety of structure; yet the broad facts of physical geology can even there be dealt with practically.

But when the proofs of what were formerly called "revolutions" in the surface of the globe have been made plain, and the slow and gradual nature of those changes has been impressed upon the student in the field, the review of the past history of life becomes the main object of his study.

For the public at large, this portion of the subject has, to my thinking, the greatest educational value; I urge, indeed, the fundamental importance of geology on account of the desirability of giving everyone, from the peer to the proletariat, an outline of the history of life upon the globe.

It is impossible to disguise the fact that the teaching of human history is liable to be both partial and partisan; and that, under existing systems of examination, a pupil may be well read in the events of one century of the history of his own people, and yet be entirely ignorant of the causes that have moulded the popular opinions of his own time. Firstly, then, Geology introduces the student to a history in which one is not called on to take sides, a history the compilation of which is the outcome of enquiry rather than enthusiasm—a remark that holds good, at any rate, until we reach the Glacial Period. The pupil learns, then, the beauty of facts, and that what we call truth is a conscientious and necessarily imperfect deduction from them. The very imperfection of the record will cause him to reason for himself, and to recognise fully the difficulties of the process. Thus, perhaps, his judgment of things in general will become less personal, less harsh, and less redolent of the dogmatism of a "school."

But any natural science will similarly benefit and broaden our egotistical souls; the peculiar claim of Geology is its direct bearing upon history. To the average schoolboy the Greeks and Romans divide the honours of antiquity; the classical models of heroic virtue are so marked out and emphasised that it is often doubted, in serious works and popular lectures, whether humanity has altered for the better since the days of Horatius Cocles. The gloomy remark that human nature will always be human nature is continually regarded as an argument in favour of repressing the species, rather than of providing loopholes for its development. To present the ordinary man or woman with a picture of the Palæolithic epoch, and at the same time with a history of life in which, on any reasonable scale, the whole of recorded human history, including the Chinese, is too short to be taken as a unit—this surely is calculated to give one room for hope, and to shake one's faith in the immutability of human nature.

One more point of view. The enormous past, which we cannot deny to have been progressive, tends to give a certain solidarity to our conception of the human race. Man becomes a prominent feature along a particular and limited horizon of the earth's history, just as the Romans did in the recent history of Europe. The race acquires an importance to the geologist, standing, as it does, at the end of so vast a series of life-changes; and he who learns to respect the race may in time become less callous about the extinction of an individual. I do not mean to assert that a diffused knowledge of, let us say, the Eocene Period would have prevented the hecatombs of Waterloo or Weissenburg; but it would have gone far to make such tragedies more generally regretted, and more generally hateful. The stratigraphical position of man, when it is once realised, makes him appreciate both his present possibilities and his dangers. If Geology helps him to recognise the differences between groups of men, as, at the most, specific rather than generic, the study will have done much to promote the harmony of nations.

I would ask, then, those interested in Geology to watch the progress of public education, and to endeavour to secure a place for this science side by side with the historic and human studies of later years of the curriculum. Personally, I have found it most interesting to note the intrusion of completely novel ideas—in my own case in Colleges for women—when such a subject is superposed on the ordinary courses for an Arts degree. Although the foundation of Chemistry and Physics is still in many cases slight or absent, the results, as far as I can observe them, are full of encouragement and hope.

GRENVILLE A. J. COLE.

II.

Natural Science at the Chicago Exhibition.

SCIENCE of any kind is lamentably absent at the World's Fair. In this century, which we delight to patronise as "The Age of Science," and in a country where the educational method of exhibition has been brought to such perfection as it has in the larger American Museums, one would have expected something more than a heterogeneous assemblage of exhibits, some good, many bad, and most indifferent, which, in the expressive language of the day, are planked down anywhere. Organisation and arrangement must have expended all their strength on the outward appearance of the Fair, which is indeed admirable, and have had none left to tackle the far more important problem of displaying the exhibits in the most instructive manner. In this respect the only satisfactory building is that belonging to the Government of the United States; and had the same intelligence which directed that, been, as was originally intended, brought to bear on the rest of the Exposition, the result would have been as gratifying to the earnest student and the scientific visitor as it now is to the pleasure-loying and wonder-seeking public of the World.

Our concern, however, is more particularly with those branches of Science known as Natural, and it may prove instructive to consider how such are represented at Chicago. The exhibits of this kind may be divided into the Natural Science exhibits and the Natural History or semi-scientific exhibits: the latter largely preponderate. As no one visitor can hope to discover everything of interest to him that may be hidden in the nooks and corners of the White City, we have not scrupled to draw when necessary on an excellent article that appeared above the well-known initials of W. H. D., in *The Nation* for September 14, 1893; quotations therefrom are distinguished by inverted commas.

"In ZOOLOGY the Fair offers a rather meagre display, which is made still more obscure by being broken up into small exhibits, many of which are found in most unexpected places." Most of them, however, are, or are supposed to be, contained in the Anthropological Building, and among these the most representative general exhibit is that of Professor Ward of Rochester. "Here a very excellent series of specimens suitable for a teaching museum are brought together, well and clearly labelled, and attractively arranged. The model of the

mammoth here attracts much attention, and the taxidermy throughout is good, if not remarkable." Among the Invertebrata of Professor Ward's collection, the Corals and Echinoderms, notably the dried and beautifully-mounted specimens of *Pentacrinus*, are worthy of attention: for exhibition purposes these latter are certainly superior to spirit-specimens, as they can be clearly seen. In the East Gallery of this building are the faunal exhibits of Maine, Ohio, Colorado, and Ontario; also "a number of specimens of taxidermy exhibited by private persons, none of which needs special comment; the general average is poor and inartistic. The furriers in the Liberal Arts Buildings have here and there a well-mounted fur animal; a few may be found in the Leather Building; and small collections representing State faunas may be found in most of the buildings erected by the several States or foreign countries. Most of these are poorly mounted and very imperfectly labelled. Occasionally a rare creature may be detected among these forlorn representatives of their kind, as in the case of the Liberian hippopotamus, a mounted specimen of which is included in the exhibit of that little African State. The exhibit made by the State Museum of New York is creditable, clean and well labelled, one of the best of the minor collections." The collection of stuffed animals shown by British Guiana is also worthy of special mention.

It is, as above indicated, in the Government Museum that the most scientific exhibits are to be looked for. Here "the National Museum exhibits a mounted series showing examples of all the families and most of the genera of American mammals and a number of groups of North American mammals, accompanied by accessories indicating the natural surroundings of the species. Among these the most interesting are those containing the Rocky Mountain sheep and goats, the woodland and barren-ground caribou, and the Pacific sea-lions. A fine walrus might have been more life-like 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 most important among the exhibits in this series is the mounted skin of a very good example of the Alaskan sea-otter, perhaps the best existing specimen in any museum. A number of large African game animals are also shown, some of which are rare. Similar series, illustrating the American families of birds, reptiles, and batrachians, fishes, insects, etc., are also exhibited by the Museum, as well as two very attractive collections of the birds of Paradise and humming-birds. Several groups of species, formerly common but apparently now verging on extinction, comprise the Carolina paroquet, the wild pigeon, and the ivory-billed woodpecker. There are also spirited groups representing the courtship of the prairie chickens, and the flamingo with its singular nests." We, in England, are familiar with such natural groups of birds, and excellent examples of this sort of

mounting may be seen at the American Museum of Natural History, New York; "but the restricted space and the small amount of money available at Washington have hitherto prevented the National Museum from extending to birds the treatment which has been so successfully applied to the larger mammals." The popularity of this Smithsonian exhibit may be gauged by the difficulty that a visitor experiences in forcing his way through the almost immovable crowd. One portion, however, he will have almost to himself; yet it is by no means the least interesting. This is the admirable Synoptic Series of Invertebrata, arranged by Mr. Lucas, where every help is afforded by means of well-expressed labels. Adjoining is the no less instructive series illustrating the osteology of the vertebrata. This shows the homologies of the principal bones by means of compared articulated and disarticulated skeletons of the chief vertebrate types, the bones of the cranium being distinguished by colours.

Those who visited either the Fisheries Exhibition at South Kensington, or the similar division of the Philadelphia Exposition, will learn little new at Chicago. The Fisheries Building is truly, as it has been called, "a poem in architecture"; but, except for the fisheries of N. America, the exhibition is small and mostly unimportant. "The reason," writes Mr. Dall, "is not far to seek. The United States and Canada collectively have nothing to learn at present from foreign countries, and no foreign dealers in fish products have any reason for supposing that they can gain a foothold in our markets, except for sundry specialties like cod-liver oil. Consequently the commercial incentive is lacking, and, apart from such countries as New South Wales and Japan, which have made an exhibit as a matter of national pride and in evidence of the state of their industries, the foreign fisheries are very imperfectly represented." The exhibit of the Fish Commission in the Government Building is good, "though less prominent than in 1876, and the practical rather than the scientific side of its work is emphasised. Similarly, the collection shown by Japan is more interesting from the economic or anthropological side than as illustrating the ichthyology of the empire." Pounded shark and dried cuttle-fish do not strike the Western mind as particularly appetising; but with cockroach catsup they are by no means bad. One doubts, however, whether the enterprising Japanese firm who are trying to introduce dried sea-weed as a relish will meet with the success that they deserve. Some of the Japanese methods of fishing are so curious that they might have been illustrated at Chicago, but, as they are not, one cannot now describe them. "There are a few interesting models of fish traps and weirs in the collection shown by the Sultan of Johore in the Plaisance, but these objects belong under the head of Anthropology rather than that of Ichthyology."

Among the more obviously attractive exhibits in the Fisheries Building are the skeleton of a Pacific Humpback Whale, 47 ft. 6 in.

long, and 48 ft. in girth, the tent used by Lieut. Peary on his present Arctic expedition, and the Aquaria. In these latter the foreigner will probably be most attracted by the fresh-water fish of America; while, as Mr. Dall remarks, the visitor from the Western States will learn much about the salt-water fish so unfamiliar to him. Certainly, it was round the sea tanks that crowds congregated. The fresh-water tanks have, unfortunately, suffered from virulent attacks of *Saprolegnia*, the fish fungus. "This exhibit, as a whole, is that of tanks of water with fish in them, and not aquaria in the strict sense of the word, which implies a balance of conditions between the water plants and the animals, as in a state of nature."

"Many of the States show more or less dilapidated stuffed fishes as a part of their faunal exhibit, but California has an excellent series of models, by Denton's method, of her chief economic species. These are well coloured and properly labelled. New South Wales has a good series of alcoholic fishes, in the same excellent order as the rest of her exhibits," as well as a striking set of coloured drawings.

"The shell-fisheries are almost unrepresented in the Exposition except by the canned product. A few unlabelled and poorly-preserved shells appear in the Brazilian exhibit, a few oysters and pearl shells in the collections of New South Wales and Mexico; Wisconsin shows a pretty series of fresh-water pearls in (of all places) the Mining Building; and Tiffany has a fine series of pearls in their natural state, beside those worked into ornaments—but this is about all that is visible in this line, excepting the series of shells contained in the Ward exhibit in the Anthropological Building."

In the Government Building the Bureau of Animal Industry has an admirable exhibit, which shows how well science may be combined with practice. Part shows the distribution of animal life according to elevation, and part animals which are either beneficial or injurious to agriculture. Most noticeable here is the exhibit of the Entomological Bureau. "Probably no portion of the whole Exposition better illustrates the scientific modern method of presenting such matters to the public eye. The insect is shown fully labelled in all its stages and varieties, with its food plant and cocoons, with illustrations of its ravages when injurious, or beneficial methods of work when useful, supplemented by a small map showing its geographical range, and often, when of economic importance, accompanied by enlarged anatomical models. Nothing more clear, instructive, and satisfactory can be imagined. The experiment stations of the several States in the Agricultural Building show a collective exhibit which is very creditable, though less efficiently displayed than that made by the Government; and there are the usual miscellaneous collections of attractive butterflies, etc., to be found in many of the State exhibits and in those shown by foreign Governments."

The exhibits and collections hitherto alluded to fall, for the most part, under the head of semi-scientific. Those of deeper scientific

interest are chiefly to be found among the Educational Exhibits in the Manufactures and Liberal Arts Building. Here such institutions as Harvard, Johns Hopkins, Princeton, Columbia College, and the University of Pennsylvania illustrate the methods of their science-teaching and of the original research carried on under their auspices. To a scientific man this is the most interesting part of the whole Exhibition; for we are all of us students, and a few of us are teachers. Still one must regret that so small a part of the Exhibition is on these lines, and that foreign countries are so little represented. Japan, indeed, gives evidence of her new birth in this direction as well as in others; but where are the Laboratories and Museums of our old Universities? where is the Royal College of Science? where are Bonn, Heidelberg, Freiburg, Liége, Paris, Upsala, Dorpat, and Bologna? Surely the heirs of Galileo need not shun the land of Columbus. What an exhibition rises before the scientific imagination at the mere mention of these few names! But, alas! men of science are too poor even to advertise themselves, so we must make the best of Chicago, and that which the American Universities have been good enough to do for us.

From these exhibits each visitor will carry away thoughts in accordance with his own needs. The following are what appear in my note-book. A giant microtome, used chiefly for cutting microscope sections through the entire brain, exhibited by the University of Pennsylvania. The object to be transected is fixed at the end of a very heavy lever and is allowed to sink down upon the edge of a broad knife, the blade of which is parallel to the side of the lever. The section as it comes off is caught on a sloping sheet of paper. The Biological Laboratory of this University makes a regular practice of selling photographs of preparations, specimens, and drawings; and a catalogue of such, which might prove useful to lecturers, may be obtained from the Secretary. Lecturers will also be interested in some models of brains exhibited by the Physiological Department of the Loomis Laboratory of the University of New York City. These models are first of all constructed in clay; the clay is then covered over with strips of newspaper soaked in glue, which form a shell about a quarter of an inch thick. When the structure is quite dry, the clay is removed, and the hollow paper model remaining is appropriately coloured in oils. Such models may be made any size, are very light, and will stand rough usage. In the Harvard Zoological Department, W. McM. Woodworth exhibits some enlarged wax models of microscopic objects which are constructed from thin sections by an ingenious method. A camera drawing is made of each section in the series on some thin paper. The drawing is then attached to a sheet of wax which bears the same proportion to the thickness of the original section as the drawing does to its area. Each sheet of wax is trimmed to the outline of the drawing, and the successive sheets are then stuck together. The details of manipulation, which cannot be described here, are fully illustrated by specimens and apparatus.

There is no regular exhibit of scientific instruments, except that shown by several German firms in the Electrical Building. Here, too, are the excellent anatomical and embryological models made by Fr. Ziegler, of Freiburg i/B. His series now numbers 279, and costs about £120: which is not dear, considering its educational value.

Under the auspices of Harvard there are carried on in the Anthropological Building an Anthropometric and a Psychometric Laboratory. Here, in return for a dollar, one may have almost every physical and mental character tested. With the anthropometric tests Mr. Francis Galton has pleasantly familiarised us in England; but the psychometric ordeal is somewhat of a new departure, worthy of a more detailed account than is possible in this article. The casual but curious visitor to Chicago is constantly checked by some psychologic puzzle, either a pathetic appeal to choose his favourite colour among several crude abominations, or an entreaty to determine which of several meaningless geometric figures most appeals to his artistic sense of beauty. In a conscientious endeavour to advance Science by submitting to these mental tortures, I lost several hours that might have been as usefully employed in the Midway Plaisance.

To turn to GEOLOGY. One cannot complain of lack of material; collections of Fossils and Minerals are to be found in at least eighteen buildings. The most interesting of these are the Stratigraphical series of Fossils exhibited by the United States Survey in the Government Building; the educational collection of leading Fossils exhibited by Professor Ward in the Anthropological Building, and the very instructive set of specimens illustrating Dynamical Geology, in the Mining Building; his systematic collection of Minerals and set of Meteorites in the same building are also fine; the magnificent set of local fossils, including the Worthen Collection of types, in the Illinois State Building; the G. F. Kunz collection of Minerals and gems. A smaller collection, but one of the best shown by a private individual, consists of the Fossils exhibited by Mrs. A. D. Davidson in the Woman's Building.

The Mining exhibits of most of the States and Countries are either advertisements, commercial, or clap-trap. When the best that Great Britain can do is to hew a statue of Liberty in a lump of rock-salt, one begins to wonder whether we have ever boasted a School of Mines or a Museum of Practical Geology. True that there is a good collection of Economic minerals by Mr. B. H. Brough, which we are glad to see has been awarded a prize. I am told, too, that our Geological Survey has an exhibit somewhere; but what geologist would ever look for a geological map under the head of Liberal Arts? The German and Japanese exhibits are the only ones of any scientific pretensions. The merit of the German miners is, however, sufficiently well-known to Englishmen, and the geological work of Japan is important enough to demand treatment in a separate article.

The Standard Oil Company has an interesting exhibit in the form of a large scale model of a geological section across Ohio, Pennsylvania, and New York, showing the positions of their chief wells, and the relations of the oil-bearing shales. Except for a few geological maps, this is the only exhibit of the kind in the whole of the Fair. *Apropos* of the Oil Company, it may be mentioned that the twenty huge boilers that supply all the power on the grounds are heated by petroleum brought a distance of eighteen miles in a pipe.

In the Anthropological Building, Professor Wright illustrates by numerous specimens and photographs the relations of man to the Glacial Period in Ohio, of which he gave an account in the *Popular Science Monthly* for May of this year. Here, too, are a couple of marvellous exhibits. First, some footprints of various animals from a quarry of what appears to be Tertiary Sandstone at Carson, Nevada, among which certain large depressions, about the size of an elephant's footprint, are confidently assigned to Man. This, of course, upsets all the conclusions of the geologists and biologists. There have never been wanting cranks in America to uphold the existence of Man in any geological period, from the Silurian downwards; but we hardly expected to see their "notions" seriously admitted into the World's Columbian Exposition. And yet, close by, is an even more childish case, entitled Freaks of Nature, and containing such rarities as Noah's Canary, Mother Eve's Mitten, and Little Ham's Pegtop. At the end of the same gallery is an enormous map, illustrating the psychic lines of force that govern the structure of continents, from which the future course of events on our planet may be confidently predicted. Truly the Americans are a great nation, and the Anthropological Building is a great building, so they can afford to amuse us with a little folly.

Botany, considered as a Science, is not largely represented at the Fair, although the Botanist should find plenty to interest him in the Horticultural, Forestry, and Agricultural Buildings. In the first of these, the Japanese garden, with its dwarf trees, attracts some attention. Here one may see a pine tree 100 years old, and only 2 ft. high. The process of dwarfing appears to consist chiefly in eliminating a large number of the leaves and pruning the fresh shoots. The Forestry Building is chiefly devoted to polished woods, many of which are also to be found in the various State Buildings. The most interesting botanical exhibit is probably the selection from a wonderful series of glass models of flowering plants, shown by Harvard University. These are made by Blatschka, of Dresden, in his inimitable manner, and represent the plant as fresh as a daisy, as well as enlarged anatomical details. Nothing so beautiful exists in any other Museum of Natural History. After these, one cares little for the ordinary herbarium; but those who wish to see dried plants will find a few in the State Buildings of California and Michigan, as well as in the Woman's Building.

The scientific exhibit in the Woman's Building is, indeed, chiefly confined to botanical collections of the ordinary type, the only other specimens of interest being Mrs. Davidson's fossils. If Woman chose to divorce herself from Man, and to have a house to herself, is she need not have shown herself as weaker than she is. One can imagine something a great deal better than this rather amateurish natural history display that poses as a scientific exhibit. Some papers by Miss Cora Clarke do not represent all the scientific activity of *la belle Americaine*; while foreign ladies seem to have been too shy to put in an appearance at all. To speak only of the English ones, surely the Misses Barton, Buchanan, Crane, Donald, Johnson, Raisin, and others might have exhibited something, if not for the sake of their sex, at least to the honour of their country. As for so eminent a writer as Madame Pavlov, as she has not yet described any fossil pigs, it is not to be expected that Chicago has ever heard of her.

To include ANTHROPOLOGY and ETHNOLOGY under the head of Natural Science, and adequately to describe their manifestations at Chicago, would double the length of this article. For, in one sense, the whole Fair is an Ethnological collection. There is, however, so little real science about any of the exhibits, that we need only allude to the more important. North American Indians are well represented by both recent and prehistoric relics in the Anthropological Building, where also are to be found some rare Mexican and Aztec curiosities. Reproductions of burial grounds, with real live mummies, serve as a Chamber of Horrors and attract a goodly crowd. These have been arranged by the Bureau of Ethnology, under the skilled direction of Professor Putnam, who also shows a fine series of human skulls. Outside the Anthropological Building is a hill, made of tinned iron, which contains an accurate copy of the caves and remains of Colorado Cliff-dwellers. The Australian and African weapons and utensils, and the Japanese prehistoric relics, are exceedingly interesting. Some of the State Buildings and a few Foreign Buildings also contain good collections of prehistoric relics; but, after all, the chief Ethnological interest of the Fair is living, and all around one, notably in the Midway Plaisance. Turks and Egyptians, Dahomeyans and Samoans, Javanese and Japanese, Chinese and Hindoos, meet and mingle in good fellowship with French and Russian, German and English, Italian and Swede, and with that perplexing product of Western civilisation—the American negro. It is all very well for the Dryas-dusts to scoff at the pleasures of the Plaisance; but it is as superior to the legitimate collections of the Exposition as a zoological garden is to a row of stuffed animals, or an aquarium to a collection of fish-bones. Nor must we forget the forty beauties, in their national costumes. If the World's Fair offered no other object of interest to the naturalist, it would at least enable him to study the fair of the world.

F. A. BATHER.

III.

The Place of the Lake-Dwellings at Glastonbury in British Archæology.[1]

THE discoveries made by Mr. Bulleid in the Lake-dwellings at Glastonbury are of great historical value, and give a remarkable insight into the condition of the inhabitants of Somerset in the Iron Age. Moreover, their archæological horizon is clearly defined, and they lie close to the frontier which divides the Prehistoric Archæology from the history of the British Isles.

The dwellers in the Glastonbury marshes were spinners and weavers, and used whorls of stone and earthenware in twisting their thread, and weights to keep the warp tight on the loom while they worked in the weft with bone shuttles. The weft was pushed home with the weaving combs, which are both abundant and perfect. The weaving comb probably was the ancestor of the comb worn for ornament in the head-dress of later times. Numerous wooden fragments of a kind of frame probably represent the loom. Flax, in all probability, was the material which was woven, although no direct proof has been met with in this settlement. They used bone-needles for sewing.

They worked wood with great skill with the saw, the bill-hook, the knife, and the gouge, and probably also with the axe and the adze, although the two latter can only be inferred from the workmanship of the boards, and squared parts of the platform.

They also used the lathe, and are proved by the "chucks" of Kimmeridge Clay to have turned ornaments of Kimmeridge shale. Some of these have been discovered.

The lathe-turned vessels, some bearing the marks of a punch found in the settlement, prove that pottery-making was also carried on. Crucibles, and the remains of tuyères, imply that smelting was also carried on, and a piece of blue glass slag may perhaps imply that glass-working was also practised. A file implies also metal-working.

They used rings of jet, amber, and glass, and of bronze, and bracelets of bronze and Kimmeridge shale, and beads of glass, and fastened their clothes together with safety-pins and split-ring brooches of bronze, and with bone links similar to those found in Romano-British caves, such as the Victoria Cave in Yorkshire. They also used amulets of bone, among which is a roundel fashioned out of a human occipital.

[1] Abstract of Address, British Association, Nottingham, Anthropological Section.

Their huts were round and, as at Mount Caburn, near Lewes, composed of wattle. They grew wheat, and had sheep, cattle of the small *Bos longifrons* breed, pigs, horses, and dogs. They did not, however, rely wholly on the domestic animals for food, but, at times, ate the stags, roedeer, beavers, and otters living in the district.

They ground their corn in well-fashioned querns, and boiled the food by putting hot stones into the pots filled with cold water. It is strange to note among these the "potato stones" of the neighbourhood.

A spur of a fighting-cock renders it probable that they were given to cock-fighting like the ancient Gauls.

They rode or drove horses with iron snaffle-bits, and fought at close quarters with daggers, halberts, and bill-hooks, and at a distance with slings. Vast numbers of clay pellets (= the Roman "glandes") for slings, both burned and unburned, have been met with.

The position of the settlement in the marsh implies the fact that warfare was the normal social condition, and testifies to the danger of attack from neighbouring communities.

A fragment of a human skull, long, and with a low forehead, and strong frontal sinuses, implies that some of the inhabitants belonged to the long-headed section of the Britons. It may further be remarked that a shaft of a human humerus, gnawed by some weak-jawed carnivore such as the dog, was also found in one of the huts.

It remains now to sum up the place of these remains in British Archæology. The pottery is distinctly of Southern derivation and of the Late Celtic type, which belongs to the late period of the Iron Age, before the Roman influence had fully penetrated into Britain. Although the split-ring fibula and the bone links are identical with forms of Romano-British type, the absence of Roman pottery and of coins implies that the Roman civilisation had not yet arrived in the Isle of Glastonbury. Roman pottery, it may be noted, abounds in other sites in the district. On a comparison with the Late Celtic remains found by General Pitt-Rivers at Mount Caburn, near Lewes, it will be found that the iron tools and weapons, the earthenware "glandes," the pottery, and the various other articles, and the wattle-work, are practically the same, and belong, therefore, to the same age. The whole group of domestic animals, including the fighting-cock, is also the same in both. The safety-pin brooches, too, are of late Celtic type, and similar to that found in the Late Celtic cemetery at Aylesford, explored by Mr. Arthur Evans. We may, therefore, fix, with tolerable certainty, the age of these Lake-dwellers as being just before the time that the Roman influence was directly felt in the West of England, and certainly before the Roman Conquest. The discovery is most important. When fully worked out it will probably throw a flood of light on the history of pre-Roman Britain.

W. Boyd Dawkins.

IV.

The Air-sacs and Hollow Bones of Birds.

NOT very long ago the problems connected with this subject were settled in a very offhand way. The heated air in the sacs being lighter than the surrounding air, made the bird a balloon, and so flight was easy. For the same object, the bones were hollow and marrowless. Thus a clear and interesting solution seemed to have been found for a great problem.

This theory has withered beneath the cruel light of fact. A bird can carry only a very small amount of air in its sacs, and the difference in weight between a few cubic inches of heated or cold air is too infinitesimal to be worth considering. The sight of an eagle flying off with a lamb ought to convince anyone who cannot otherwise be convinced, that the saving of the tiniest fraction of an ounce of weight would make no difference. True, air within the bird, whether heated or not, will expand its volume, lessen its specific gravity, and so fit it better for floating, but it could not help it to rise, and this is the real difficulty. Moreover, many birds, for instance the swallow, which fly to perfection, have all their large bones solid.

We must, therefore, look for some sounder theory, and first it will be well briefly to survey the facts. The lungs of birds open out into great membranous expansions which lie within the body-cavity next to the ribs or extend far back between the kidneys and the intestines; and, in addition to this, in many cases, the membrane finds its way into the bones as the marrow dries up, sometimes even to the very extremities of the limbs, and in some instances under the skin also, into some of the feathers and between the muscles. Anyone who doubts the connection between the chambers within the bones and the lungs, may convince himself by a very simple experiment. He can take a dead bird (any bird which has some of its bones aërated), break the humerus, and, after tying up the trachea, blow down the bone through a tube, when all the air-sacs will expand as promptly and completely as if the tube were inserted into the trachea.

Before now, a wounded bird, whose windpipe has been stopped with blood, has been known to breathe through a broken and exposed bone. It is beyond a doubt, then, that the hollow bones are lined with expansions of the bronchial membrane; it extends, in fact, even to some parts of the skull.

I shall first discuss the functions of the air-sacs proper as distinguished from their extensions into the bones. It cannot be

questioned that they exist partly for purposes of respiration. The lungs themselves are small and inelastic, and the diaphragm, whether it represent that of mammals or not, certainly does not do the same work. By the action of muscles the body-cavity is expanded, a vacuum is created in the sacs, and the air rushes into them through the lungs. But it is difficult to believe that the air-sacs fulfil no purpose beyond that of respiration. If that is the case, they are far larger than necessary.

I have measured the cubic content of the lungs of a pigeon as nearly as I could, and found it to be $\frac{1}{4}$ inch. The aggregate length of the air-sacs was about $3\frac{3}{8}$ in., the depth about 1 in., the breadth about $\frac{3}{8}$ in. This gives a cubic content of $1\frac{17}{64}$. According to this estimate, therefore, the air-sacs can hold more than five times the amount of air the lungs would hold, even if they were mere bags. And probably this estimate is decidedly below the mark. This disproportionate amount of air is of some use in breathing. When the bird exhales, comparatively fresh air from the sacs is driven into the lungs, so that exhalation and inhalation alike renew the supply of oxygen; but even when this is allowed for, the air-sacs are far more spacious than is necessary for breathing alone. We have to consider, therefore, whether they do not fulfil some useful purpose in addition to their respiratory functions, and if we reflect upon the subject, it is difficult to avoid coming to the conclusion that a bird's temperature is regulated mainly by the lungs and their extensions. This will seem more than probable if we consider the means by which the temperature of the human body, when in health, is kept almost at the same point, however much that of the surrounding air may vary. Heat is lost (1) through skin by conduction, radiation, and evaporation; (2) by respiration; (3) to a small extent through the excreta. Besides this the temperature of the body is partly controlled by the vaso-motor nerves which regulate the flow of blood to particular parts. And, that this is not the only way in which the nervous system governs the temperature, there is indirect evidence in the fact that when a warm-blooded animal is subjected to urari poisoning it behaves like a cold-blooded animal. It has no longer any power of generating heat within itself in order to withstand external cold, or, when exposed to heat, of keeping itself cooler than the surrounding atmosphere.

Whatever the exact nature of this nervous apparatus may be, it is certain that in man the skin plays a very important part in the regulation of temperature, and that it is mainly by evaporation that it does its work.[1] Radiation is constant, but is much checked by

[1] Dr. Michael Foster ("Text-book of Physiology," p. 464, 1883 edition) writes: "It has been calculated that the relative amounts of the losses by these several channels are as follows: in warming the urine and fæces about 3, or according to others 6 per cent. By respiration about 20, or, according to others about 9 only per cent., leaving 77, or alternatively 85 per cent., for conduction and radiation and evaporation from the skin."

clothing; it is only occasionally that we lose much heat by conduction, for instance, when we touch cold iron; evaporation never entirely ceases, and it varies in amount according to the needs of the body. Man, in fact, is like one of the porous earthenware pots used in India for cooling water. Put them in a hot, dry wind, and, the rate of evaporation increasing, the water cools all the more rapidly, and the Sahib's bath is ready all the sooner. Great heat can be endured if only it is dry heat. A French physiologist once stopped a considerable time in a stove heated to 160° Fahr.; but this is far below the record. In 1760, according to the testimony of two French academicians, a woman entered a stove heated to 237° Fahr. and stayed there 12 minutes. Doctors Fordyce and Blayden were able to remain in a chamber heated to 260° Fahr. I have been told that a man who earned his living by feats of this kind found himself compelled to rush precipitately from the heated oven because some one, who was more scientific than kind, had placed a can of hot water in one corner. Everyone knows how oppressive the heat of a hot-house is. The heat of the vapour-baths in Russia is said sometimes to rise to 116° Fahr.; but between this and 260° there is a great gulf. When the air is moist evaporation is checked, and the human system has greater difficulty in keeping its temperature down to the normal.

Birds have, probably, nerves or nerve-fibres similar to our own exercising a control over their temperature; but evaporation from the skin cannot go on except to an inconsiderable extent since they have no sweat glands. Radiation on any large scale is prevented by the thick covering of feathers; but it is a question whether one object of the bare patches, called apteria, may not be to allow of the free access of air. Their temperature is very high, varying from 100° Fahr. to 112°. Every warm-blooded animal, we may take it for granted, has an efficient heat-regulating system. The higher its temperature the more efficient, we may assume, the system to be. What, then, in birds takes the place of radiation from the skin? We may get a hint from watching a dog when taking vigorous exercise. Dogs perspire through the tongue, and also, I believe, through the feet, but not through the skin generally. The greatly-increased rapidity of breathing is the chief means by which they keep their temperature down. All the expired air is about the temperature of the body. From this the cooling effect of rapid breathing may easily be gathered; and here we have, I believe, the explanation of the great size of birds' air-sacs, and also, perhaps, of the long ramifications of the trachea which we find, for instance, in the crane. The greater the amount of air breathed in and out, the more the body will be cooled. In man and in birds the method is in reality the same; but in birds evaporation and radiation take place mainly from the lungs and air-sacs, in man mainly from the skin. If we watch a bird's breathing we obtain evidence of this; when standing still it breathes about 20 times in the minute, whereas an adult man in a sitting

posture takes only from 13–15 breaths in the same time. During flight a bird's breathing must be far more rapid, and it is impossible to avoid the conclusion that its temperature is kept equable by this means.

There remains the difficult question of the aëration of bones. It is possible that this also may be of some slight use in the regulation of temperature; but if this is so, it can only be a very secondary object, since the air cannot be expelled from them at will. Before proceeding to discuss the problems connected with pneumaticity, I will briefly set down the main facts.

(1.) Many small birds that are first-rate flyers have either marrow in all the large bones, or else in all except the humerus.

(2.) Most of the big strong-flying birds have a great deal of aëration.

(3.) The hornbills, which, according to good observers, are very poor flyers, are as pneumatic as any birds, or, perhaps, more so than any.

(4.) Birds which dive have solid bones or only the humerus aërated.

(5.) Birds which spend much of their time in the water without diving have, at least in all the cases which I have been able to investigate, nearly all the bones solid.

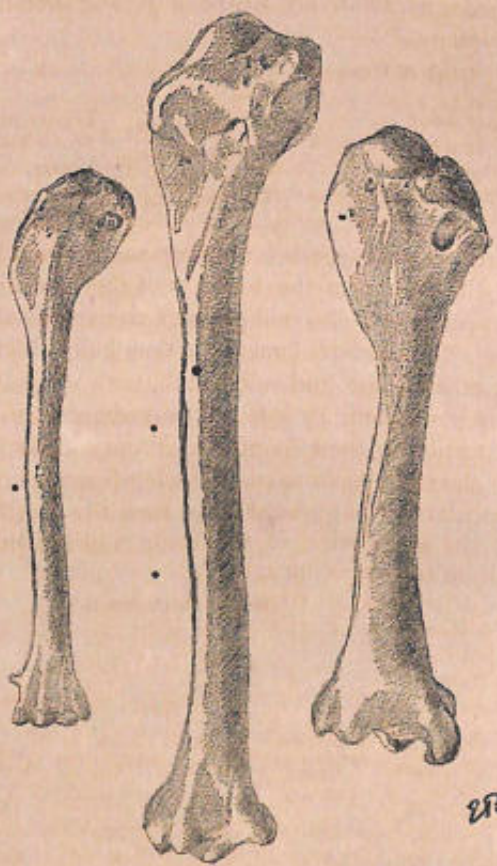
(6.) There are great differences between nearly-related species, *e.g.*, the gannet has an extraordinary amount of aëration, while its near ally, the cormorant, has only the humerus pneumatic. The hornbill is not very distantly related to the swift, which is singularly deficient in aëration.

(7.) The bones of birds that are highly pneumatic are, relatively to their length, larger in girth than those of birds in which little aëration is found.

To find one's way through these facts is not easy. But one point must strike anyone. Great pneumaticity might be an inconvenience to a diver. As it is, he can regulate the amount of his body that appears above the water, sometimes sinking till no more than his head is visible. This, no doubt, he effects by driving the air from his sacs. But aërated bones would certainly not help him to vary his specific gravity, and they would make it more difficult for him to swim under water. Probably, the marrow in the bones serves a very important physiological purpose. Divers are frequently exposed to great cold when in the water. They are protected against this by a peculiarly thick coat of feathers, and by a deep layer of fat beneath the skin; and I cannot help thinking that the marrow also helps to maintain their warmth. It is held that in it a large proportion of the red blood corpuscles are generated, and unless they are very thick in the blood, a high temperature cannot be maintained. But if the marrow is a factory of red corpuscles, what substitute for this have birds whose bones are marrowless? Though, as a rule, exposed to less cold than water-birds, they have a

very great power of generating heat. Birds, as a class, have more red corpuscles than any other animals. Is the spleen, the other great red corpuscle factory, more developed in birds which have little or no marrow? These physiological problems are of the greatest interest, and I only wish that I were in possession of facts that would throw light upon them.

Putting physiology out of sight, I am going now to consider why it is that, among birds of powerful flight, we find differences so great



a. *b*. *c*.

Humerus of Pomatorhine Skua (*a*), Vociferous Sea Eagle (*b*), and Rhinoceros Hornbill (*c*).

in the amount of aëration, and why such a poor flyer as the hornbill, is, in respect of bones, so well equipped for aërial navigation. To put physiology aside, is to assume that if hollow bones are advantageous to a bird, Natural Selection can bring it about that they become hollow, and that the bird is able to dispense with the marrow. This would indeed be a bold assumption, did we not know that it is an accomplished fact. The bones are hollow and the processes of life continue.

We shall find that as the wing lengthened, so as to make a longer and stronger stroke possible, the bones became larger in girth, larger not only absolutely, but in proportion to their length; and that a decrease of weight accompanied the increase of strength, mainly through the drying up of the marrow, but partly through a reduction, if we allow for the increased size of the bones, in the thickness of the hard, osseous shell. I shall give, first, a few measurements to show that in the case of birds whose bones have little or no aëration, the girth of the bones is, relatively to the bulk and weight of the body, considerably less—

		Girth of Humerus. inch.			Girth of Humerus. inch.
Bones highly pneumatic.	Screamer	$1\frac{5}{8}$	Bones very little or not at all aërated.	Logger-headed Duck	$1\frac{7}{16}$
	Rhinoceros Hornbill	$1\frac{3}{8}$		Scoter Duck	$\frac{13}{16}$
	Golden Eagle ..	$1\frac{5}{8}$		Nestor Parrot ..	$\frac{9}{16}$
	Vultur monachus ..	$2\frac{1}{4}$		Red-throated Diver	1
	Marabou Stork ..	$2\frac{5}{16}$		Spur-winged Goose ..	$1\frac{5}{16}$

These measurements speak for themselves, even without any exact statement of the weight of the birds; but the following illustration and the accompanying tables will do more to explain the problem of hollow bones. The shoulder bones of a skua gull, which has scarcely any aëration, of an eagle and a hornbill, both of which are highly pneumatic, are placed side by side. The greater girth of the hollow bones in proportion to their length is at once clear. But to bring this out more clearly, I have taken the wing-bones of the skua as a model and calculated what would have been the length of the same bones and of the whole wing in the eagle and the hornbill, if they had been built on the same lines:—

	Girth of Humerus.	Girth of Ulna.
Pomatorhine Skua	$\frac{25}{32}$ inch	$\frac{5}{8}$ inch
Vociferous Sea Eagle	$1\frac{9}{16}$	1 ..
Rhinoceros Hornbill	$1\frac{3}{8}$	$1\frac{1}{4}$ ..

	Humerus.		Ulna.		Aggregate length of Wing bones.	
	Actual Length.	Length proportionate to girth.	Actual Length.	Length proportionate to girth.	Actual Length.	Length proportionate to girth of Humerus.
Skua	$4\frac{3}{4}$	—	$4\frac{9}{16}$	—	$13\frac{5}{8}$	—
Eagle	$6\frac{3}{4}$	$7\frac{7}{16}$	$7\frac{1}{16}$	$7\frac{3}{16}$	$20\frac{7}{16}$	$22\frac{1}{20}$
Hornbill	$4\frac{5}{8}$	$7\frac{7}{16}$	7	$9\frac{1}{4}$	$15\frac{5}{8}$	$23\frac{13}{20}$

Thus, if in the eagle's humerus length were proportioned to girth, as in the skua's, the bone would be more than $\frac{1}{2}$-inch longer; on the same principle the aggregate length of the wing-bones would be greater by more than $1\frac{1}{2}$-inch. The increase of length in the case of the hornbill is far more startling.

If now we take a saw and cut the humeri of the skua and eagle from end to end, we shall find that the walls of the latter are not thicker in proportion to the greater girth of the bone. The girths are in the ratio of 25 : 42; while 3 : 4 represents the ratio of the thickness of the walls, the measurements being $\frac{3}{100}$ and $\frac{4}{100}$ inch. We can now see why small birds have so little aëration. In their case there would

be no great reduction of weight, since the exterior shell of the bones forms a great part of their bulk. In the case of a larger bird, with bones many times multiplied in size, but the thickness of the walls increased very little, the removal of the marrow will be a great advantage. This will be clear if we take two cubes, a side of one of which is twice the length of a side of the other. Then the face is four times as large and the cubic content eight times that of the smaller cube. This will be true of other figures, so that if the average diameter of one bone be double that of another, and if the length also be double, its cubic content will be approximately eight times as great. And as the walls thicken very little with increased girth nearly all the enlarged interior can be filled with air. Clearly, therefore, a large bird has much more to gain by dispensing with marrow than a small one.

The eagle, then, has gained in lightness. It must also have gained in strength, for increased length of wing means an altogether disproportionate increase of work. The longer the wing, the greater the pace at which its extremity will move, and the resistance of the air increases as the square of the velocity.

The hornbills are a puzzle. The extreme shortness of the hand bones, a ridiculous anticlimax following upon so grand an ulna and so portentous a humerus, might suggest that they were once better flyers, and that the wing is slowly undergoing reduction. But the mountainous beak seems to show that colossal bones are an ancient heritage of the family, and that even feeble flight might have been difficult had they not become hollow. In either case they have been very great gainers by aëration.

Anyone who wishes to see clearly the relation in birds' bones of slimness to solidity, and of large girth to aëration, should inspect collections such as those at the Royal College of Surgeons, or at the Natural History Museum at South Kensington, where a large number, representing different families, may be seen side by side. It is easy, then, to see that big, long-winged birds have wing-bones thicker in proportion to their length in order to bear the far greater strain upon them: the aëration of the bones has obviated the natural increase of weight, which would have been a serious hindrance. But there remains the perplexing physiological problem: what organ of the body does the work that, in mammals, and presumably in birds with solid bones, is done by the marrow?

F. W. Headley.

V.

On the Ætiology and Life-History of some Vegetal Galls and their Inhabitants.

THIS paper is to be regarded as of a preliminary character, merely treating of the subject of galls in its more popular aspects. It embodies the results of observations made by the writer in his scanty leisure of the past twenty-five years.

With regard to their origin, galls (in the restricted sense in which the term is here applied) are complex organisms, resulting from the co-operation of a plant and an animal; and to determine the extent and *modus operandi* of these two factors in their production is one of the many interesting problems which this study suggests, but for the solution of which no complete answer can as yet be given. Why, for instance, from the action of one species of insect, a large, irregular excresence should be produced; or why, from that of another, a smooth, spherical gall, or a scaly bud, or a circular disc, is a mystery which, for the present at least, science is powerless fully to unravel. It is, however, but a special instance of the universal problem, as to the cause by which normal organic structures are produced in normal organisms.

Though abnormal with regard to the plant, inasmuch as their presence is exceptional and foreign to the performance of its proper functions, galls, in themselves, are nevertheless as normal as any other organisms. Each has its own characteristic form, its special habitat, and its proper office.

Composed at the outset, like all vegetable growths, of cellular tissue, galls undergo more or less modification as they pass through the several phases of their life-history. Some, at maturity, are hard and woody; others soft and succulent. Their colours are bright red, or green, russet-brown, or white, or yellow; with, oftentimes, such nice gradations and harmonious blendings of all these, as to give to them the aspect of ripe fruit or quasi-flowers. Though, as a rule, more or less globular in form, their shapes vary considerably. Some are smooth and regular, others rough and amorphous. In one, we have the form of an elongated cone; in another of a cup or goblet; a third is urn-shaped; a fourth discoid; a fifth reniform. Some, as the oak-apple, are composed largely of a mass of spongy parenchyma;

others, like the woolly gall of the oak, are surrounded by a thick coating of cottony-down; while in the case of the Bedegnar of the rose, we have a tangled mass of branching-filaments, compact yet free, and so compressible as to have obtained for this gall the names of the "Rose-Sponge" and "Robin's Cushion."

Few plants, it is said, are altogether free from these parasitic growths. Of those indigenous to Great Britain, a list of over 150 gall-bearing plants has been given. Such growths are found alike upon trees and shrubs and herbs. No organ escapes their deleterious presence. They appear on root and stem, on branch and leaf, on bud and flower, and fruit.[1] Every part, in turn, pays tribute to the invaders.

Of our native plants, the Oak (which is especially rich in varied forms of animal-life) produces the greatest number and variety of galls. From the oaks of Central Europe Dr. Mayr has described and figured ninety-eight specific galls.[2] Upon those of Nottingham and the immediate neighbourhood I and my friend, Dr. W. H. Ransom, F.R.S. (to whose initiative I owe my early interest in this subject), have noted some twenty-nine or thirty, out of a total of forty-one known British oak galls; while upon a single leaf I have counted more than 2,300 distinct gall "spangles"—those of *Neuroterus lenticularis*. Though many tumour-like galls are due to the action of parasitic fungi, Acaridæ, and other as yet imperfectly classified organisms, the great majority are produced by various orders of Insecta. Among these, the busiest gall-makers are found among the Cynipidæ (or Gall flies), and the Tenthredinidæ (or Saw-flies), two subsections of the Terebrant Hymenoptera. The galls of these insects are (so far as I know) invariably closed galls, from which the larvæ, in some cases, and in others the imago—then furnished with mandibulate jaws—eat out their way, when mature, and escape.

In the case of the Cecidomyidæ (or gall-midges) and other families of Diptera, as well as among gall-producing Aphides—all of which have suctorial mouths—the galls, on the other hand, though in most instances closed for a time, decay or dehisce so soon as the contained insect has reached a stage when its exit from the gall becomes necessary; but for this wondrous adaptation and provision, the home of the little occupant would, of necessity, become its prison-house and grave.

Limiting now our attention to a few only of the typical galls referred to, let us endeavour to trace out some of the more salient features of their life-history.

And first, of the "Oak Nut," or "Marble gall," as it is sometimes called. This is a simple, unilocular gall, due to the puncture of *Cynips lignicola* of Hartig, or *Cynips kollari* of Geraud, the largest

[1] Malpighi, quoted by Réaumur, vol. iii., p. 418.

[2] "Die Mitteleuropäischen Eichengallen," 1870.

member of our British Cynipidæ. It is said to be indigenous to France, and to resemble very closely the Aleppo gall, or galls of Commerce (*Gallæ tinctoriæ* of Olivier).[3]

It first appeared in this country, or was first noticed here, some 50 or 60 years ago, and was then limited to Devonshire and other of the South Western Counties. Gradually it extended itself inland, and is now, I believe, to be found abundantly in nearly all parts of Great Britain. It is the only one of the group to which the ink-gall belongs that occurs so far North as England or even Northern Germany.[4]

Each gall, with regard to position, occupies the place of a terminal or axillary bud, from the side or growing point of which it is developed; but I have occasionally met with instances in which one gall has grown out of or been superimposed upon another during its development. In its early condition, the gall is more or less cone-shaped, with pointed apex, and is slightly pubescent. It is then of a bright green colour, the surface spotted thickly over with crimson. As development proceeds, the crimson spots, which assume a scale-like aspect, separate wider and wider from each other; and the pointed apex, though persisting for a time, and in some instances altogether, usually disappears, leaving the gall at maturity more or less spherical in form, and lightish brown in colour.

In ordinary seasons, the young galls are first met with in July, and mature in August or September. This year, owing to the exceptionally bright weather of the spring, I found them as early as the 20th of June, and even at that date they had attained a diameter of from half to three-quarters of an inch.

The ovum of the gall-producing insect is, in this case, deposited in or near to the young bud of the oak at an early stage of its development, before any part of the cellular tissue is differentiated into leaves or other special organs; except, it may be, the few embryonic scales by which it is at times surrounded.

What is there, we may ask, in the casual presence of the little ovum; in the action of the developing larva; in the mechanical puncture of the parent cynips; or in the deposit of a tiny drop of irritating fluid by which it is said the ovipositing is accompanied; what is there, I ask, in any one, or more, or all of these, or, it may be, in the action of some other factor yet to be discovered, that impels those wonder-working changes by which the gall itself is produced, and by which its future growth and development are accompanied? What is it that, in the presence of this new agent, leads the succulent cells to resign their normal tendencies, and, instead of differentiating into

[3] The Gall-producer is *Diplolepis* or *Cynips gallæ tinctoriæ*.—"The gall of *Cynips tinctoriæ*," says Dr. G. L. Mayr, "occurs in the southern half of Europe, and near Vienna is frequently met with; that of *C. kollari*, however, is found as far as the German Ocean."—7 Entomol., 242.

[4] Zool. 4964, sec. 7, Entom., 245.

stem and leaves and blossoms, as otherwise they would do, to group themselves about this foreign organism, in radial and concentric layers, each endowed with new and peculiar properties, each destined to perform a new and special office?

Attempts have, of course, been made to answer these and cognate questions; but the explanations have, in great part, been little more than crude conjectures; and in no case, so far as I know, have they been of a character to meet, in any complete degree, the scientific requirements of the problem.

Redi[5]—who, like Von Helmont and the other vitalists, explained all organic phenomena by reference to the guidance of a distinct spiritual entity or Archæus—believed in a vegetative soul in each plant. This soul he considered presided at the formation of the egg, the insect, and the gall; and so determined their specific characters and their several relationships. Such views, at this day, we shall find it difficult to appreciate. They are, as Lucaze Duthiers has said, but the dreams "de l'esprit philosophique d'un autre temps."[6]

Réaumur, whose "Memoirs pour servir a l'histoire des Insects," (published A.D. 1738) contain so many curious and interesting details, was early led to refer the growth of galls to the suction of insects, having been himself much impressed by those produced by Aphides on the leaves of elms and limes. This suction, he believed, determined an increased flow of sap to the part affected, and, as a consequence, excess of vegetation and the production of the gall. As, however, both original puncture and subsequent suction not unfrequently take place in plants without any gall resulting, or without, in fact, any hypertrophy whatever, this can scarcely be regarded as the true explanation.

Neither, as Réaumur also supposed,[7] can the increased heat evolved by the developing egg be looked upon as a sufficient cause of the growth of the gall, inasmuch as no development, in the sense in which he used that term, takes place in the embedded ovum.

Malpighi,[8] who gave some considerable study to this subject, assumed a fermentation to be excited in the acid of the oak by the poison of the Cynips, and in this way sought to account for the production of the gall. He, however, like Redi and other of his contemporaries, was largely influenced by the prevalent spirit of the times, which saw, or thought it saw, fermentations in all things.

Lacaze Duthiers, whose "Researches on Galls," published in the *Annales des Sciences Naturelles* for 1853, give evidence of much painstaking study of this subject, adopts the hypothesis of a specific poison, a special *lignis virus*, as the main cause of the gall. It is, he says, a fact that all Hymenoptera have a poison gland connected

[5] Redi, born A.D. 1626.

[6] "Recherches pour servir a l'histoire de gallas," par M. Lacaze Duthiers. *Ann. des Sci. Nat.*, 3rd series, Botanique, vol. xix., pp. 284, published 1853.

[7] See "Memoirs," vol. iii., p. 478. [8] A.D. 1628–94.

with the ovipositor, from which it is easy to make them expel a drop of liquid by irritation. The presence of this fluid he considers to be the first essential fact in the order of causation; and he proceeds to illustrate its mode of action by reference to the analogy which he says exists between it and some well-established facts in animal and vegetable pathology. When the surgeon, charging his lancet with a drop of vaccine lymph, or with fluid "d'une ulceration syphilitique," introduces the point under the skin of a healthy subject, it is invariably followed, whenever morbid action is set up, by the reproduction of disease akin to that from which the virus has been taken, whatever may be the form of the incision, or the quantity of the fluid inserted. The specific quality of the morbid poison in these cases is, he says, an accepted doctrine—neither the result nor the evidence is questioned. Taking another series of facts, viz., those relating to the stinging of bees, wasps, scorpions, and other animals, he shows that the results, though in many respects akin to one another, are nevertheless specifically different. The swelling in one case is slight and temporary, in another it is large and persistent. It is the same with regard to the attendant pain. That resulting from the sting of a bee will continue for an hour or two, while, in the case of the scorpion, it may last for years.[9] No one, he says, denies the relation of cause and effect in such cases. Why, then, should we do so in that of the specific virus of the Cynips and the resulting gall? Once this first step taken, once the doctrine of a specific poison admitted, and we are in a position, he thinks, not only to explain the occurrence of galls in general, but to account also for their specific forms. No one, he says, has any difficulty in understanding how the normal forces of the plant vary with the poison, or how all secondary characters are due to the mode in which the poison acts in the developing vegetable tissues.

This view of Lacaze Duthiers is the one most generally entertained, and it has for its support the concurrent authority of Hofmeister,[10] and Darwin,[11] Sir James Paget,[12] Sir John Lubbock,[13] and other distinguished writers.

Whether or not, however, it can be regarded as the correct one, is, in a measure at least, doubtful. The analogy upon which it rests is by no means perfect. The presence of the ovum (not found in any of the cases referred to by Lacaze Duthiers) is, it may be, as necessary

[9] The injurious effects of the sting of the scorpion are said by Mr. Andrew Murray, F.L.S., to be much exaggerated, the pain being in some cases less than that of the sting of a wasp, and of incomparably shorter duration. *See* "Science Handbook of Economic Entomology—Aptera." South Kensington Museum, 1877.

[10] "Allgemeine Morphologie der Gewächse," p. 634 (1868).

[11] "Origin of Species," 5th ed., p. 572 (1869), and 6th ed., p. 6. "Plants and Animals under Domestication," 1st ed. (1868), vol. ii., pp. 382–4 418.

[12] "An Address on Elemental Pathology," Brit. Med. Assoc., Aug., 1880.

[13] "On the Origin and Metamorphosis of Insects," *Nature* Series, 1874, p. 10.

a factor in the production of the gall as is the deposit of a specific virus; while it must be remembered that galls, in many cases, result from the action of other animals than Terebrant Hymenoptera, as, for example, of *Chermes*, *Cecydomiæ*, and *Acari*, where no such poison gland as that referred to exists.

Very early in our investigations Dr. Ransom and myself arrived at the conclusion that another agent, as potent as that of this hypothetical virus, was essential to the production of at least some species of vegetable galls, such agent being the presence and action of a living larva—a conclusion communicated by me in a paper read before the Natural Science Section of our then Literary and Philosophical Society, on the 4th of April, 1877.

Subsequently to that date, the subject received exhaustive study at the hands of Dr. M. W. Beyerinck, of Utrecht, whose "Observations on the Early Development-Phases of some Cynips Galls" were published by the Royal Academy of Sciences, at Amsterdam, in the year 1882. In these, Dr. Beyerinck, as a deduction from the like series of facts we had ourselves observed, submitted the opinion that, in the cases referred to, the action of the Cynips larvæ, and not the injection by the parent Cynips of a specific virus, was the sole cause of gall-formation.[14]

Whether so or not, is in a measure, at least, doubtful. This, however, I think we may safely conclude, namely: that while, on the one hand, in those chemical and other forces which produce growth, greater activity is induced by the stimulus of the injected fluid—assuming such fluid to be actually present—so, on the other, those mechanical conditions which determine form in organic beings are furnished, to a large extent, by the contact of the included ovum, and by the activities of the embryonic larva.

As the nut-gall approaches maturity—which, under favourable circumstances, it may do within a fortnight or three weeks of its first appearance, early in July—the cellular tissue of which it is at first composed becomes differentiated into five principal layers. The innermost of these, surrounding the central chamber, and known as the alimentary layer, is composed of thin-walled cells, filled with protoplasm, minute starch granules, oil, and albumen. This layer disappears *pari passu* with the developing larva, which finds in its substance the food-elements necessary to its growth. At first the chamber-walls are in direct contact with the very young larva, whose increase is, in the first instance, due to absorption or to suction.

Next in order is the protective layer, or *Couche protectrice*, built up of hard, compact cells, with strong, thick walls. Around these are arranged, in radial series, bundles of elongated cells, forming a sort of *chevaux de frise* or abatis—the whole constituting a fortified casemate or stronghold for the protection of the occupant and her commissariat

[14] *See* "Beobachtungen über die ersten entwicklungsphasen einiger Cynipiden gallen" von Dr. M. W. Beyerinck, p. 8.

from the attacks of parasitic and inquiline enemies. How far it is effectual for this purpose will be seen in the sequel. Together, these several layers constitute less than one-half the diameter of the gall, the remainder being composed of loose cellular parenchyma, rich in tannin, over which extends a thin epidermis, formed of flattened cells, and resembling, in general aspect, that found on other parts of the plant. The colour of the gall, when young, is due to the presence of chlorophyl granules in the *Couche cellulaire*, these being readily seen through the thin transparent cortex. Later on, these epidermal cells become thickened and coriaceous, and the colour of the gall changes to light brown.

Passing now from the consideration of the nut-gall to that of the oak-apple—the *Pomme de Chene* of Réaumur—we find, in lieu of a single central cavity, a more or less numerous series of closed loculi, each containing a single occupant, the whole embedded in a spongy mass of cellular tissue, surrounded by a common cuticle or epidermis. Like the nut-gall, the oak-apple has its origin in a bud, terminal or axillary, and usurps the place of what, under normal conditions, would be a stem or branch. It is among the best known of our British galls—appearing early in the spring, in some cases even before the scales of the leaf-buds separate or the first tender green of the year is seen. In favourable seasons it is met with as early as the third week in April, already a blush of crimson on its delicate cheek. About the middle of May, or by "Royal Oak Day"—a time when, according to custom,

> "Custom which all mankind to slavery brings,
> That dull excuse for doing silly things."

many of our king-and-constitution-loving ancestors were wont to use it in the decoration of their May-boughs—the oak-apple attains, in favoured situations, its full development. In very fine specimens it measures from two to two-and-a-half inches in diameter; though in size it varies considerably, and in form is less regular and globular than the marble nut-gall.

About the last week in June, or the first in July, the normal insect—*Cynips terminalis* of Fabricius, *Teras terminalis* of Marshall—makes its escape from the gall, and, after a brief but active existence of four or five days, devoted to the intercourse of the sexes, sickens and dies,—a result to which, so far as my experience goes, there is no exception: not a solitary insect survives.

At the time of our first investigation of this subject, the generally-accepted opinion was, that the eggs of *Teras terminalis* were laid in the summer or autumn of the year in which the gall appeared in the spring; and this at a time when the axillary and terminal buds of the summer shoots, close shut in their envelope of scales, had yet so far differentiated as to show a well-marked series of embryonic leaves. To this view, however, a difficulty presented itself. Our investigations showed that, at the time the insect died, these buds had not

attained that stage of development at which it was clearly seen they had arrived when the ovipositing must have actually taken place. Here, then, was a case in which fact did not agree with theory; our business, therefore, was, by further observation and experiment, to seek a solution of the difficulty. Accordingly, between the 16th of July and the 13th of December, 1876, we made a daily examination of buds from freshly-gathered twigs, by cutting these in section from apex to base, so as to expose the interior of the bud. No trace of eggs, however, appeared until the latter date, when the first group was met with. Subsequently, they were found in increasing numbers on each examination. In this way, between the dates referred to, I myself bisected 4,100 of these buds, many of them being examined under the microscope. Dr. Ransom, in like manner, opened 2,250, together 6,350 in all.

The sought-for result being thus arrived at, a new difficulty presented itself. If the gall-producing cynips died, as it was found to do early in July, how could it possibly lay its eggs in the December following? To this our own investigations gave no present answer; but a solution of the problem shortly came from Dr. Adler of Schleswig, whose researches on the alternation of generations of Gall Wasps (published in the "Zeitschrift für wissenschaftliche Zoologie" for 1880) established the fact that *Teras terminalis*, in issuing from the gall, proceeds not, as was supposed, to puncture the young buds of the oak, but to make its way to the roots of the tree, and there to deposit its eggs. In association with these a new and specific gall is formed, from which, in due course, issues a swarm of females (and none but females, for no male has ever yet been found) so unlike the parents which produced them, as to have been classed by entomologists, not simply as a different species, but as a distinct genus. These new husbandless females are altogether destitute of wings, no nuptial flight being called for, and have hence received the name of *Biorhiza aptera*.

Thus, between these two distinct and well-marked forms, we have, as you will see, a conspicuous case of heterogenesis, or alternation of generations—the bisexual giving rise to the agamic, the agamic, in its turn, to the bisexual.[15]

Let us now follow one of these agamic females, and see the kind of work she is called upon to do. Eating her way through the hard, subterranean gall in which she has passed her successive metamorphoses, she struggles forward through the intervening ground, creeping upwards to the now leafless branches of the oak. Here, by a dexterous use of her terebra, preparatory to ovipositing, she makes a transverse cut across the axis of a winter bud, above the circlet of

[15] In certain *Lepidoptera* (Psychidæ and Tineidæ) parthenogenesis appears to be a normal process; indeed, so far as known, the only process, for of some species the males have never been found. (Herbert Spencer's "Biology," vol. i., p. 215.) This agrees with *C. hollari* and other of the "Cynipidæ."

scales, so as completely to separate the cone-like apex with its appendages. In the space thus prepared (for the severed cone is still retained *in situ* by the enclosing scales), a variable number of eggs is deposited; sometimes, it may be, but a dozen or two; at others, as many as 200 to 270 or more. It is not altogether without wonder that one realises the possibility of a lodgment being found for so many eggs in so small a space, a space not one-fifth of an inch in transverse diameter across the bud, and less than one-fiftieth of an inch in depth.[16]

Each egg consists of a white, semi-transparent, pyriform body, with a long, silvery filament or pedicel at its smaller end. To the base of the detached cone, the distal ends of the several egg-pedicels are, as a rule, anchored, so as to leave the ova pendent, with their broad ends downwards, towards the fixed surface of the cut axis.

As winter passes away, and slumbering vegetation begins to feel the stir of new activities, a further phase in the life-history of the gall and the gall-insect commences. Fed by streams of nutritive sap which now circulate through the tree, the young, uninjured buds begin to swell, and the green axes to elongate.

With those, however, which have been severed by the cynips, another and a different result takes place. Should ova, notwithstanding the incision, fail to be deposited, or, if laid, perish during the winter, no growth, normal or abnormal, takes place from the divided axis. This remains brown, dry, and inactive. But if, on the other hand, healthy ova are present, and these, in due course, hatch out their living embryos, then, by the action of these upon the dormant tissues, new and peculiar powers of growth are manifested in the cut axis—powers which result in the production, not of a normal branch, but of an abnormal, tumour-like gall.

Cognate facts with regard to the galls of *Cecidomya verbasci*, found on the stamens of the Figwort and hoary Mullein (*Scrophularia canina* and *Verbascum pulverulentum*), have been noted by M. Leon Dufour.[17]

"From meteorological influences or other little-understood cause, it happens at times, he says, that the larva of this insect dies soon after leaving the egg. Then the parts in course of enlargement tend to recover; the fundamental excitement, which would be continued by the suction of the embryo, stops and fades; the swollen tissues (subjected again to normal physiological laws) contract and shrivel; the sap loses its morbid exuberance, resumes its normal course, and at last, though slowly, the stamens re-enter upon their generative functions, while the lobes of the corolla spread out and display themselves, and, in the end, recover their bright and glowing colour. Under other circumstances, where the death of the larva thus ensues—the

[16] In the ovaries of *Biorhiza aptera*, which gives rise to *Teras terminalis*, I have found 1,326 ova—in one instance as many as 1,570—the whole length of the abdomen being less than one-eighth of an inch, that of *Teras terminalis* only half as much.

[17] *Ann. des Sci. Nat.*, vol. I., 1846.

efforts of nature proving powerless to remedy the pathological turgescence—a veritable atrophy ensues, the stamens wither and the resiant gall languishes and dies."

The mode by which, in the case of the oak-apple, the essential changes are carried into effect and the development of the gall completed, is worthy our attentive study. I have already mentioned that, in ovipositing, the parent cynips so disposes of her eggs that, for the most part, they lie with their broad ends downwards towards the fixed surface of the cut axis. As spring advances, this surface becomes moistened by the rising sap; and, influenced by the process, the first range of pendent eggs is brought into direct and immediate relations with it.

About this time the embryo-larva hatches from the egg; that is, it perforates, by means of a dental apparatus already developed, the larger end of the shell, so as to come into contact with the moistened but still dormant tissue of the axis.

In this state of things new and important phenomena are exhibited. Under the stimulus of mechanical irritation, or, it may be, of chemical changes set up in the juices of the plant by a specific exudation from the embryonic larvæ, many of the cells about the woody axis become endowed with active powers of reproduction. Cell-multiplication thereupon ensues, and the formative tissue, insinuating itself between the anchored ova, gradually surrounds the escaping embryos, until each, embedded in the growing mass, is left sole occupant of its separate chamber.

In this process, the lowest eggs, or those with longest petioles, are, of course, the first to be overtaken, and, as a necessary consequence, their liberated larvæ occupy a place nearest the base of the growing gall. Others in turn are successively reached, until, finally, every embryo being embedded, the cellular mass fuses together, and presents to us the characteristic features of the oak-apple gall.

Here, then, we have a series of facts, both positive and negative, which point to the action of the embryo, and not to the deposit of a special virus by the parent cynips, as the direct and essential agent in the production of the gall. This agent, as will be seen, carries with it all the elements of a *vera causa*. It is, so far as my observations go, invariably present whenever an oak gall appears; and, though the exact mode of its operation is, for the present, undetermined, it may not unfairly be regarded as sufficient for the effect produced. This is not so with respect to the deposit of a specific virus. Many of the gall-producing insects and acaridæ have neither terebra nor poison gland; and yet, in presence of living ova, hypertrophy ensues, and a veritable gall appears.

Granting, for the sake of illustration, the existence and potency of such virus, ought we not, in this case, to expect that, even in the absence of egg or living larvæ, the normal energies of the fluid would be exerted, and, in the end, a gall—destitute though it might be of

proper occupants—of necessity result? In my long experience, however, no facts confirmatory of this view have been discovered; nor is it probable that, under any conditions, such barren galls exist. Are we not then justified in discarding the hypothesis of a specific virus deposited by the parent cynips, and in attributing to the activities of the living embryos, combined, of course, with the normal forces of the plant, the genesis and metamorphosis of the gall?

Dr. Ransom, in his address as President of the Section of Medicine at the recent meeting of the British Medical Association at Nottingham,[18] defined a gall as a local hyperplasia due to the reaction of living cells to irritation. There were no grounds, he said, for adhering to the view, once held, that the parent insect deposits a virus with the egg. No galls were known to be due to any single act or impulse of any kind. The character of the reaction varied mainly with the irritant, but also, in a much less degree, with the tissues irritated. The irritant might inhere in the embryo *in ovo*, or in the free-hatched larva, or in both, or perhaps in an adult gall-mite, or in a mycelium, or in other more imperfectly understood organisms, animal or vegetal, if living; and although he granted the possibility that it was sometimes mechanical or physical, yet he thought it more probably always in some organic liquid, chemical substance, not very diffusible, produced in small quantities either continuously or at short intervals during a part of the life and development of the parasite, and having a different composition in each species.

Resting, as I think we may now do, on this solution of the problem, let us pass from the further consideration of the gall, to that of the gall-producing insect and its parasites[19]; and here we are met by one of the most curious episodes of entomological science, with a condition of things which, if not proved to be true, might well be regarded as a mere day-dream of the imagination. Nothing, in the prosecution of studies such as these, is more calculated to arrest attention or excite astonishment than the strict balance which is preserved in Nature among the various orders of animated beings. This is especially so with regard to the Insecta. Countless millions of insect-forms continually make their appearance upon the stage of existence, millions equally innumerable as constantly perish—"Comme si" (as Leon Dufour has said), "Comme si, dans le but des harmonies de la Nature, une loi de destruction devait contre balancer une loi de production"—as if the end of the harmonies of creation was, that a law of destruction should counterbalance a law of production—as if Nature, prodigal of her resources, created only to destroy; bent, as one might believe, from one aspect of her dealings, upon securing the greatest

[18] See *Brit. Med. Journal*, July 30, 1892.

[19] I desire here to acknowledge my obligations to E. A. Fitch, Esq., F.L.S., Maldon, Essex (one of the joint editors of the "Entomologist"), for the trouble he has from time to time kindly taken in identifying and naming for me the various insects resulting from oak and other galls.

happiness to the greatest number; while, from another, she would seem but to have set her heart upon devising and providing means by which to torture and destroy the sentient creatures she has made.

In this way, season by season, the greater part of the larvæ of insects, multitudinous often as the plague-flies of Egypt, are decimated by other larvæ, inquiline or parasitic —

"The unbidden crew of graceless guests."—*Virgil.*

which live upon their juices, or so rob them of their food, that they die of atrophy and inanition.

To realise this more fully, let us follow some phases of the life-history of *Cynips kollari*, the maker of the marble nut-gall.

And here, at the outset of our enquiry, we are met by the fact that no male member of this species has yet been discovered, the entire genus (in its present more restricted sense)[20] is, in fact, believed by Hartig and other competent observers[21] to be parthenogenetic or agamic. Whether this is so or not remains, perhaps, an open question. The evidence in its support is wholly negative; and though hundreds of thousands of galls of *Cynips kollari* have been examined by ourselves and others with no result but that of yielding females, the male element may nevertheless exist, and sooner or later be found, as in the case of the oak-apple and other now well-established instances, in a gall, differing in character, in date of maturity, and in position, from that in which the female appears.

Following, then, as we may now do, one of these presumably agamic females, we find her in due season puncturing the young buds of the oak, and depositing in each a single egg.

This action, apparently so simple, becomes, as we have seen, the initial factor in a series of organic perturbations, which result in the production of a gall of a persistent and determinate character. Were it not for our experience to the contrary, we might well believe that a home so strongly fortified as this is, and so lavishly provisioned, would, under all conditions of attack, hold out against invaders. This, however, is far from being so. The cynips larva, though hidden away in the very centre of the gall, is yet amenable to the incursions, the attacks, of numerous cruel enemies, for whom its property and its life become the necessary conditions of existence.

Of these, two well-marked groups especially invite attention. These are known as *Inquilines* and *Parasites*—the one phytophagous in habit, living on the vegetable tissues of the gall; the other entomophagous, or insect-eating, preying on the juices of the living host, which in this case feeds but to nourish the wolf within.

Early in the development of the gall and the gall-insect, these parasitic and inquiline enemies are alert and active. Guided by a marvellous instinct, by a perfection it may be of smell, or subtlety of

[20] This genus comprises more than 50 species.

[21] *See* Beyerinck, "Beobachtungen über die ersten entwicklungsphasen einiger Cynipidengallen," p. 139 (Amsterdam, 1882).

bearing, one of these entomophagous parasites (of which a type specimen may be seen in *Callimome regius*), discovers that a larva, condemned to become the living prey of her progeny, is at a sufficient distance from the surface of the gall to be reached by means of her fine abdominal terebra or ovipositor, with which she accordingly proceeds to puncture the gall, and on the body of the contained larva to deposit a single egg.[22]

In due course this hatches out its living embryo, the direct enemy of the legitimate possessor of the gall. This, feeding on the life-blood of its host, grows into a well-fed larva. The larva, in its turn, becomes a pupa, and the pupa an imago. Then, plying its mandibulate jaws, the mature insect proceeds to eat its way through the woody substance of the gall; and, clad in livery of golden sheen, with bright and iridescent wings, enters upon a new stage of existence; and, strong in the instincts of its race, begins, as its parents did of old, its insidious career of brigandage and death.

Nor are the dangers of the legitimate possessor of the gall limited by the operations of these sanguinary parasites. No sooner have the galls begun to grow, and the normal larvæ to feed upon the nutritive stores with which they are provisioned, than another group of enemies —near relatives of the Cynipidæ—are introduced, unbidden, into their dwelling-place, consume their food-substance, and shorten their lives. These are the phytophagous inquilines, which, pauper-like, avail themselves of others' labours, and live at their expense. Taught by an innate faculty that the home of the cradled cynips is furnished with provisions exactly suited to the early requirements of her own offspring, the vegetable-feeding *Synergus* usurps the well-stored sanctuary, and introduces there, not a single egg, as in the case of the *Callimome*, but a dozen or more, whence issues, in due course, a tribe of greedy larvæ that—to quote M. Leon Dufour—"vont realizer le '*sic vos non vobis*' de Virgile."

Thus the normal tenant of the gall, if not fortunate enough to escape, finds itself in this most dire dilemma—either to be eaten alive by its direct parasite, the *Callimome*; or to die of starvation from the action of its enforced messmate, the *Synergus*.

Nor are the ends of Nature yet fulfilled. The Inquilines, battening on their stolen viands, suffer, like the rightful owner, from parasitic enemies, which, consuming the invaders, become, in rough and barbarous way, the avengers of the dispossessed and suffering aborigines.

But here, again, as if to punish wrong, and work retributive justice, these parasites themselves are preyed upon by other parasites; losing thus in turn their own lives, as they before had sacrificed the lives of others. This is due, in great part, to the action of a new set of spiculiferous parasites, lower in the scale of creation than the

[22] See M. Leon Dufour, *Ann. des Sci. Nat.*, 3rd series, Zool., vol. I.

Chalcididæ, and which include in their number some of the most minute and slightly-formed of all the Hymenoptera. These are the Proctotrupidæ, several of which, belonging to the genus *Ceraphron*, are connected with the oak-apple, where I have found them in all stages of their development. Unlike the *Callimome*, these *Ceraphrons* do not lay a single egg on the *outside* of their host, but, on the contrary, deposit a number *within* its body. Here the contained eggs are shortly hatched, and the resulting grubs begin to devour the fatty substance of the attacked larva, until, in its dry and inflated skin, they find a comfortable, cocoon-like home in which to pass their further metamorphoses.

Perhaps, in their case, as in that of their predecessors the Callimomes, other families of entomophagous parasites attack and thin the ranks of the Proctotrupidæ, and in so doing maintain, though, as it would seem, under conditions of more than Bulgarian atrocity, the necessary balance of organic life.

I only ask, in presence of such facts as these, that amiable, but, as I venture to think, mistaken "Zoophilists," when they declaim against the cruelties of scientific investigators, and seek by penal legislation to check the progress of human knowledge, should bear in mind the actual course of Nature's dealings, and believe that in this, as in other things, Man, like the Creator,

> "From seeming evil still educes good,
> And better thence again, and better still
> In infinite progression."

G. B. Rothera.

VI.

Desert or Steppe Conditions in Britain: a Study in Newer Tertiary Geology.

THOSE who, without taking an active part in geological research or controversy, attempt to follow the latest results of the science, must often be struck by the curious way in which old theories fade and disappear, without being directly attacked. Such a fate seems to have overtaken the Diluvial Theory, notwithstanding certain recent attempts to revive it; we would now draw attention to the crippled state of the allied theory of the former existence of a "Pluvial Period." That a period of somewhat heavier rainfall may have existed during some part of newer Tertiary times, we are in no way concerned to deny; but as a question of evidence it is noteworthy that all the facts formerly relied on can now be shown to bear a quite different interpretation, and that the new facts accumulated during recent years tend to demonstrate the former occurrence, in place of the supposed Pluvial Period, of one or more periods of excessive drought.

Geological text-books still teach that the wide sheets of gravel found in the river valleys of the southern half of England, where only sands and clays are now deposited, point to a former excessive rainfall. Some of them even still suggest that the rivers once filled the wide valleys from bluff to bluff. The writers apparently have never tried to calculate how much water would be needed to fill these sloping valleys, and are unaware that a pluvial period with a rainfall of double or treble the present amount would be quite insufficient, and that at least one hundred times the present fall would in many cases be needed. The existence of these sheets of gravel, and the constant occurrence of deeply excavated valleys in regions where no streams now exist, constituted the evidence on which was founded the well-known Pluvial Theory.

Let us, however, try to put on one side all preconceived views and examine afresh the evidence. We will take first the fossils. After comparing a number of collections from various old river-deposits, and working out each one separately, it is possible to arrive at a clearer idea of the climatic conditions that prevailed than could be obtained by any mere examination of museum specimens or of lists of species. Among the more striking results of the study is the

extreme rarity of fish remains, the comparative scarcity of truly aquatic mollusca, and the infrequent occurrence of perennial aquatic plants. The amphibious mollusca are common enough, especially such forms as can survive in the mud beneath a dried-up crust. With these amphibious forms are associated several continental species now rare or entirely extinct in Britain. We find, for instance, that one of the most abundant shells is usually the *Succinea oblonga*, now so scarce in Britain. We have also *Hydrobia marginata*, *Corbicula fluminalis*, *Unio littoralis*, and several species of *Helix*, all these forms having now disappeared from our islands. It might be thought at first sight that the assemblage pointed to a warmer climate; but a careful analysis of the list does not support this view, and the only character which the species possess in common is that all of them now live in sunnier and drier regions than ours, though not necessarily in warmer ones. The Pleistocene mammals found in Britain also point in the same direction, though not so decidedly, for many of them belong to extinct species whose former habitat is unknown to us. We notice, however, two or three species, such as the Saiga antelope and certain of the small rodents, which belong distinctly to the desert fauna of Central Asia. When the corresponding strata are traced eastward into Central Europe the evidence becomes much stronger, for Professor Nehring has discovered in Germany a mammalian fauna corresponding closely with that now inhabiting the Central Asian steppes.

If the loess of Central Europe be examined, it is found rarely to contain aquatic mollusca, but to yield in myriads such species as love sand-dunes and dust. The loess, as Baron von Richthofen has shown, is a desert-deposit such as now drifts before the winds in the dry regions of Central Asia. Thick dust-deposits like the loess do not extend so far to the west as Britain, where the climate must always have been comparatively moist owing to the proximity of the ocean; but in England there are inland deposits of blown sand, where sand does not now drift owing to the growth of vegetation, and these also probably point to drought. In the composition of our surface-soils we have also, I believe, evidence of the former wafting to and fro of fine material, which could not be obtained from the weathering of the underlying rock. The soil on our chalk Downs, for instance, is always full of small rounded grains of quartz, which cannot be derived from the underlying strata, for the upper and middle Chalk in this country do not yield anything but carbonate of lime, flint, and a little fine clay. The loess period seems to have affected Britain, though not so strongly as it did Central Europe.

The erosion of the valleys, undoubtedly exceptionally rapid during certain parts of the Pleistocene Period, and the formation of enormous sheets of valley gravel, remain, therefore, the sole evidence of the former existence of a Pluvial Period. Let us re-examine this evidence from a new point of view, and see what would be the necessary

consequences of the existence of a cold dry epoch like that indicated by the fossils. In the first place, with a clear sky the winter must have been much colder, and all pervious strata would become frozen and impervious to a considerable depth. Any rain falling before the thick frozen layer had melted could not sink in, but would immediately drain off the surface, carrying away with it a thin layer of rock shattered by the frost. Thus, the chalk Downs, which now yield perennial springs, even after a drought such as that of the past summer, would then yield no springs, for the rain could not penetrate. When the surface was frozen, the heaviest fall of rain would be entirely thrown off these steep slopes in a few hours, and the Downs would become channelled by ravines containing impersistent torrents, such as we now only associate with mountain regions where the rocks are impervious. As soon as the torrents reached flatter open country, the material brought down would be deposited in fan-shaped deltas, like that on which Chichester is built.

Implements made by man are not uncommonly to be met with beneath such gravel deposits, and these discoveries are often, but to me unaccountably, taken to prove the former existence of rivers at spots now high up the slopes of hills, or even near the highest points of nearly level plateaux. From the occurrence of stratified gravels above the implements in such situations, it is further argued that since Palæolithic times deep valleys have been cut out, and what were formerly the alluvial flats, have now become outliers of implement-bearing gravel capping isolated hills. Many writers even reason as though Palæolithic man were an amphibious animal, unable to live far from a river, and absolutely proving from the abundance of his weapons at certain spots, that a river must once have existed in the immediate neighbourhood. If such were the case, Palæolithic man must have been very different from existing savage races of the arctic and temperate regions. An overwhelming desire for an abundant supply of water is not a marked characteristic of these, and, as a rule, a small quantity for drinking purposes is all that they require. We have evidence that Palæolithic man hunted the big game that was then so plentiful; but we have no evidence in this country that he was much of a fisherman.

The conditions under which the implements are found, often scattered over deeply-buried ancient land-surfaces, and associated with hearths and other remains, which prove that on that spot was the site of an ancient settlement,[1] are to me more suggestive of sudden flood action than of ordinary rivers. Is it not possible that Palæolithic man may have lived on the existing hills and plateaux, and that the gravel beneath which his remains are buried is merely a local flood gravel deposited in any hollow over the frozen slopes, and not necessarily proving the existence of any river at such heights?

[1] See Worthington G. Smith, ref. no. 6.

The occurrence in chalk Downs, and in hills of similar porous strata, of deep valleys in which no streams now exist even after the heaviest rain, and the existence of enormous sheets of irregularly stratified or rubbly gravels in the lowlands adjoining, instead, therefore, of constituting the strongest evidence in favour of the existence of a Pluvial Period, seem more probably, like the fossils, to point to the occurrence of cold desert periods, when the rainfall, though small, could act much more energetically as a denuding agent. Under such conditions it is possible to understand the correlation of a fauna poor in truly aquatic species with deposits indicating violent floods. The absence of subterranean drainage would not only cause violent floods, even with a small rainfall, but would lead to the disappearance of the springs and consequently of all perennial streams except such as drained a large area. This, again, would tend to emphasise the desert character of the fauna.

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CLEMENT REID.

VII.

The Genesis of Mountain Ranges.

THE Origin of Mountain Ranges is one of the most interesting problems in Geology. It is only of late years that anything worthy of being called a *theory* of their origin has been formulated. This is not surprising, because their internal structure as well as their external form had to be first known. American Geologists have had, perhaps, the best opportunities of engaging in this enticing study, hence the interest it has evoked has been greater on the other side of the Atlantic than here.

Among those who have given form to our ideas on the subject Professor Joseph Le Conte has been one of the foremost. It is therefore an important event when, as President of the American Association for the Advancement of Science, he chooses for his theme a review of what is already known of mountain structure, and an analysis and criticism of the leading theories that have been advanced to account for the origin of mountain ranges.

Professor Le Conte commences by drawing a distinction between what he calls a *Formal Theory* and a *Causal or Physical Theory*. The first must precede the latter, and while the formal theory according to his view is well advanced, the physical is in a very "chaotic state."

The *Formal Theory* is summarised as follows:—(1) "Mountain ranges while in preparation for future birth, were marginal sea-bottoms receiving abundant sediment from an adjacent land-mass and slowly subsiding under the increasing weight. (2) They were at first formed, and continued for a time to grow, by lateral pressure crushing and folding the strata together horizontally and swelling them up vertically along a certain line of easiest yielding. (3) That this line of easiest yielding is determined by the hydrothermal softening of the earth's crust along the line of thickest sedimentation. (4) That this line, by softening, becomes also the line of greatest metamorphism, and by yielding, the line of greatest folding and greatest elevation; but (5) when the softening is very great, sometimes the harder lateral strata are jammed in under the crest, giving rise to fan-structure, in which case the most complex foldings may be near, but not at the crest. Finally (6), the mountains thus formed will be asymmetric, because the sedimentary cylinder-lenses from which they originated were asymmetric."

Several American examples illustrating these views were then given, and it was shown that eruptive phenomena, faults, mineral veins, earthquakes, and other minor phenomena associated with mountains, are well explained by them. To quote Professor Le Conte:—"Leaving out the monoclinal type, which seems to belong to a different category, all phenomena, major and minor, of structure and of occurrences, connected with mountains, are well explained by the theory of lateral pressure acting on lines of thick sediments accumulated on marginal sea-bottoms and softened by invasion of interior heat. This view is, therefore, satisfactory as far as it goes, and brings order out of the chaos of mountain phenomena. It has successfully directed geological investigation in the past, and will continue to do so in the future."[1]

This is a statement of the theory with which Professor Le Conte's name has been associated, and it is one that is distinctly intelligible. It, however, contains in itself a proof of the difficulty of drawing a marked line between the *Formal* and *Causal*. The "*hydrothermal* softening of the earth's crust along the line of thickest sedimentation" is a *physical* assumption to account for certain phenomena. It is also a further *physical* inference that "*lateral pressure* acting on lines of thick sediments, accumulated on marginal sea-bottoms, and softened by the invasion of interior heat," accounts for all mountain structure excepting the monoclinal type.

I am not either affirming or denying the truth of these statements—into which I shall examine further on, but simply wish to show that the confusion of the two modes of thought complained of by Le Conte in other investigators has not been altogether avoided by himself; this being the case, it is only reasonable to ask if such a hard and fast division be possible.

Professor Le Conte next deals with *Physical* or *Causal Theories*, which he seems to limit to an enquiry as to the cause of the lateral pressure with which he builds his mountain ranges.

First he describes what is known as the *Contraction Theory*, which refers all lateral pressure of the crust to the *interior contraction of the earth*: a theory which is commonly known as the "Shrunk Apple Theory." It is this: The nucleus of the earth, represented by the interior fruit of the apple, shrinks in bulk, while the crust remaining of the same size adjusts itself to the contracting globe by folding and wrinkling, like the skin of a shrunk apple. It is difficult to understand, if the physical theory of the hydrothermal softening of the crust can be included as part of a formal theory of mountain building, why the contraction theory providing lateral pressure should be put in another category. To my mind, the contraction theory is as formal as the theory of mountain building we are considering, especially as at first understood; for it was founded upon

[1] *Nature*, October 5, 1893, pp. 551-2.

an assumption afterwards proved incorrect in several important particulars. It is impossible to state any theory without involving cause and effect, and what is not observation in these theories is mostly physics.

After explaining the contraction theory, Professor Le Conte gives a summary of the objections to it, and a formidable list it is—too extensive, indeed, to enumerate in the space I limit myself to. Many of these difficulties have been dealt with in my "Origin of Mountain Ranges," especially the most salient of them, based on the existence of a level-of-no-strain at no great depth in the crust of the globe, above which the crust is in compression and below in tension. Le Conte evidently attaches great importance to this principle, and he may well do so, as it clearly follows that the bulk of the crust in compression is quite inadequate to account for the irregularities of the earth's surface.

Reade's Expansion Theories are next dealt with, and Professor Le Conte honours me with a generally fair formal statement of my views. He feels, however, that he does not thoroughly grasp my meaning in several important particulars, more especially in relation to the cumulative effects of recurrent expansion. It would savour of self-conceit to repeat my views here, indeed it is unnecessary, as the theories and investigations can be perused in their original extended form, by which a more just conception of their value, whatever that may eventually prove to be, can be formed.

After stating the objections to Reade's theories, which will be dealt with further on, *Dutton's Isostatic Theory* and *Reyer's Gliding Theory* are explained and dismissed—Le Conte finally, whether we may agree with him in whole, or part, or entirely dissent, concludes a very able address, written in the clear and charming style peculiar to himself, with the following sentence: "After this rapid discussion of alternative theories, in which we have found them all untenable, we return again to the contraction theory, not, indeed, with our old confidence, but with the conviction that it is even yet the best working hypothesis we have."

Before criticising the theories of this able investigator and writer, many of which are quite individual and original, I think it will be best to dwell first upon those general principles in which we are in agreement. They are these: (1) The leading principle, and one first stated in America by Professor James Hall, is that mountain ranges are built up out of immense thicknesses of sediment; (2) that there is a relation of cause and effect between sedimentation and mountain building which Le Conte attempts to explain in one way consistent with the Contraction Theory, and I in another in more direct relation to my Expansion Theory. This is a very important agreement, as there are still some, though in a minority, who are not prepared to admit either the fact or the relation. It is nearly seven years since I published my work, and the additions to our knowledge, too numerous to mention here, all point to the fact that Mountain Ranges are built

out of thick sediments. To go no further than our own island, Sir Archibald Geikie has clearly shown that the range of mountains in the Highlands of Scotland, the remains of which the Geological Survey has been investigating for many years, were built up of sediments many thousands of feet thick. It is the same, so far as is known, with all the existing great mountain ranges of the world, such as the Alps and Appalachians, the sediments of which they are composed being in many cases estimated at from 8 to 10 miles thick; (3) the accumulation of sediment, together with a consequent sinking of the sea-bottom, leads to a rise of the isogeotherms which first affect the strata of the crust on which the sediments repose, and next the sediments themselves.

These are the three leading principles in which we agree, but there are also minor points in which our ideas run parallel or partially so, but it will hardly be necessary to state them here.

But while our main principles so far run abreast, I attribute the elevation and folding of the chains in their initial stages, not to the contraction of the earth, but to the internal expansion caused by this heating of the sediments and the crust of the earth upon which they repose. These effects are intensified and continued until the building of the range is complete by other related and cumulative causes. It thus becomes a case in my view of *direct* cause and effect. Professor Le Conte apparently thinks that this expansion will have no effect at all in the way of elevation or folding, relying upon a criticism of Davison, which I shall presently deal with in the form stated in Le Conte's own words. The whole cause of both the folding and elevation is assumed to be in Le Conte's *Formal Theory* lateral pressure, and in his *Physical Theory* this lateral pressure is assumed to be due to the contraction of the earth. The hydrothermal softening of the earth's crust under the line of thickest sediments, in his view, determines the concentration of this lateral pressure in the localities in which it takes place.

Granting all the postulates involved, we here undoubtedly have a machinery which looks effective for mountain-building; but it is one thing to make a formal statement which is sufficient, and another to make its assumptions square with physical facts.

The theory was undoubtedly in better case when geologists were content to look upon the earth as in two simple conditions, namely, possessing an *unshrinkable crust* of unknown thickness—that is, of any thickness required for theoretical purposes—entirely in compression, and a *nucleus* whose function is to shrink. Such theories, even if afterwards proved to be wrong, are of value in focussing thought; they are often stepping-stones to the truth. When, however, we come to apply quantitative analysis, with the help of the physical knowledge of the day, to the Contraction Theory the problem assumes a different complexion. We find that the shrinking nucleus is but a shrinking shell, in thickness some one-twentieth part of the earth's

radius, while the underlying real nucleus is a heated globe of some 7,500 miles in diameter, which has not parted with its original heat or shrunk at all. The crust in compression is also found to be only a few miles thick, the compression being most active at the surface, gradually diminishing to nothing at the level-of-no-strain. These conditions of strain and stress in a cooling globe being pretty generally accepted by physicists of the day, Le Conte seeks to eke out the amount of contraction required by the theory in other ways. They are, however, so far, but formal assumptions having very little foundation in physical facts, and may fairly be dismissed until some better proof of their existence is forthcoming.

Let us, however, grant the lateral movement required by the formal theory, and consider how such machinery would be likely to work.

It is difficult to conceive in what way the lateral pressure of an enormous shell, 8,000 miles in diameter, could be concentrated in one or two places by any softening of strata under incipient mountain ranges, for it would involve a torsion, in varying degrees, of the whole crust, and a shearing over the whole area of the nucleus on which it rests; but this difficulty has been already dealt with by Dutton, so we will not dwell upon it. When, however, we come to consider that the application of the force is *external* to the lenticular plate of sediments to be acted upon, our difficulties increase. A softening of the crust along a certain axis might determine an elevation and folding in its immediate vicinity, but it would hardly shade off into those regular folds over a great area of country which are nearly always present where former sedimentation and upheaval has taken place. This would involve a refined gradation of softening that is not likely to occur in nature. I have also in my work dwelt on the fact that the pressure which has produced mountain ranges must have been internal and *centripetal*, and have illustrated this idea by reference to the domed form of anticlinals, varying from almost a segment of a sphere through an ellipsoid to an inverted canoe-form with their accompanying similarly-shaped but inverted synclinals.

It is, perhaps, only my individual opinion, but it seems to me improbable that the earth's crust could, by hydrothermal softening, be led into the symmetrical structures which characterise its surface configuration.

If, however, we look upon the pressure as *internal* to the folded area, the difficulty vanishes. A heating of the strata and crust would produce a state of compression acting throughout the mass from atom to atom, and greatest where the heat is greatest. This, it appears to me, is what is wanted to produce a folded mountain chain, and it will, as I have attempted to show in detail in my work and various papers, originate those great features of folds, foothills, and granitic and gneissic axes distinguishing all great mountain chains.

But, say my critics, your theory does not give lateral movement sufficient, we want much more; to which I have replied and sustained the affirmation by figures, that it gives more lateral movement than any of the other contending theories. I have also shown, and this view has been endorsed by no less an authority than Stefani,[2] that the amount of lateral movement required for mountain building is very much less than what is given by measuring the folds of mountain ranges and comparing their length with the base line of the chain. The assumption that the difference represents the amount of movement that has taken place is fallacious, because the reproduced arches never existed, and the strata have been lengthened by rolling out.

The subject of mountain building is so intricate and complex, and has so many aspects, that it would take much more space than is at my disposal to do justice to it; but before concluding these observations it will be necessary for me to reply to the objection considered by Le Conte to be fatal to my theory. He says, "But the fatal objection is that brought forward by Davison. It is this: sedimentation cannot, of course, increase the sum of heat in the earth. Therefore the increased heat of the sediments, by rise of isogeotherms, must be taken from somewhere else. Is it taken from below? Then the radius below must contract as much as the sediments expand, and therefore there will be no elevation. Is it taken from the containing sides? Then the sides must lose as much as the sediments gain, and therefore must contract and make room for the lateral expansion, and therefore there would be no folding and no elevation. I do not see any escape from this objection."

This, in my view, is a *formal* objection, and *formal* only. The criticism is underlaid by the profound misconception that the sediments *abstract* heat from the earth. So far from this being the case, *they conserve it and retain heat that would otherwise be wasted into space*. They act as a blanket or top-coat would do to the human body, as pointed out by Herschell and Babbage. The radial contraction of the earth is less under the blanketing sediments than under the other portions of the crust. Looked at in even this partial manner, there is practically no difference between a radial expansion of a part of the globe under the sediments—the only expansion apparently contemplated by Le Conte as effective—and a radial contraction of the surrounding crust of the earth. It is a question of *relative* movement and of *differential*, not absolute, heating. Increase of the sum of heat in the earth is entirely beside the mark. The answer is therefore simple and conclusive. The sediments neither abstract heat from below nor from the sides, they conserve and utilise what would otherwise be wasted. The flow of heat from below could not be quickened by the laying down of sediments, unless they were at a lower temperature than the surface of the earth's crust on which they were deposited.

[2] "Le Pieghe delle Alpi Apuane," p. 110, Firenze, 1889.

To be effective, the difference of temperature would have to be great —something quite impossible.

But it is not on radial expansion or contraction that the theory of the origin of mountain ranges bearing my name hinges. Reasoners on such complex problems should have the conditions fully and accurately before them.

A well thought-out theory cannot be gaily dismissed in a few short formal sentences.

The horizontal extension of aggregate groups of sediments compared with their maximum vertical depth is enormous; it may be 200 times as great. Consider what would be the result of heating a slab from below measuring 200 feet at the sides and 1 foot thick in the thickest part. If it were bound at the sides in the way the sediments are bound on the solid crust of the earth, the effect would be to throw it into domicular folds distributed in relation to the differential heating and thickness of the slab at various points. Even if it were not so held, the differential heating—the temperature being most intense at the thickest parts—would still throw it into folds through the intense stresses and strains set up. This is what happens in my view by the heating of the sediments which afterwards become mountain ranges. It initiates the movement, liberating other forces which continue the building of the chain. I cannot pretend to go into the details here, as they are of considerable complexity, and must refer those who wish to pursue the subject to my work and the several papers I have at various times during the last seven years given to the scientific world.

I must, however, before concluding, say a word on cumulative recurrent expansion, as Professor Le Conte here finds my theory obscure. This seems the more strange to me, as I actually commenced my work by giving concrete examples of the effect of recurrent expansion due to alternate heating and cooling. The folds in this way induced in sheets of lead are given as good examples and illustrated by photographs. Anyone who cares to look for such folds can see them in any lead gutter, lead-lined bath, or lead-lined sink exposed to varying temperatures. It is one of the plumber's arts to provide as far as he can for this well-known effect of frequent minute expansions; but I have shown that such effects are not confined to so ductile a material as lead. Terra-cotta copings are frequently affected in a somewhat similar manner. They go on expanding for years by infinitesimal degrees, mostly in the heat of summer, until I have seen what is technically called a "ramp"—a portion of the coping curving down to a lower level—lifted from one to two inches from its bed. Not only so, but in some cases a time comes when the bending at this point becomes a fault at one of the joints (normal fault), the expansion still proceeding. I have had a very interesting letter and photograph from Mr. John D. Paul, F.G.S.,

Leicester, recording an independent observation of his own on a coping at Leicester, showing a similar result, which he attributed to alternate heating and cooling by ordinary changes of temperature. The reason why this sort of expansion shows so much in terra-cotta copings, is because the joints are made in cement, and the whole becomes a long bar, alternately exposed to the vertical rays of the sun and to the cold.

Finally, Professor Le Conte seems to think that if expansion produces mountain elevation, contraction should bring the strata back again to the same level. This is a very common criticism with my objectors, and is due to a misconception born of looking at things in the gross—and, if I may venture to say it—in too formal a manner. The reason why the strata on contraction do not go back to their original form and level I have explained over and over again. It is this, no force on earth can pull strata back that have once been folded. The material by every expansion, however small, is pushed forward, and accumulates mostly in folds, so that the mountain range is actually piled up, by transfer of material from one locality to another, just as much as if it had been done by means of navvies and wheelbarrows.

To give another illustration, landslips are of frequent occurrence, and have been seen, or their effects seen, by many. One of those effects is to push up a fold, or pile up any loose materials, such as a shore deposit, before it. One might as well expect this material to go back again, and spread itself out over its old bed if the landslip were removed, as to expect mountain strata to do it, whether piled up by horizontal or cubical expansion, or pushed up laterally in any other way.

If thinkers would not concentrate their minds upon *vertical* expansion to the exclusion of *horizontal and cubical*, they would, I think, find no difficulty in understanding one of the leading ideas of my theory. In consequence of the extended areas of the sediments compared to their vertical thicknesses, horizontal expansion is infinitely greater in its effect than vertical.

When, however, all is said, there is no gainsaying the fact that the Origin of Mountain Ranges is a subject involving intricate reasoning, and cannot be disposed of by formal statements, however clear; for their clearness frequently arises from the exclusion of conditions which ought to be considered.

My object in making these explanations and criticisms is entirely to further the cause of a science which I have deeply at heart, and which, for its progress, is dependent on the labour of many. No one can rejoice more than I do at the valuable contribution towards the elucidation of so difficult a subject as the Genesis of Mountain Ranges with which the veteran Le Conte has favoured us.

T. Mellard Reade.

VIII.

Indexes to Botanical and Zoological Nomenclature.

THE compilation of indexes by means of which a student can find an accurate reference to the first description of a generic or specific name, entails a devotion to the subject known to but very few. To deliberately determine to spend some fifteen or twenty years of a lifetime in order that other students of science may be spared the time occupied in hunting up descriptions in more or less obscure publications, is a species of insanity that one could only wish were more common. The appearance of the first volume of Mr. Daydon Jackson's index to the genera and species of flowering plants, shows that it is possible to accomplish what appeared at first a hopelessly gigantic task, if one has the requisite energy, is provided with the funds, and is able to devote the greater part of his time to the undertaking.

In 1840–41 botanists were presented by Steudel with the "Nomenclator Botanicus," a list of plant names then supposed to be of specific value, together with their synonyms. In 1855 G. A. Pritzel published his "Iconum Botanicarum Index Locupletissimus," containing a reference to the first description of all the then known flowering plants and ferns, and he issued a second edition of this work in 1861; but botanical literature had grown to such an extraordinary extent by twenty years later that Darwin found to a worker like himself, living far from museums and libraries, that it was a most difficult task to get information as to new species and to the place of publication. He accordingly discussed the matter with Sir Joseph Hooker, who put the idea into form, and made arrangements for the manuscripts to be stored in the Herbarium at Kew. Darwin and his friend George Bentham agreed to supply the funds necessary to ensure the continuous working of the scheme, and Mr. Benjamin Daydon Jackson was chosen to carry it out. Mr. Jackson accordingly, in 1881, started the compilation, arranged for a special staff of clerks to assist him, has carried the work triumphantly through, and presented botanists with Volume I. (from A—Dend.) last month.[1] The successful termination of so grand a work

[1] "Index Kewensis plantarum phanerogamarum nomina et synonyma omnium generum et specierum a Linnaeo usque ad annum MDCCCLXXXV. complectens nomine recepto auctore patria unicuique plantae subjectis sumptibus beati Caroli Roberti Darwin ductu et consilio Josephi J. Hooker confecit B. Daydon Jackson." Fasciculus I. [Aa—Dendrobium]. 4to. Oxonii e prelo Clarendoniano. MDCCCXCIII.

must be a matter of supreme satisfaction to its compiler, and in congratulating Mr. Jackson, one congratulates also the botanical world on the immense labour saved to them in having the "Index" for reference.

While, however, giving Mr. Jackson the lion's share of the credit for this work, he himself would certainly be the last man to ignore the help of others. To the enthusiastic little band of clerks (despite the fact that they were paid for their work) a large amount of thanks are also due; while of his other helpers Sir Joseph Hooker has read the whole of the proofs and annotated and corrected the MS. as only one of his large experience could do. From the Kew authorities and from the staff of the British Museum (who have been supplied with proofs), Mr. Jackson has received invaluable assistance, and the same may be said of officials of the numerous public Herbaria, and many private collectors.

It is, perhaps, not too much to say that it is only in London that so gigantic a task could be successfully terminated in so comparatively short a time. The libraries at Kew, the collections at the British Museum, Bloomsbury (where are stored the books of Sir Joseph Banks), and the still more complete library of the Natural History Museum, present a series of botanical books equalled by no other city in the world; and these have all been fully drawn upon. Further, there are the collections of the Linnean and the Royal Societies, besides the rich stores of volumes in the hands of private individuals, all of which have been placed unreservedly at Mr. Jackson's disposal.

As a commencement of work, the generic names from Bentham and Hooker's "Genera Plantarum," were arranged in alphabetical order, and the names in Steudel's "Nomenclator," intercalated; each genus was placed in a separate wrapper, and housed in boxes with a fall-down front, so as to allow of easy reference and insertion of any new names that were indexed by the workers. These boxes of MS. ultimately reached the number of 168, and were arranged in pigeon holes.

The "Index" includes all names of flowering plants and ferns from 1735, the first edition of Linnæus's "Systema Naturæ," down to the end of 1885, and the estimated number of entries reaches 500,000.

The following are examples of Mr. Jackson's entries:—

Acnida rusocarpa, *Michx. Fl. Bor. Am.* ii. 234. t. 50.—Am. bor.

Beesha capitata, Munro, in Trans. Linn. Soc. xxvi. (1868) 145.—Madag.

Cyperus leptostachys, Vahl, ex Kunth, Enum. Pl. ii. 93=distans.

Printing began in the autumn of 1891, and has now reached the letter K, but owing to the extreme care bestowed on the proofs, it has only been possible to issue the first part at present. The book will be completed in four parts, and it is anticipated that the whole will be issued to the public by the end of 1894. One thousand copies will be published at a price of eight guineas nett (two guineas each part). It

is sincerely to be hoped that so monumental a work will not be allowed to stand still, but that it will be kept up-to-date in manuscript, both at Kew and at the British Museum. Indeed, nowadays, it is the obvious duty of a public institution to save the valuable time of its own staff by arranging for a special department where information of this kind can be obtained for the asking.

It is, perhaps, in a general review of this kind, somewhat carping to criticise so great a result, but one is impelled to notice the very serious oversight of the omission of dates in all but those entries relating to serial publications. It would have conduced to far greater perfection had it been realised that the date of a generic or specific name is of considerable importance; so considerable is it, indeed, that it is perfectly incredible that it should have been left out. But Mr. Jackson can remedy this if he has still the energy and the means to do it, by issuing with volume 4, at a slightly extra cost, a complete list of the abbreviations used in alphabetical order, together with the date and a brief extension explanatory of them. We trust he will see his way to do this. For a criticism on the scientific aspect of the work, we need but refer to a masterly article from the pen of Mr. James Britten in the *Journal of Botany* for October; with this criticism we are quite in accord. The introduction of the word "Kewensis" strikes us as unnecessary, and we would like to have read Jackson and Hooker on the cover, instead of Hooker and Jackson, but this without the slightest disrespect to Sir Joseph Hooker.

To turn to Zoology: up to the time of Louis Agassiz, zoologists had practically no book to refer to that would assist them in discovering the literary whereabouts of a generic name. Gmelin, in his edition of Linnæus's "Systema Naturæ," 1788–1790, had published a complete index to the many thousand species described in that work, but it was reserved for Louis Agassiz to publish a "Nomenclator Zoologicus," in which he gave a full reference to every generic name he could find that had been used up to his day (33,000 entries). Nothing further was available beyond the lists published in the yearly volumes of the *Zoological Record* (vol. vii. and onwards), until 1873, when August Marschall published his "Nomenclator Zoologicus," a book containing much new matter, but, unfortunately, in many respects inaccurate. On October 9, 1879, a letter appeared in *Nature* from Samuel H. Scudder, containing a notice that the writer had arranged and collated previous lists of generic names, and asking assistance towards its completion. In 1882 subsequently appeared Scudder's "Nomenclator Zoologicus, an alphabetical list of all generic names that have been employed by naturalists for recent and fossil animals from the earliest times to the close of the year 1879." This work was divided into two parts; part 1 containing a full reference to all generic names not in Agassiz, Marschall, or the *Zoological Record*; and part 2, "Universal Index," an index giving the name, author, and date of every generic

name, so far as the compiler had collected, which had been used for an animal from the earliest times to about 1880. This book, which contained 80,000 references, though necessarily incomplete, was at once recognised as of extreme value, and one may say without exaggeration is absolutely indispensable to zoologists. Scudder has promised a supplemental list of generic names discovered to have been omitted, but this has not yet appeared.

None of these books, however, offered those advantages to the zoologist that Pritzel offered to the botanist. They dealt with generic names only, while Pritzel dealt with specific names. The example set by Mr. Jackson in botany was followed by Mr. Davies Sherborn, who, on 15 May, 1890, published a scheme in *Nature*, for an index to the genera and species of all known animals. With some slight modification of the scheme, work was commenced on July 1, 1890, and has steadily progressed ever since. Accommodation for the MS. was provided at the Natural History Museum by the Director and the Keepers of Zoology and Geology, and the MS. is available for reference in alphabetical order of genera to anyone who desires such information. The plan of compilation adopted by Mr. Sherborn differs somewhat from that of Mr. Jackson. Every species has a separate and distinct slip, and every reference is taken from the original source; a volume is systematically searched page by page, and every species extracted, so that once that volume has been indexed, the compiler never requires to see it again. The slips are all made in duplicate with transfer paper, one set being sorted up into the great alphabet of genera, and the other set tied up and put away under the author of the book.

By this method not only is one able to see the whole of one author's work collected together, but this second set of slips will remain clean and intact for the final sorting, if publication is ever reached. Synonymy has not been attempted in the strict sense of the word by Mr. Sherborn; that, in his opinion, is more the duty of a specialist, but every time a species-name has been placed in another genus a reference is given, so that when complete and arranged the searcher will be able to find the history of that species-name without trouble, through all its generic vicissitudes.

The following are examples of the entries:—

subterranea Anas, J. A. Scopoli, Annus i. 1769, p. 67.

vampyrus Pteropus (L.), C. Illiger, Prod. 1811, p. 118 [Vespertilio].

panthera Felis, J. C. D. Schreber, Säugth. iii., p. 384 (1777) and 586 (1777), pl. xcix. (1776).

Working alone, and with one small grant from the British Association, the compiler has already collected about 90,000 references, and as the books are taken as far as possible in order of date from the earlier to the later, a considerable number of the rarer and more obscure publications are already indexed and available. The references are taken from 1758, the date of the tenth edition of the

"Systema Naturæ" of Linnæus, that having been found the most convenient starting point and more in accord with the ideas of foreign as well as British naturalists. The system above described (the completion of one book at a time) is apparently an especially good one, for it permits the MS. to be always complete as far as it goes, and will enable any person, at any moment, to take up the work, should it unfortunately be found necessary.

A still more gigantic proposition has been advanced by the German Zoological Society, which proposes not only to form a catalogue of all described species, but to give a short diagnosis of each as well! The vastness of this compilation overwhelms one, but that indefatigable indexer, Professor Victor Carus thinks well of the scheme. This could not possibly be done by one man, and the notion is, that specialists in every branch of natural history might undertake some family or even genus alone. The final details of Professor Carus's scheme are unknown to us, a brief notice only having as yet appeared in a recent number of the *Zoologischer Anzeiger*.

IX.

The Wilds of South-East Africa.[1]

IT is not difficult to prophesy that this handsome volume will be a distinct and immediate success. The public have anxiously waited to hear what Mr. Selous had to say on the Mashunaland question, and they receive the information at the very nick of time. But it is to a far larger section than to the political public that this book will appeal, for the boy will welcome Mr. Selous' volume with its stirring adventures as a companion volume to his "Robinson Crusoe," while the man will read it with the pleasure derived from his knowledge that it is written by one who knows the lions, elephants, and other large game as perfectly as he knows his horse.

Mr. Selous left England in November, 1881, for the Cape, and travelling on horse by way of Kimberley reached Klerksdorp, sleeping sometimes in the open, sometimes in the houses of the Boers, according to the weather. Purchasing a waggon and a pair of oxen and taking with him several Matabili boys from Klerksdorp, the traveller started for the interior, passing through the Matabili country and reaching the junction of the Loangwa and Zambesi rivers on 22 June. Here a camp was made, and for the next six months Selous was hunting and travelling in a country where lions were always prowling about, though nowhere plentiful.

Returning to Klerksdorp to pack and despatch his collections to England, the traveller laid in a fresh stock of provisions and trading goods, and by May, 1883, had once more entered Mashunaland, and pitched his camp on the Manyami River. In July he made a start south for the Sabi River in search for the white rhinoceros (*R. simus*) and Lichtenstein's hartebeest (*Alcelaphus lichtensteini*), in the hope of adding some skeletons and skins to the collections of the British Museum. But he was unsuccessful, and in November he broke up his camp and turned to the south-west, reaching Matabililand in December, where after having a disturbance with Lo Bengula over

[1] TRAVEL AND ADVENTURE IN SOUTH-EAST AFRICA; being the narrative of the last eleven years spent by the author on the Zambesi and its tributaries; with an account of the colonisation of Mashunaland, and the progress of the gold industry in that country. By Frederick Courteney Selous. 8vo. Pp. xviii., 504, map, portrait, 22 plates and 35 illustrations in the text. London, 1893. Price 25s.

a hippopotamus, concerning which he was innocent, and for which he had to pay £60, he set out for the Transvaal.

Setting forth again in March, 1884, in company with W. M. Kerr, Selous decided not to hunt in Lo Bengula's country after the sorry treatment received at the king's hands, but to turn west after reaching Bulawayo, and spend his time in the northern parts of Khama's territory. Towards the end of June Mababi flat, N. of Lake Ngami, was reached, but as there was not much to be had in the way of game, the party returned to Horn's Vley, where elands, giraffes, and gemsbucks were to be found in abundance. Selous briefly describes a storm, which he saw at this place, during which the rain came down so heavily that, although the soil was a deep loose sand, the lightning showed a sheet of water, for the sand could not absorb the rain as fast as it fell. Resuming the eastward journey, the travellers followed the old waggon-spoor to Tati, and thence went back to Bulawayo. Having obtained permission from Lo Bengula, at the price of £60 (a salted-horse), for the right to hunt in Mashunaland, Selous decided to do so, especially as he had several commissions to execute for European museums in skins and skeletons of antelopes of a species more abundant in Mashunaland than elsewhere. He accordingly crossed the river Gwelo and passed through the country to Umfili and the Machabi Hills, where he fell in with a mighty herd of nearly 200 elephants, of which he and his man Laer killed six. Spending the best part of eighteen months in this district, during which time the camp was moved to the Manyami River, and excursions made in all directions, Selous was fortunate enough to secure five fine specimens of Lichtenstein's hartebeest, of which one pair are now preserved in the Natural History Museum, and a second pair are in Cape Town. In December, 1885, a return was made to Matabililand, and from thence a quick run home to England, only to return to Mashunaland early in 1887. It is interesting to read that the track which Mr. Selous' six waggons made in 1887, when passing over the ground on which Fort Salisbury now stands, was still visible in the softer soil near Fort Charter in 1890.

In April, 1888, Selous started from the Transvaal for the Zambesi, having received through Westbeech, the trader, an invitation from a missionary friend, who had once travelled with him, to visit Garanganzi country, near Lake Bangweolo. To a man of Selous' temperament, such an idea was delightful, particularly as it meant a journey through new country, and the probability of abundance of elephants, and hastily fitting out an expedition he set forth for Wankie, as the most favourable place to cross the Zambesi. Journeying north, the little party reached Minenga in the Mashukulumbwi country, and it was here that Selous met with one of his most stirring adventures—a night attack on his camp, which dispersed all his men, and left him alone in Africa with nothing save what he stood

up in, and a rifle with four cartridges. Taking the Southern Cross for his guide, Selous began his lonely journey southward, having his rifle stolen by the way, and narrowly escaping being shot. He fell in with the remains of his party near Wankie, and then found that on the fatal night twelve men had been killed and six wounded, the survivors bolting into the bush, and making south during the night, across the country. From Wankie, Selous, undeterred by the perils of the last journey, went up the Barotsi Valley as far as Lialui, and returned by canoe along the Zambesi River to Kazungula, where he arrived in October, 1888. In December, although no rain had fallen, he pushed rapidly across the desert country, and reached Bamangwato

HEAD OF LICHTENSTEIN'S HARTEBEEST.

in January, 1889. After paying a flying visit to England, Selous reached Quillimani, on the Coast, in July, and conducted a prospecting party to the Upper Mazoe, travelling by canoe up the Zambesi from Mazaro to Tete. Leaving the river, the travellers struck S.W., reaching Mount Hampden (close to where Salisbury now stands) about September, returning to Tete in October. Thence journeying down the Zambesi to Vicenti, he was back in Cape Town in December.

About this time Selous threw in his fortunes with the British South African Company, and in March, 1890, we find him at Palapye and Bulawayo interviewing Lo Bengula, whose ideas on the subject of

roads through Mashunaland were being discussed with Mr. Cecil Rhodes at Kimberley, in eleven days after the interview. Returning to Palapye he formed a camp on the Maclootsie River, and examined the whole district for a strategic position, which resulted in the building of Fort Tuli in the middle of the year 1890. Towards the end of June the pioneer expedition to Mashunaland set out for Tuli, and a road was opened up through Victoria and Charter to Mount Hampden, and on September 11 the expedition, despite the hostility of Lo Bengula, was brought to an end by hoisting the British flag at Fort Salisbury. Concluding treaties with the chiefs of the southern and eastern districts occupied Selous three months, and on his return to Salisbury he plotted out a five-inch to the mile map of Mashunaland, which, completed in 1892, is now in the hands of the Royal Geographical Society. In January, 1891, we find the traveller signing a treaty of alliance and concession between Mofoko, a chief of north-east Mashunaland, and the Company, and afterwards passing south to the Umtali camp, in order to cut roads from thence to the Lower Revui, and from the Odzi River to Salisbury. By the beginning of July, Selous had the entire road from Umtali to Salisbury "in good order for heavy waggons, all the bogs being corduroyed and the streams bridged." Journeying alone to Tuli to see if the waggons coming up country were in a position to bring sufficient stores for Fort Salisbury, Selous returned to Mashunaland, and was employed by the Company in laying out and making roads until May, 1892, when, there being no more work for him to do, he terminated his engagement, spent two or three months shooting and collecting specimens, and finally made his way to Beira, on the coast, and returned to England on 17 December, 1892. After a brief rest in this country, Mr. Selous returned to Mashunaland, on the news arriving of the trouble with Lo Bengula, and it is a matter of supreme satisfaction to know that so experienced and courageous a man is guarding our interests and native subjects in the wilds of Africa.

From the above somewhat lengthy account of the traveller's wanderings, the reader will have gathered that this book is one of peculiar interest, quite apart from the remarkable adventures with which it is crowded. Page after page teems with stirring exploits with lions, elephants, hyænas, and other animals, besides the risks undergone in dealing with the treacherous Matabili and other tribes. Mashunaland and Matabililand no longer convey a mere geographical expression, but are living countries, and the descriptions of their inhabitants, whether man or beast, related as they are by one who has crossed and re-crossed the country in every direction, are of surpassing interest.

The book is well got up, the illustrations are interesting, and the map of the country is of considerable value, though it suffers from the fact that the spelling of the names does not agree in all cases with

those in the narrative. It is distinctly a book to buy and put on one's bookshelves, and not one to borrow from a circulating library.

By the kindness of the publisher we are enabled to reproduce an illustration of Lichtenstein's hartebeest; and one of special interest in the book is that of a Mashukulumbwi warrior, whose singular head-dress conveys the fact that this people must necessarily live in an open country.

SOME NEW BOOKS.

AN EXAMINATION OF WEISMANNISM. By George John Romanes, M.A., LL.D., F.R.S. Pp. 221. London: Longmans, Green & Co., 1893. Price 6s.

THIS book presents many of the characters of book-making. It has been written piece by piece—one section of it being intended for the second part of "Darwin and after Darwin," unfortunately delayed by Dr. Romanes' illness; another part consists of additions and modifications suggested by recent publications of Dr. Weismann; one appendix is a reprint of some controversy in the *Contemporary Review*, and an excellent portrait of Dr. Weismann has been put at the beginning that readers may know the features of the gentleman who is being criticised. There is in the book, therefore, the precise difficulty which Dr. Romanes complains of with regard to the writings of Professor Weismann. Inconsistencies, additions, and emendations make it very difficult to understand the exact views of Dr. Romanes. Thus in the preface the non-inheritance of acquired characters (the discussion of which is postponed to another volume) is said to be the "fundamental postulate upon which Weismann has reared his elaborate system of theories": on page 49 the fundamental postulate is stated to be the continuity and stability of germ-plasm. On page 77, while admitting that influence of a first sire upon children to a second might be reconcilable with absolute continuity of the germ-plasm by supposing the germ-plasm in the spermatozoa to penetrate an unripe ovum, he holds that an analogous phenomenon in plants goes to exclude that hypothesis and most definitely to substantiate the hypothesis that the spermatic element must exercise some influence on the somatic-tissues of the female, which in turn act upon the ovum. In the second appendix he attacks Herbert Spencer for precisely this second view, and strongly supports the first.

These, however, are mere accidental blemishes, and Dr. Romanes' general attitude can be observed. This general attitude apparently was determined by the question of acquired characters. As is well-known, when observations on the Hydrozoa led Weismann to the conception of a continuity of germ-substance from generation to generation, he raised the question as to how acquired characters could affect the germ-plasm at all. The controversy that arose turned upon the interpretation of facts, and most will agree that the controversy has contracted since it began. At first, the opponents of Weismann said that there was an overwhelming amount of evidence in favour of the general inheritance of acquired characters. Against that view Weismann and others urged that the germ-plasm was stable: unaffected by somatic changes. Now, at least, Dr. Romanes and others freely write that acquired characters are much less veritable than are congenital characters, and a large number of cases, formerly insisted upon, have been abandoned. On the other hand, Professor

Weismann has expanded those hints in previous writings, that Dr. Romanes called illogical admissions, and in his most recent book, by allowing that unequal nutrition affecting the germ-plasm may cause variation of the germ-plasm, he "admits" the action of the soma upon the germ-plasm to an extent which will probably be found to cover the residuum of facts now that they have been sifted by controversy. For this source of variation, in addition to amphimixis, and to the inherited results of the original action of the environment upon primitive organisms, brings Weismann's theory of evolution so nearly into line with what Dr. Romanes agrees to, that at first he proposed to cancel a large part of this book—but he has left it, showing how very prettily he would have fought had there been anything to fight against! But he still insists, against Weismann, that Weismann's theory requires the absolute stability of the germ-plasm. However, his examination of this question cannot be complete without the promised sections upon acquired characters, and everyone will hope that Dr. Romanes' health will allow him to publish the new volume soon.

In other matters, however, there is quite enough left for controversy.

Dr. Romanes very acutely divides Weismannism into the theory of heredity which deals chiefly with the germ-plasm, and the theory of evolution which deals chiefly with variation.

He objects to the vast elaboration of the germ-plasm with its molecules, biophors, determinants, ids, idants, and so forth, chiefly because he regards it as a work of "artistic imagination" rather than of "scientific generalisation." So far as concerns "determinants"—the particles of germ-plasm which correspond to groups of cells that vary similarly and simultaneously—he admits the great value of the conception, and he accepts the "ids" as a logical consequence of "determinants"; but he sees no reason whatever for identifying idants with chromosomes. In this, Dr. Romanes makes the same error as he did in the matter of the continuity of the germ-plasm. In both cases, Weismann started with the observed fact; the visible changes in the Hydrozoa led him to the continuity, and the actual divisions and marshallings of idants in sexual cells led him to the particulate theory of germ-plasm. And it is because his theories start from observed facts of this kind that they appeal so strongly to "laboratory naturalists."

He gives a very interesting comparison between Galton's "Stirp," and Weismann's germ-plasm, pointing out how the recent extensions (Romanes would call them "revolutions") in the latter theory bring it more into harmony with the former. Both allow that the soma arises from the germinal cells, and that in each ontogeny only a part of the germinal material is used up—the greater part being handed on. According to Galton, occasionally, but rarely, there are contributions from the soma to the stirp, and so acquired characters may be inherited occasionally: in Weismann there are never such centripetal contributions, and so acquired characters, as such, are never inherited. In the mechanism of ontogeny, Weismann insists on a peaceful disintegration of the architecture of the germ-plasm; Galton on a constant struggle between competing carriers of heredity. Weismann, in his dealing with "xenia"—the influence in plants of fertilisation, not on the fertilised cell, but on the tissues of the female, allows the possibility of the direct action of germ-plasm upon the somatic tissues, even though those tissues may belong to

another individual. Dr. Romanes calls this a surrender of the perpetual isolation of the germinal substance to a sphere of its own, and evidently holds that it goes towards establishing a reciprocal action between soma and germ-plasma. It is difficult to follow this. Weismann has of course always held that germ-plasm is isolated only so far as it does not receive impressions from the body, to be reflected upon the body of the next or after generations. The whole of his theory of the structure of germ-plasm implies that its particles have a determining influence upon the soma, and there is no difference in kind between supposing that determinants can enter and determine embryonic cells, and supposing that they may enter and determine adult cells. It is not always given to a man to know his own father, and there is no reason to suppose that a determinant can nicely distinguish between protoplasm of a developing cell and protoplasm of another cell, even although that other cell belongs to another plant.

In the theory of evolution Dr. Romanes apparently resents that the recent elaboration of Weismann's views has met many objections to them. This is chiefly in the matter of variation. Dr. Romanes writes:—"Weismann has now expressly surrendered his postulate of the absolute stability of germ-plasm! We have already seen that, even in the first volume of his *Essays*, there were some passages which gave an uncertain sound with regard to this matter. But as they seemed attributable to mere carelessness on the part of their author, after quoting a sample of them, I showed it was necessary to ignore such inconsistent utterances—necessary, that is, for the purpose of examining the theory of germ-plasm as even so much as a logically coherent system of ideas."

How charming! but with all deference to Dr. Romanes, it is, at least, as fair to suggest that Weismann, by the inclusion of "inconsistent utterances," was, in the true spirit of science, guarding himself against "logical coherence" in a shaping theory. It may be given to schoolmen, writing of such matters as the dancing of angels upon a pin-point, to be logically complete—but until, root and branch, we have eaten up the tree of knowledge, any theory of evolution or heredity that is logically coherent and complete will be scientifically incoherent and incomplete. The particular extension in question is the idea that local variations of nutrition, of which all along Weismann, as everyone else, has recognised the capacity to produce variations in *the individual*, may also produce variations in the germ-plasma. As Dr. Romanes rightly points out, this extension removes the great stumbling block of seeing as the cause of variation only the combination produced by amphimixis of original Lamarckian modifications of the Protozoa.

A Dictionary of Birds. By Alfred Newton, assisted by Hans Gadow, with Contributions from R. Lydekker, C. S. Roy, and R. W. Shufeldt. Part ii. 8vo. London: A. & C. Black, 1893. Price 7s. 6d. nett.

Amongst a certain section of that vast brotherhood ycelpt "Ornithologists," a work like the present has been a long-felt want; and, when it was announced that Professor Newton had in hand a "Dictionary of Birds," we are afraid that we allowed our imagination a rather lofty flight. As might be expected in such a case, when the first part actually appeared, we were bound to confess to a certain feeling of disappointment, inasmuch as we had expected a

work on a rather more pretentious scale: we felt that neither the Professor nor his colleagues were doing themselves justice. After having made ourselves tolerably familiar with the first part, however—the general scope and purpose of which, it will be remembered, was set forth in these pages a few months since—this feeling has, in many respects, given place to a sense of gratitude for small mercies, for undoubtedly it is the best book of its kind which has yet appeared, and we imagine that, not until the second edition appears will there be a better.

To attempt anything more than a very rapid survey of the part before us would be to far exceed the space at our disposal, but it shall be our endeavour to crowd as much information into that space as possible.

The first article of any importance is that on Geographical Distribution, and when we say that it covers some 52 pages, it will be seen that it is fairly exhaustive. We can bestow no further praise upon it than to say that it is in every way worthy of comparison with the excellent article on Extermination which has preceded it. Conning its pages, we feel that the whole of this most difficult subject has been so lucidly explained, that even those who take up this book for the first time will have no difficulty in mastering it.

The account of the Grebes interested us much. The figure of the Great Crested Grebe conjured up a host of old and delightful recollections of days spent, so to speak, in their company, in one of their few remaining strongholds, the Norfolk Broads. We believe that we are correct in stating that its feathers are no longer "much in request for muffs and the trimmings of ladies' dresses," inasmuch as the tide of fashion has turned in another direction.

That the gizzard of this species, and Grebes in general, is invariably filled with feathers from its breast, mixed with fish-bones, is a fact that has long been known, yet, curiously enough, it has escaped notice here. The serration along the back of the tarso-metatarsus has also passed unnoticed, though this, it is true, is a point of small importance.

The articles on Humming-Bird, Hornbill, and Kiwi are brimful of interest—especially the first-mentioned. The marvellous brilliancy of the plumage, the extraordinary variety in size and ornamentation, coupled with the Edenic life these birds are popularly supposed to lead, have tended to obscure them in a haze of poetic glamour shared, probably, by no other animal on earth. We imagine, therefore, that not a few will find it difficult to picture humming-birds "dwelling in a world of almost constant hail, sleet, and rain," or "flitting about the Fuchsias of Tierra del Fuego in a snow-storm." There is surely good reason to fear that the recording angel will soon have to inscribe the name of "*Chrysolampis mosquitus*" upon the calendar of departed species, or, in other words, it will go to swell the ranks of Birds *recently exterminated*, since "thousands of skins are annually sent to Europe to be used in the manufacture of ornaments (!), its rich ruby-and-topaz glow rendering it one of the most beautiful objects imaginable."

In speaking of the Manucode, Professor Newton says: "As with members of the Paradiseidæ generally, the nidification of the Manucodes had been shrouded in mystery, until . . . the nest and eggs of *M. comrii* were found in July, 1891 . . . ." This is correct as far as this species is concerned, but the egg of *Paradisea raggiana* was first described so long ago as 1883, by Mr. E. P. Ramsay, in the

Proc. Linn. Soc. N.S.W., vol. viii., and the egg of *P. apoda* was described from Aru in the *Zeitsch. f. ges. Ornith.* for the same year.

In the brief description of the Guinea Fowls, *Numida vulturina* has been included with those species which have "the crown bare of feathers and elevated into a bony 'helmet.'" This is, of course, an oversight, this particular species having no "helmet."

This part concludes with two extremely interesting articles on Migration and Mimicry. The former is especially fascinating, and brings together a mass of information, for which the general reader should be grateful.

Dr. Gadow's contributions can in no way be said to play a secondary part in this book. His task is by no means an enviable one, for to render concise and readable articles out of such uncompromising material as has fallen to his share to mould, is, to say the least, a difficult one; but, nevertheless, he has succeeded in presenting us with descriptions that are at once singularly clear and undoubtedly correct. We would, however, be allowed to point to the description of the humerus and the four accompanying figures as a slight exception. In the figures referred to—the left humerus of a raven—all the more important points of muscular attachment have been distinctly named, but, in the lowest figure, the crista superior is called crista "lateralis," while in the text "tuberculum superius" should read "tuberculum externum" so as to agree with the figures. So again, "tuberculum inferius" should read "tuberculum medium," for the same reason.

Now a word as to the illustrations. On every hand, the figures of Swainson have been most eulogistically praised. *Fas est dictu*—we have seen better. For instance, who would recognise the caricature on p. 401, which is supposed to represent the head of the Guinea Fowl? or the ? model for an umbrella handle on p. 406? which we are asked to believe is the head of *Scopus*. To our mind, the figures on pp. 388, and 508–10 are far-and-away more life-like and beautiful. Some of Swainson's figures undoubtedly are good, such as that of the head of the Hawfinch, but these are the exception, and not the rule. The elegant figures of the Heron, Jabiru, and Merlin are almost beyond praise, but these are not Swainson's.

Figures of bills and feet come prominently to the fore in this book; doubtless they have their value, but they are by no means so important as the prominence given them would lead the lay mind to believe. In the case of unfamiliar forms, such as the Guan, we venture to think they are of no use whatever; here, not even the entire head is given, but only the beak. The amateur must indeed have a vivid imagination if he would conjure up the remainder of the bird!

We wish it to be distinctly understood that these remarks are offered, not in any spirit of captious criticism, but, as suggestions which might be acted upon in a second edition, which, we feel sure, will be called for.

We will conclude as we began, by reiterating that this book undoubtedly fills a gap, and it is hardly likely to be supplanted by any similar work for a long time to come. W. P. P.

THE ZOOLOGICAL RECORD FOR 1892. Edited by D. Sharp, F.R.S. 8vo. Pp. 926. London: Zoological Society, 1893.

IT is no exaggeration to say that the *Zoological Record* is the most important zoological publication in existence. The absence of this

yearly volume would bring about a state of affairs too terrible to contemplate even in imagination; it is our business, therefore, to be exceedingly thankful to the editor and his staff of contributors for the great pains which they have taken in registering the immense amount of zoological work of the past year, and to the Zoological Society for bearing the expense of this necessarily costly publication. These grateful feelings on our part leave scarcely any wish to criticise; but as a matter of precedent it is a very easy task to criticise the *Zoological Record*; it has been laid down in all criticisms of past volumes which we have read, that the way to deal with this work is to search diligently for misprints; to ignore the vast amount of labour expended upon its production, and to dwell particularly upon the defects of newly-enrolled members of the staff of recorders. We, personally, happen to be of an extremely conservative disposition, and are, therefore, intense respecters of precedent; in spite, however, of natural inclination, stimulated by such misprints as "*Aelosoma*" for "*Aeolosoma*," *fluviventris* for *flaviventris*, *Naidonina* for *Naidium*, etc., we feel very strongly that the only proper way to criticise the *Zoological Record* is to point out, if it can be done, that the record is incomplete. So far as we have tested the present volume this is not the case; it appears, however, that the last volume was so to some extent, for there are a few references to papers published in 1891, which should, of course, have been included in that volume. But—particularly in this kind of work—better late than never! One of the more useful features of the *Zoological Record* is the general section with which it commences; this improvement was introduced by the late editor, and we are glad to notice that the present editor has not dispensed with a very important part of the volume.

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While the book is primarily intended as a popular work, we do not think the author will be disappointed of the hope expressed in the preface that the serious student of entomology will find it useful. The only objection is likely to be raised by the householder, who will perhaps complain that while the author describes with enthusiasm the jaws of the creatures and the depredations they commit on

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Judging from the part now before us, we find that the articles as a rule, are much shorter than those of Fullarton's Gazetteer of England and Wales, of which the first edition was published in 1843. It was no doubt difficult to get good accounts of the soils in each parish, consequently we find the subject treated very unequally, and not always clearly, nor accurately. The following are instances:—At Abbeystrowry, "Soil clay and artificial loam, overlying slate"; Abbotsbury, "Soil chiefly red clay" (no mention of iron ore and limestone); Aberavon, "Soil sand and gravel, overlying coal and minerals"; Abernethy, "Soil chiefly granite rock"; Aldborough, "Soil loamy, overlying clay" (no mention of shelly "crag"); Aldington, "Soil various, overlying rock"; Aldsworth, "Soil stone-brash, overlying lime and freestone." A concise and reliable account of the principal soils and economic products of the rocks in each parish would have been useful.

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the books being adapted for beginners, while others will deal with special topics and will be useful only to more advanced students. The series will be divided into two sections, a Biological and a Physical. The former will be published under the general editorship of Mr. Arthur E. Shipley, M.A., Fellow and Tutor of Christ's College, Cambridge, it will include a manual of Invertebrate Palæontology by Mr. H. Woods, Demonstrator of Palæobotany at Cambridge, which is now ready; a text-book on the Practical Physiology of Plants, by Mr. Francis Darwin of Christ's College, and Mr. E. Hamilton Acton of St. John's College, which is in the press; works on Physical Anthropology, by Professor Alexander Macalister; on the Vertebrate Skeleton, by Mr. S. H. Reynolds of Trinity College; on Fossil Plants, by Mr. A. C. Seward, Lecturer in Botany in the University, and an Introduction to the study of Botany by Mr. Francis Darwin, which are in preparation. Other volumes will shortly be announced.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

MR. CHARLES F. W. MCCLURE, M.A., has succeeded Dr. Osborn as Professor of Biology at Princeton College, New Jersey.

THE REV. A. IRVING, D.Sc., F.G.S., has resigned his appointment at Wellington College, Berks, and removed to Hockerill, Bishop's Stortford, Herts.

THE late Professor Newberry's name is to live in a fund which the scientific societies of New York have resolved to raise. It will be called the John Strong Newberry fund, be not less than 25,000 dollars, and the income derived from it will be devoted to the encouragement of scientific work in geology, palæontology, botany, and zoology. Professor N. L. Britton is secretary to the subscription committee.

THE Jersey Biological Station promises a quarterly publication together with an issue of microscopical preparations of the rare and less known marine organisms. The text will be descriptive of the slides issued, and will contain hints and notes on microscopical manipulation, together with original observations upon Marine Zoology. The subscription for the year, including fourteen slides, is 21s., and this issue is limited to seventy-five copies.

WE regret to learn that, owing to the transfer of political power in the Illinois legislature, Dr. Josua Lindahl has been dismissed from the post of State Geologist. Dr. Lindahl's scientific attainments are well known; he served as zoologist on the "Porcupine" expedition, and the concluding volume of the Report of the Geological Survey of Illinois showed that in him that State had secured an energetic, industrious, and capable servant.

WE much regret to learn that the post honoured by the names of Amos H. Worthen and Josua Lindahl, has been handed over to a Mr. William F. E. Gurley. This gentleman is, we understand, a collector of fossils, and his name has once appeared in so-called scientific literature as co-author of a paper with Mr. S. A. Miller, of Cincinnati, O. One more step remains for the State of Illinois—to make Mr. Miller State Palæontologist; and this step will doubtless be taken.

THE fine collection of fossils made by the late Professor A. H. Worthen, which contains very many of the specimens figured and described in the Report of the Geological Survey of Illinois, has at last been bought by that State. The specimens are at present exhibited in the Illinois building at the World's Fair. Each has a register number, and, as a very complete sale-catalogue was published,[1] all future visitors to the State Museum will easily be able to identify them.

[1] Warsaw, Ill. 1889; sold in London by Wm. Wesley & Son, price 6d.

It seems odd to us in England that a well-paid State Geologist should be able to make a private collection of 2,871 fine specimens, to treble its value by selecting 752 of these as types for the descriptions by other State officers in the State publications, and as a consequence to finally force on the State the purchase of this collection for a very large sum of money. But in America such a proceeding is not thought in the least odd; and that is the oddest thing of all.

The Indiana Academy of Science has, according to *Erythea*, determined on a biological survey of the State. The promoters intend to publish a complete bibliography of the botany, zoology, and palæontology of Indiana, to ascertain what has already been done, while the main purpose of the survey will be to make known the extent, distribution, biological relations and economic importance of the entire fauna and flora. L. M. Underwood is at the head of the botanical division, and the special work of this season in botany will be a study of distribution, particularly of the lower cryptogams and of the rarer forms of flowering plants and ferns. We trust the enthusiasm of the Indiana naturalists will endure throughout and carry to a successful issue this worthy project.

The *Transactions of the Norfolk and Norwich Naturalists' Society* for 1892–93, which has just appeared, form an excellent model for local societies. Of the 170 pages contained in this part, all except four or five relate to the natural history of Norfolk. The address of the President (Mr. H. B. Woodward) deals with the geology of the county. Then follow papers by Mr. Southwell on the Siberian Pectoral Sandpiper, Sowerby's Whale Shooting at Holkham, The Herring Fishery of 1892, and some Additions to the Norwich Museum. Mr. J. H. Gurney writes on the Immigration of the Lapland Bunting; Mr. H. B. Woodward on Caleb B. Rose, the Norfolk Geologist; Mr. Mayfield on Norfolk Slugs; Mr. A. W. Preston on Meteorology; Mr. Clement Reid on *Paradoxocarpus carinatus* from the Cromer Forest-bed; and last, Mr. James Edwards contributes an account of the Coleoptera, forming Part XII. of the Fauna and Flora of Norfolk. Sir P. Eade's note upon Tortoises is the only contribution not of local interest; and even it relates to tortoises that have lived ten years in the county, and may therefore be considered almost to belong to it.

The first number of the *Proceedings of the Malacological Society of London* has just made its appearance, edited, under the direction of a publication committee, by Mr. B. B. Woodward. In addition to the proceedings of the inaugural and subsequent meetings of the first session, it contains eight of the papers read by members. One-half of these are systematic, while of the remainder two are anatomical, so that the ground covered is fairly representative of the aims of the Society, as set forth on the second page of the wrapper. The number, which runs to 30 pp., is illustrated by two full page plates and several illustrations in the text. A footnote to page 11 makes us aware that conchologists cannot, alas! more than any other section of humanity, implicitly trust their fellow man, some smart American seemingly having robbed Mr. E. A. Smith of one of his new species, a proceeding which the publication committee "greatly regret," and reflect upon.

The fifteenth annual meeting of the Greenock Natural History Society was held on September 28, and the secretary, Mr. G. W. Niven, presented a satisfactory report. Only one paper embodying local research seems to have been read during the last session, that by Mr. M. F. Dunlop, on some rare Rotifers. The meetings are chiefly occupied with papers of a general character, and sometimes relate to physical science.

The fifty-fifth annual meeting of the Manchester Geological Society was held on October 10. Notwithstanding unusual losses by death and other causes, the membership is still well maintained, and many important communications, especially

of economic interest, have been discussed during the past session. In the early part of this year a circular was addressed to the members and the coal-owners and lessees of Lancashire and Cheshire, asking for their assistance and co-operation in the work which the Society is hoping to accomplish, of giving a fairly accurate enumeration and description of the fossils of the coal-bearing rocks of this district. Instructions were given to carefully note the exact horizon and locality from which each fossil was derived, so that an attempt might be possible to correlate the various coal seams in different parts of the county, thereby rendering valuable aid to future explorations for coal. The result of this appeal has not, as yet, been such as the Council had reason to expect, but it is hoped that the object has not been lost sight of, and that considerable additions to our knowledge of the fossil fauna and flora of the Coal-measures will yet be forthcoming from the hands of the members and others interested in the work. Several members have already responded by sending specimens, whose assistance will be acknowledged and their contributions incorporated in the next list of Fossil Plants, etc., which is being prepared by Mr. Robert Kidston, F.G.S., for publication in the Society's Transactions.

OBITUARY.

H. W. CROSSKEY, LL.D., F.G.S.

BORN 1826. DIED OCTOBER 1, 1893.

BY the death of Dr. Crosskey, Geology loses one of the most painstaking students of Glacial and post-Glacial deposits; while to the general public he was well-known as a leading Unitarian minister, and as one devoted to the educational advancement of the poorer classes. He was born at Lewes, and undertook his first pastorate at Derby. Thence he removed in 1852 to Glasgow, and in 1869 to Birmingham. While in Scotland he paid much attention to the post-Tertiary deposits of the Clyde Valley, and about the year 1855 he became associated with David Robertson, "The Naturalist of Cumbrae," and their joint labours on those fossiliferous deposits were published by the Geological Society of Glasgow. Later on they were joined by Dr. G. S. Brady in a special study of the post-Tertiary Entomostraca (or Ostracoda), and this resulted in a Monograph on the subject, which was published by the Palæontographical Society in 1874. Dr. Crosskey was the author of a series of Reports on the Erratic Blocks of this country, which were communicated during the past twenty years to the British Association.

THE death is also announced of MR. THOMAS BAIN, the South African Geologist, who for a long period continued the work of his father, the late Mr. Andrew Geddes Bain, in discovering so many of the fossil reptiles of the Triassic Karoo Formation now in the British Museum.

CORRESPONDENCE.

The pre-Glacial British Fauna.

The solution of the problem which forms the subject of Mr. Bulman's article is a very important one, both from a biological and from a geological point of view, and he has very ably argued the points in favour of a survival of a pre-Glacial Fauna and Flora in the British Islands. Since the appearance of Professor Forbes's classic essay referred to by Mr. Bulman, many additional facts regarding the existence of a Lusitanian Flora in the South of Ireland have been brought to light, and although the possibility of their post-Glacial transmission by birds is not to be excluded, it is not likely that such was their origin, since they are now known to be associated with Lusitanian animals, of which one at least (*Geomalacus maculosus*) must have reached these shores by a land passage, as both it and its eggs are almost immediately killed by immersion in sea-water. That this slug should have been accidentally carried by birds from Portugal to the South of Ireland is surely not worthy of argument. The geographical distribution of slugs, indeed, and of non-operculate molluscs, especially the subterranean species, may be most profitably utilised in the task of solving the problem of the origin of the British Fauna.

Equally important, and in many respects even more so, are the mammals, and an enquiry like that initiated by Mr. Bulman might with advantage be started, tracing the origin of the Irish species first. Familiar English species, such as the Weasel, Mole, Voles, Common Shrew, and Hare, and many others, are absent from Ireland. It has been suggested that Ireland was separated from England before these mammals had time to cross over, but among them we have some of the most quickly-spreading animals, and it is more probable that they arrived in England only long after the species at present inhabiting Ireland, which therefore formed part of another and much earlier immigration from the continent. This immigration was probably pre-Glacial, but that the second was not entirely post-Glacial seems proved by the fact that remains of some of the mammals mentioned as being absent from Ireland occur in the Forest Bed.

R. F. Scharff.

22 Leeson Park, Dublin.

Archæopteryx.

In reference to Dr. Hurst's interesting, but somewhat intemperately-worded article on *Archæopteryx* last month, I should like to remark that although Professor von Zittel's copy of Owen's restored figure of the hand of this bird wants one finger in the original German edition of the "Handbuch der Palæontologie," the mistake is rectified in the subsequent French edition. It is thus obvious that the author's omission was an accident, and not intentional; possibly the block was "battered" at the edge of the page.

A. S. W.

TO CORRESPONDENTS.

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

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 22. Vol. III. DECEMBER, 1893.

NOTES AND COMMENTS.

A National Trust for Places of Beauty and Interest.

WE learn from the *Daily News* that a movement which may turn out to be of some importance was started at the rooms of the Commons Preservation Society on November 16. It has long been felt that the nation is in the way of losing some of its most valuable possessions through the want of some custodian to whom they may be readily transferred, and by whom they will be jealously guarded. Districts celebrated for their natural beauty are by degrees marred and disfigured; houses and ruins of unique interest are destroyed, because, passing from hand to hand, they some day come into the ownership of persons unable to appreciate them, or forced to realise any money value they may have. Within the last two years, such noteworthy spots as the top of Snowdon, the island in the middle of Grasmere Lake, and the Lodore Falls have come into the market, and might not improbably have been permanently secured for the public enjoyment had some body capable of acquiring and protecting them in the public interest been in existence. We have also noticed with grief the quarrying operations that are carried on along the Cheddar Cliffs—the grandest inland cliffs in this country—as if there were not abundance of the Mountain Limestone to be obtained elsewhere in the neighbourhood. Local authorities can hardly at present be expected to help the public to preserve the beauty of its great pleasure grounds; their area of action and the sources from which they draw their funds are, as a rule, too contracted, and they have many claims upon their not too ample resources. But apart from local authority there is absolutely no body which can hold and manage places of beauty and interest on behalf of the public; for the existing open space societies are not corporate bodies and exist rather to influence public opinion than to hold property. This want it is now proposed to meet. Canon Rawnsley, the Vicar of Keswick,

well known as an energetic champion of the Lake District, has for some weeks been enlisting support for a body which it is proposed to style the "National Trust for Places of Historic Interest and Natural Beauty." This body is to be incorporated under the Joint Stock Companies Acts, with the licence of the Board of Trade, as a non-profit-earning society. Its primary function will be to accept from landowners gifts of places which they desire to place beyond risk of injury from their successors, and to keep such places intact and at the service of the nation. As funds increase it is thought that purchases of important places may be made, either at the expense of the general agents of the society, or by means of special contributions for the purpose. The new society is enabled to make a beginning by the generosity of a Welsh landowner, who is desirous of transferring to its care a beautiful sea-cliff on the West Coast. It numbers among its adherents such well-known names as the Duke of Westminster, Lord Dufferin, Lord Rosebery, Sir Frederick Leighton, Professor Huxley, the Master of Trinity, Cambridge, Mr. Shaw Lefevre, and Miss Octavia Hill, and will lose no time in "acquiring legal form, and entering upon its duties." In so doing it will certainly carry with it the good wishes of all who have a feeling for nature and historic association.

WINCHESTER COLLEGE.

ALL the world knows that Winchester College has recently been celebrating its Quingentenary. To commemorate this event by some permanent memorial has long been the wish of Wykehamists; but the multiplicity of proposals proved the truth of the saying, "quot homines, tot sententiæ." It has at last been decided to raise a fund for two purposes. First, the restoration of William of Wykeham's Chantry in Winchester Cathedral; secondly, the establishment of "a group of Memorial Buildings for the preservation of Wykehamical antiquities, and the encouragement of art, archæology, natural history, and other sciences."

The former of these objects lies outside our province. With regard to the latter, we extract from the circular of the Executive Committee the following excellent remarks:—

"The aims of the collection of archæology and art would be (i.) to illustrate and encourage the regular course of school study; (ii.) to furnish boys with interests outside that regular course. The first division would naturally consist mainly of collections illustrating classical art, or otherwise bearing on the study of the classics or the Bible. The second division would consist mainly of mediæval or modern specimens of art. Such collections would consist of representative series of reproductions of objects of art selected for their beauty or educational value, and of any good originals that could be obtained. The latter are needed to give reality to the collection,

and to let boys acquire a touch-and-handle familiarity with specimens; the former to show the real beauty of antiquities, and to stimulate a desire to visit the famous collections."

"The science collections would probably be a development in more adequate quarters of the present collection of the Natural History Society, which is good though not large. Special stress would doubtless be laid on the collection of local minerals [fossils are doubtless meant], fauna, and flora. An attempt might also be made (as has been done at Harrow) to imitate the admirably instructive series of type forms exhibited in the Museum of Natural History at South Kensington; and it would be highly desirable to connect some moderate provision for elementary biological and botanical work with the natural history museum."

"It would be premature at present to venture on more than the most general sketch, but many other developments might be suggested which should be the work of subsequent years, for it must always be borne in mind that for the purposes of a school museum the process of growth is more valuable than any completed collection, and that the undertaking will be a failure unless it commands the sustained interest and support of masters and boys alike."

All who sympathise with the extension of scientific education will rejoice that Winchester is no longer to be behind our other great Public Schools in this respect; and if we number any Wykehamists among our readers, we have no doubt that they will hasten to send in their subscriptions to Mr. Percy Toynbee, 109 Gloucester Terrace, Hyde Park. As for the presentation of specimens, we will lead the way with a little bit of advice to the Committee:—Be as ready to refuse as to accept!

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

WE take the following comment from the last number of the *American Naturalist*, and commend it to the notice of those interested in the welfare of the British and kindred Associations:—"The question is often asked, Why do the American zoologists so universally neglect the American Association for the Advancement of Science? For many years scarcely an American publishing zoologist has been present at the meeting, while the few papers on zoological subjects are in striking contrast to the interest shown in the sister science of botany. The reasons for this state of affairs are not readily stated. Possibly most potent of all is the feeling that the Association is far from being a representative of American science, and that it has degenerated into an annual junketing party. It is certain that the interests of science have been often sacrificed to excursions which interrupt the sessions, and which should be postponed to their close. Then, too, criticisms are often heard that it is run as a close corporation, that nominating boards are packed in order that certain persons may be put in office,

and that the expenses of the Association are far greater than they should be for the results achieved. There is, too, an inside history which cannot be detailed, which would explain a large portion of the indifference displayed. Before the Association can regain its influence it must undergo a complete transformation in its management and methods of administration. It must also present features which will attract the better workers of the country."

Fortunately, the harmony of the British Association is not marred by any such "inside history" as that of which our American cousins complain; but even here, and in other British learned Societies, those who happen to hold professional positions do not always pay that respect to distinguished amateurs which is their due, when chosen to officiate or to inaugurate discussions. The picnic-element can never be entirely eliminated from annual gatherings—it is not desirable that it should be. Science cannot progress satisfactorily without some practical sympathy on the part of the outside public; and it is the duty of those concerned in purely scientific work at the great festival which nearly every civilised nation now holds, to maintain the correct balance between technical discourses and intellectual recreation.

THE CORRELATION OF GEOLOGICAL FORMATIONS IN EUROPE.

WITH the advance of science, more and more minute methods of investigation are adopted, methods which could not have been adopted before, while sources of error remained so numerous, but which, when once a firm foundation has been laid for them, speedily lead to important results. It is often found, too, that the foundations are laid by branches of a science that appear distinct, and that the eventual results are attained by the combination of differerent methods. Changes of this kind are now taking place in Palæontology and Stratigraphical Geology. It is no longer possible for the palæontologist to study his fossils within the four walls of a museum. He has learnt all that can be learnt there; but now he sees certain minute differences between forms, which he cannot quite explain. They may be important or they may be only accidental. He can only decide by himself carefully collecting the specimens in the field. Prepared by long study of certain groups of animals, intimately knowing them, almost as a shepherd does his sheep, he now proceeds to examine the rocks with equal minuteness. He is no longer content with the broad generalisations of pioneer geologists, but, just as he has already split up the genera of the old naturalists into numberless more accurately defined new genera and sub-genera, so he must divide the strata into zones and sub-zones and almost infinitesimal horizons. Then at last he will be able to discover the relations that exist between the variations that he has observed in his fossils and their place in geological history, and he will be able to trace out with a certainty that none can gainsay the evolution of species and of genera, and the migrations of fauna.

Work of this kind has been done abroad for some time. The important biological results of Beecher, Clarke, and others, to which we have often alluded (NATURAL SCIENCE, vol. i. pp. 606, 628; vol. iii., pp. 15 and 163), almost entirely depend on very careful collecting, inch by inch, through considerable thicknesses of rock. Munier-Chalmas and Haug have done similar work in Europe of late years; but in England, if we except the admirable investigations of Lapworth on Graptolite zones, which could only have been carried out by a man thoroughly acquainted with the fossils, little of the kind has been accomplished. We therefore welcome heartily the very interesting paper by S. S. Buckman, on the Bajocian of the Sherborne district, just published in the *Quarterly Journal of the Geological Society* (vol. xlix., p. 479). Not only by his exact knowledge of, and previous work on, the Stratigraphy of the Dorset Oolites, but by his prolonged studies of their most important fossils, the Ammonites, he is excellently qualified for work of the kind we have just described. Hence we are not surprised to see that he goes very much further than previous geologists in his subdivision of the strata, while that his subdivisions are not illusory is proved by the possibility of tracing them in adjacent districts and even on the Continent. Thus, he is able to show that there is the same faunal succession in Dorset, Somerset, Gloucestershire, Normandy, Southern France, and Würtemberg. This important point, it must be remembered, could never have been proved under the old system, when three zones were the most that were recognised in the Inferior Oolite. Indeed, as the President of the Geological Society remarked at the reading of the paper, "It was scarcely too much to say that if rocks were to be studied in this minute way, the whole of stratigraphical palæontology would be revolutionised."

The point in Mr. Buckman's paper that will most strike the casual reader is his proposal of a new term, "hemera," as a subdivision of the technical term "age." Practically the word means the time during which a particular species was in existence. Thus "*fuscæ* hemera" means the period of time during which *Oppelia fusca* lived in the district in question. The species chosen to give names to hemeræ are, of course, those that only had a short existence, for the object is "to mark the smallest consecutive divisions which the sequence of different species enables us to separate in the maximum developments of strata." The term, therefore, is a purely chronological one, neither superseding nor a subdivision of "zone," so that the objections to it made by speakers at the meeting fall to the ground. Mr. Buckman, indeed, points out that species actually occurring together in a thin band of rock, may really belong to different hemeræ, a fact which can sometimes be proved by examining the same stratum in another place where its development is greater.

Of the other very interesting results brought out by this detailed method of work, such as the variations in the amount of deposition and the migrations of the mollusca correlated therewith, we can hardly

speak here. It is certainly obvious that this kind of investigation is what we want more of; but it is also obvious that it can only be done by those who, like Mr. Buckman, are palæontological specialists, and who are, like him, intimately acquainted with the district that they are examining, and able to take advantage of every temporary exposure. This is where our numerous local geologists and collectors will find their opportunity.

THE CORRELATION OF GEOLOGICAL FORMATIONS IN AMERICA.

THE principles alluded to above are clearly not neglected by our fellow-workers on the other side of the Atlantic. The Geological Survey of the United States has issued a series of "Bulletins" during the past few years dealing with the Correlation of the Geological Formations of the United States. These bulletins (nos. 80, 1891, Devonian and Carboniferous; 81, 1891, Cambrian; 82, 1891, Cretaceous; 83, 1891, Eocene; 84, 1892, Neocene; 85, 1892, Newark System; 86, 1892, Archæan and Algonkin) are historical studies of the classifications and nomenclatures of the formations, made for the purpose of ascertaining how satisfactory correlations have been made, and upon what principles they have been based. The literature of the century has been reviewed, and various specific problems have arisen for the solution of which it has been necessary to determine the relations between standard formations already named and classified and those newly discovered. The discussion of each problem has been followed out in detail, the various attempts at correlation have been noted, and the methods employed and the final results attained have been traced to the principles involved in their determination.

We reproduce here the headings of researches dealt with in the Devonian and Carboniferous monograph, as giving a good idea of the amount of material rendered easily available to the geological student. They are as follows:—

(1) The general correlation of the Palæozoic formations of eastern North America with the corresponding formations of Europe.

(2) The determination of the parallelism between the upper Palæozoic formations of the Appalachian region and the rocks of the interior of the continent as far west as the Mississippi River.

(3) The correlation of the northern Appalachian region with the various subdivisions of the Coal-measures and formations immediately underlying them.

(4) The problems connected with the correlation of the Chemung and Catskill groups, and with the correlation of the Waverley and Marshall groups.

(5) The elaboration of the Mississippian series, or "sub-Carboniferous" formations of the Mississippi River basin.

(6) The Permian problems of Kansas and Nebraska.

(7) The correlation problems involved in classifying (*a*) the formations of the Acadian province, and (*b*) the formations of the Rocky Mountains and Western Plateau provinces.

In the discussion of these various problems, the definite stages in the development of the principles of correlation have been recognised.

At the opening of the century, the Wernerian system of classification was adopted. In this classification, the mineral character of the formations was regarded as fundamental.

The second stage took definite shape in the New York system, and while a general "parallelism of strata" was believed in, "gaps" and "intercalations" were assumed, to make the interpretations fit the facts. Fossils played a secondary part, only being considered of value when exact identity was recognised. This principle did not reach satisfactory results, because stratigraphic order and stratification itself offer no intrinsic evidence of the age of a formation, and stratigraphic structure was found not to be uniformly persistent even for a few miles' extent.

William Smith, early in the century, advanced the idea that strata could be identified by the fossils they contained, and we need not trouble the reader with any proofs of the value of this.

Hence in the third stage of the correlations methods, fossils assumed the chief *rôle*, and the minute and exhaustive study of organised beings in their stratigraphical and geological relations has proved to be, not merely the best, but the only reliable guide to correlation of geological formations.

The conclusions reached from these historical studies confirm the belief that the description and nomenclature of structural formations should be quite independent of their correlations, and that precision in correlation must be based upon mature and exhaustive palæontological study, that the time-scale must be made independently of the structure-scale, and that the time-scale of correlation is based fundamentally upon biological data.

The investigations also lead to the further conclusions that, as nomenclature finds its basis in some intrinsic characters of the things named, *uniformity of nomenclature* for formations is impracticable, since the intrinsic characters of formations are local, and have nothing to do with their geological position; and that *uniformity of classification* can be looked for only through an exhaustive biological study of fossils, and is inapplicable to geological structure; stratigraphy, or formation.

When it is mentioned that the Archæan paper occupies 550 closely printed octavo pages, it will be seen that the amount of labour expended on these Reports is prodigious. They are most valuable to the geological student on both sides of the Atlantic, and we regret that space does not allow us to give a detailed account of the several monographs.

GEOLOGICAL SURVEY OF QUEENSLAND.

THE annual progress of the Geological Survey in this vast territory is not to be estimated by the number of square miles mapped; it must be judged by the separate detailed reports on areas of special economic interest. The work, which is under the superintendence of Mr. Robert L. Jack, Government Geologist, is well organised. The headquarters of the Survey have been removed from Townsville to Brisbane, and there is placed the collection of minerals, rocks, and fossils. There the officers, when not engaged in field-duties, are employed in writing reports, in preparing geological maps, in laboratory work, and in affording information to miners and others. Plans of old mines, wherever possible, are preserved. In some cases it is difficult to find out anything about the earlier gold workings; no plans have been kept; and yet, in certain instances, the workings, not rich enough to pay formerly, may be again approached when cheaper processes for treating the ore come into use. A most important work will thus be done by keeping a permanent record of mining operations.

Among the Reports which we have received are the following:—The Kangaroo Hills Silver and Tin Mines, by Mr. Jack; Geological Observations in the Cooktown District, by Mr. W. H. Rands, with accounts of Coal, Gold, Antimony, and Tin; and Grass-tree Gold Field, near Mackay, by Mr. Jack. These Reports and the Maps can now be obtained in London at the Office of the *British Australasian*, 31 Fleet Street.

THE INHERITANCE OF ACQUIRED CHARACTERS.

OWING to their coiling, the shells of Ammonoidea and Nautiloidea have furnished biologists with much evidence bearing on theories of evolution, and now Professor A. Hyatt has discovered yet another point, which, he claims, proves that acquired characters have been inherited (*American Naturalist*, vol. xxvii., p. 865). Coiled Nautiloidea have, as everyone admits, been gradually derived from straight forms, such as *Orthoceras*. The cross-section of an Orthoceran, or even of a slightly-curved Cyrtoceran, or loosely-coiled form, is circular or elliptical; but the section of a close-coiled form, like *Nautilus*, shows an impression of that part which comes in contact with the preceding whorl, so that there is a re-entrant curve. In old age, however, when the shells again uncoil, this impressed zone disappears, and the section becomes circular again, a fact which seems to show that the feature is directly due to pressure, and is, therefore, an acquired character. As such it is not found in the early, uncoiled stages of those Nautiloidea that are close-coiled in the adult; at least, it is not so found in any of the Silurian or Devonian species. But at last, in the Carboniferous, Professor Hyatt has found a species that seems to prove his point; for, in *Coloceras globatum*, which is in many respects a highly-specialised

species, seven specimens examined have shown this impressed zone existing while the shell was still in the partly-curved or cyrtoceran stage. The same early appearance of the impressed zone is likewise seen in numerous Jurassic, Cretaceous, and Tertiary species, including the living *Nautilus pompilius*. This, then, seems due to the inheritance of a character in successively earlier stages of individual development, according to a well-known law, while the character so inherited is believed to be an acquired one.

Nuclear Division.

In the *Annals of Botany* for September (vol. vii., pp. 393–397), Professor J. B. Farmer publishes an interesting preliminary note on nuclear division in the pollen-mother-cells of *Lilium Martagon*. The processes of karyokinesis in animals and plants now are recognised to be so similar as to suggest identity in cause, and zoologists as well as botanists will read this note with interest. It tells of the discovery of multipolar spindles in cell-division. These spindles stand in obvious relation to granules not unlike centrosomes and are placed at their poles, and there is some reason to infer that these granules in the cytoplasm have been derived from the nucleolus. Of course, multipolar spindles have been observed before, both in plants and in animals, and Hertwig and others have asserted a nuclear origin for the centrosome. But the full publication of Mr. Farmer's results will be awaited with interest, as aberrant cases like this frequently call attention to important details obscure in normal types.

The Flora of Eastern Malaya.

The Flora of the Eastern Coast of the Malay Peninsula forms the subject of the latest number of the Linnean Society's *Transactions* (vol. iii., part 9). Mr. H. N. Ridley, during the past few years in which he has held the post of Director of the Gardens and Forest Department, Singapore, has made some collecting trips on the eastern side of the Peninsula, the Flora of which was practically unknown; the researches of the earlier botanists like Griffith, Wallich, and Scortechini, and of the later collectors, having been confined to the more accessible western side. Mr. Ridley collected chiefly in the native State of Pahang, but also visited the more northern States of Kelantan and Tringganu, and the island of Pulau Tiuman, off the coast of Johore. The number of species collected and observed exceeds 1,200, but this represents only a small proportion of the whole flora, as opportunities of collecting were often limited, while many of the trees and shrubs were not in flower or fruit at the time, and did not therefore afford material for identification. It is suggested that three times this number would not be an over-estimate of the species existing in the area traversed. Those obtained give a fair representation of the lowland and coast flora, and of that of the

sub-alpine woods of the Tahan district and of the limestone rocks of Kota Glanggi.

There is a very marked difference between the floras of Singapore and Pahang. Many plants which are common in the southern part of the Peninsula, and especially the secondary jungle plants, become scarce or disappear in Pahang, while there is a large accession of Bornean types. The latter, Mr. Ridley is inclined to regard rather as the relics of an older flora common to the whole of this region, than as forming part of a distinct Bornean flora.

The littoral flora was best seen at the mouth of the Pahang River. Above high-water mark on the sandy sea-shore, was a single row of Casuarinas (*C. equisetifolia*), on the branches of which were many lichens and fungi and a fine orange *Loranthus*, which is described as a new species. The ground beneath was carpeted with many grasses and sedges, Ipomœas, etc. The most striking were the porcupine-grass (*Spinifex squarrosus*) and a beautiful prostrate, blue-flowered *Vitex*. The mangrove-swamps proved less extensive than in most parts of the Peninsula, as, owing to the sandy character of the country, the rivers do not bring down the requisite mud.

Passing up the river towards the capital, Pekan, one enters a great heath district, consisting of flat, open, sandy country, dotted over with thickets and clumps of bushes, alternating with grassy patches. Here were found many interesting grasses and sedges, and among the bushes or trees Eugenias, *Ilex*, several figs, and, growing upon them, many ferns and Loranths, and some small orchids. On the right bank of the river at Pekan, the country is more swampy and less sandy, and large tracts are covered with a new species of *Saccharum*, and great tufts of a palm (*Licuala spinosa*). There are many pools full of lotus, while, in drier places, a *Clerodendron* and a small *Crinum* abound, and on the river banks dense thickets reach to the water's edge, interspersed with pink- or golden-flowered Cassias, orange and red Bauhinias and clumps of white-flowered *Clinogyne*.

The Kota Glanggi limestone rocks in the woods of the island of Tawar afforded a distinct flora, and many good finds, the most important of which were a new genus of Musaceæ, one of Rubiaceæ, a *Trichopus* hitherto known only from Ceylon and Southern India, and many new orchids.

Among the numerous interesting plants found in the woods at the mouth of the Tembeling River, at its union with the Pahang River, was a *Brugmansia*, the first of that most remarkable of orders, Rafflesiaceæ, recorded from the Malay Peninsula. Passing up the river to the valley of the Tahan, a totally distinct flora was observed. Here, the rocky river-banks are clad with a dense forest of trees and shrubs, most conspicuous of which was a new Dipterocarp, bright with pink fruit, or scenting the air with large pinkish, cream flowers, while its boughs were laden with *Cœlogynes*, *Dendrobiums*, and many other

orchids in magnificent flower. A long, narrow-pointed leaf was very characteristic of the plants growing close to the water's edge, and belonging to very different orders. The author suggests that this may be a protective adaptation against the rapid rises to which the river is subject, from sudden falls in the mountain districts, thus exposing the plants to submersion by a rush of water. Broad foliage would be torn off or mutilated, but narrower leaves, offering less resistance, would be less liable to injury.

In the ravines, down which run the numerous smaller streams to join the river, were Begonias, Aroids, many Scitamineæ, and others. The soil of the woods is in many parts very sandy, and Mr. Ridley thinks that to this is due the paucity of *Termites*, as they are unable to make their subterranean nests in sand which would fall in, the stiffer clay being needed for the dome-shaped chambers and passages. Owing to their absence, the leaves and sticks on the ground decayed slowly and formed a richer soil, while in the clayey woods where white ants abound, the vegetable fragments are rapidly eaten as they fall and rendered useless for soil-fertilisation. In the upper woods of the Tahan River, the ground was permanently sodden with the heavy rains, and at night glowed brilliantly from the luminosity of the decaying leaves. The dense jungles of the Tahan River yield a good quantity of Rattans of many species, but in the more accessible country along the banks of the Tembeling and Pahang rivers, the best have been exterminated. Gutta-trees are plentiful in the Tahan districts, and Pahang gutta-percha fetches a good price.

There was very little cultivation in Pahang, although the soil in many parts is exceptionally good, much better than that to the south of the Peninsula. In the villages along the main river, maize, tapioca, sago, hill paddy, and fruit are chiefly cultivated. The Rajah of Tembeling had some very healthy young Arabian coffee trees in his garden, from the leaves of which he made a kind of tea, being quite ignorant of the use of the berries.

Mr. Ridley's list includes descriptions of three new genera, in Rubiaceæ, Asclepiadeæ, and Scitamineæ respectively and there is also a goodly number of new species. Thus of eighteen Cyrtandreæ, nine are new, seven of which belong to the genus Didymocarpus, many of the Scitamineæ are new, and no less than twenty-one orchids, including three *Dendrobiums*, four species of *Sarcochilus*, and two of *Phalænopsis*. What seems to be the wild original form of the Patchouli was found a long way up the Tahan River, far away from cultivation; it is quite distinct from the other plants which have been supposed to be the wild parent.

The Seedlings of Conifers.

The last two numbers of *Le Botaniste* consist largely of an account of some anatomical researches on the seedlings of Conifers by the

editor, P. A. Dangeard, who claims, among other things, to have put beyond doubt the existence of an intermediate tigellar region distinct from both stem and root, often reaching above the cotyledons, and "possessing a veritable autonomy." He also discusses the varying and often large number of cotyledons in Gymnosperms, which contrasts so markedly with the pair or single one found in the dicotyledons and monocotyledons respectively.

Thus the stone pine (*Pinus pinea*) has 11 to 13, the corsican pine (*P. Laricio*) 8, in the scotch fir the number varies from 6 to 9, in the larch there are 5, 6, or 7, in the white spruce (*Picea alba*) 5 to 7, and so on. One theory explains the large number of cotyledons by the development of new leaf-structures between the original members, while another supposes the subdivision of already existing cotyledons.

The former idea is recalled by the study of the germination of the cypresses. In *Cupressus Lindleyi*, for instance, the internode immediately above the three cotyledons is much shortened, although the following ones are well-developed. The first whorl of three leaves is thus brought on a level with the cotyledons, of which the seedling seems to have six, the three true cotyledons being distinguishable only by their smaller size. Similarly, *Cupressus Corneyana* has apparently four cotyledons. It is, therefore, conceivable that by a gradual approach of the origins of the seed-leaves and those of the succeeding whorl a permanent increase in the number of the former might be effected. Anatomy, however, does not favour this view, as in the cases examined, only the vascular bundles belonging to the cotyledons are inserted on those of the primary root, and M. Dangeard is of opinion that the increased number is due to the division of two large cotyledons, and he derives the numerous cotyledons of the Abietineæ from *Araucaria* or some closely-allied ancient genus. Examination of the seedling of the Chili pine or monkey-puzzle (*Araucaria imbricata*) will easily show how the transformation has been produced. Each of the two cotyledons has a small number of nerves which unite at the base of the leaf into a single bundle; these nerves have become independent and formed as many distinct cotyledonary bundles in the axis, while the limb of the two large cotyledons has split between each bundle, forming as many lobes. The mode of germination thus constructed recalls strikingly that characteristic of the Abietineæ, and of *Pinus* in particular. Variations occur in the genus *Araucaria* which favour this idea; the total number of nerves, instead of being distributed between two cotyledons, may be divided among three, or sometimes four; while, on the other hand, if we examine the course of the cotyledonary bundles and their relations with those of the root in a seedling of the stone pine, we observe, when the number of cotyledons is odd, a fact which recalls the original union of their bundles; some namely uniting with others and not being directly inserted on those of the root. The author remarks that this theory is at any rate not contradicted

by what we know of fossil Conifers; *Walchia*, a genus allied to the subgenus *Eutacta* of *Araucaria*, appears in the Upper Carboniferous, while the pines begin only at the Lias. On the other hand, it is generally agreed that the Abietineæ show close affinities with the Araucarieæ.

THE RESPIRATION OF PLANTS.

IN Pringsheim's *Jahrbücher* (vol. xxv., p. 1) there is an account of some investigations by Anton Amm on the intramolecular respiration of plants. With regard to the relations between the amount of carbonic acid produced in this function and the degree of temperature to which the plants were exposed, it was found that the minimum temperature, as in the case of normal respiration, was below freezing point, since at 0° C. a significant amount of the gas was given off. As the temperature rose, intramolecular respiration also gradually increased, but this increase was not proportional to the rise of temperature. In both Wheat and Lupine seedlings the optimum was reached at 40° C., which coincides with the optimum for the normal process. On the other hand, while there is, doubtless, a maximum temperature for the latter function, in the case of the wheat plant and *Lupinus luteus* somewhere about 45° C., there can, properly speaking, be no such point in the intramolecular process, since in absence of oxygen, seedlings cannot stand temperatures between 40° and 45° C. without prejudice to their vitality, and the rapid fall in the respiration curve when the optimum temperature is passed is due to the commencement of death.

The author finds the relation between the amounts of carbonic acid formed in the normal and intramolecular processes to vary with the temperature. Thus in the case of the wheat the relation $\frac{J}{N}$ decreases in passing from 0° to 25° C., and then increases regularly up to 40° C. The values obtained with the Lupine, except for some fluctuations, show a similar fall and rise; the minimum is, however, reached at 35° C., not 25° C.

The relation between the amounts of carbonic acid gas formed in the two processes varies in different stages of development of one and the same plant, the fraction $\frac{J}{N}$ increasing with increasing development. The present investigations have also supplied fresh confirmation of the fact that, by the withdrawal of oxygen, production of carbonic acid at once sinks in amount but remains constant for a long time at the lower level, and immediately rises again to the original amount when oxygen is again supplied.

Finally, the results show that the different organs of a plant, *e.g.*, flowers and leaves, give an almost identical relation between the normal and intramolecular respiration, while the organs of different species show quite a different relation.

The paper includes an account of the apparatus and methods used in these researches, as well as an historical review of the subject.

THE GROWTH OF PLANTS.

NATURE does not pose as a graceful designer in the rectangular inflorescence of *Siphonychia diffusa*, according to the description which Aug. F. Foerste gives of it in the *Bulletin of the Torrey Club*. "Here is a plant," he says, "with its flowers laid out in rectangular inflorescences—cymes with a flat top with quadrate outlines, or of a form which at once makes us wish to say parallelopipedon, as though the other dimensions were equally stiff and rectangular. In these rectangles, the flowers are laid off almost with the precision of corn rows in a field." This general effect is not lessened by the way in which the inflorescences are arranged on the stem. The latter is almost prostrate, and the cymes terminate the branches at such heights as to fall approximately all in the same plane, and being moreover disposed with their diagonals vertical or parallel to the stem, their sides all become parallel, "so that the final effect is that of a series of rectangular fields, set out in some western landscape, where all lines run north and south or east and west." The writer adds, "the plant must be seen to be appreciated."

The same author also calls attention to the renewed growth of trees in summer after having already once formed their terminal scaly winter buds. In the formation of the buds the plant is planning for the future; in many trees, the warmth of spring has hardly called the vital functions back into vigorous action before the growth for the year is completed, and a few weeks later a well-developed terminal scaly bud awaits the winter. Considerable maturing may be needed before all the characters necessary to withstand the winter's cold are acquired; but the fact is evident that "the more terminal leaves have remained in the crude state of scales when all the freshness of spring was inviting them on to full development to vigorous leaves."

The buds represent a year's growth, and before this year's is finished the tree begins to prepare for the next year's task. Hence there is a certain definiteness to the work, and we can foretell in a measure how much the plant will do from year to year. This definiteness must be related to conditions of climate found in certain areas of the plant's distribution. Northwards the relations between the number of leaves necessary for vigorous development, and the provisions for the same, are so well balanced, that it is rather rare to find woody plants renewing their growth after having once formed their terminal buds. These correlations, however, lose in value in going southward, and many trees, after having already formed such buds in the spring, start growing again in the summer, and again form terminal buds. Under exceptional conditions this may take place three or four times in the course of a year. In the black-jack oak (*Quercus nigra*), near Bainbridge, in Georgia, the author found cases of repeated renewal of growth very common. In the older trees such a growth has taken place twice this year on

certain branches, while in young shoots coming up from the roots it has been quite general. In one case, where a fire had checked development for some time by destroying the present year's growth, adventitious shoots formed scaly buds three, or even four, times. It was interesting to notice how nearly equal was the number of internodes developed each time as a striking proof of the fact that this oak has so deeply acquired the habit of producing a certain number of internodes and then a terminal bud, that it repeats this growth and termination at times when circumstances favour a single longer-continued growth before winter buds need be prepared.

Another phase of the subject was presented in some new shoots from a hickory stump, a few of which had made their long growth during one uninterrupted period. In one shoot the internodes got closer together, and the leaves smaller towards the middle of its length—evidently preparatory to the formation of a terminal bud; but the shoot, so to speak, changed its mind, and succeeding leaves became more distant and larger until, later, the real terminal bud was formed considerably farther on. In another, the leaves were reduced to scales towards the middle of the shoot, but the internodes were too long to admit of the formation of a bud, while in several cases terminal scaly buds had been developed but renewed their growth.

The Movements of Plants.

Cobæa scandens is well-known in conservatories and gardens as a graceful climbing plant. It is a native of Mexico, but has been in cultivation for more than a century. In 1805 it was figured for the *Botanical Magazine*, where it is stated that the drawing was taken in July, 1784, at Mr. Woodford's, Vauxhall. It had been previously described and figured by Cavanilles in the first volume of his "Icones," and appears to have been first raised in Europe in the royal garden at Madrid. The movements of the flower-stalk and floral organs during the flowering period form the subject of a recent paper by Max Schultz in Cohn's *Beiträge zur Biologie der Pflanzen* (vol. vi., p. 305).

During the development of the flower-bud the stalks have a strong tendency to grow towards the light and in opposition to the force of gravity, or, to put it briefly, are positively heliotropic and negatively geotropic. The axis of the young bud falls in the direction of the length of the stalk. As growth proceeds, the end of the stalk bends horizontally, and at the same time the pressure of the petals causes the hitherto fast closed calyx to open at the tip. Finally, through the more rapid growth of its upper surface, the horizontal portion dips at a slight angle, giving the flower a nodding position in which it opens. The author shows that these movements are due to the action of gravity, the end of the stalk becoming horizontally

geotropic, tending, that is, to lie in a direction at right angles to the force of gravity, while the rest still retains its original negative geotropic tendency.

The flowers are markedly proterandrous. The two upper stamens are the first to shed their pollen, the anthers standing before the entrance to the flower. They remain in this position for about twenty-four hours, and then bend back towards the interior of the flower, at the same time that the anthers of the three lower stamens open, and by curving of the ends of their filaments come to hold the same position at the entrance. After another day these have also served their turn and bend back useless towards the lower edge of the flower, while the style, which has hitherto lain concealed, grows out and curves into the position vacated by the stamens, the stigmas spreading meanwhile. A change has also occurred in the colour and scent of the blossom. While the two upper anthers are functional, the corolla is still greenish and has a pungent, disagreeable smell, but after the three lower anthers have opened, the colour is a deep dull purple, and the scent strong and sweet.

After the anthers have retired and the stigmas remained in position for another day, the upper end of the flower-stalk bends vertically downwards, and shortly after the lower portion comes to lie horizontally. Both these movements result from the action of gravity. Simultaneously with the latter movement, the upper portion undergoes further geotropic changes of position, by which the part immediately behind the flower comes to point vertically downwards, the piece between this and the original place of bending remaining horizontal. It is interesting to note that the horizontal part of the stalk has a dorsiventral structure, the cortical tissue of the ventral side being more strongly developed than that of the dorsal.

These movements of the flower and its stalk are, perhaps, a device for ensuring self-fertilisation where cross-fertilisation by insects has failed; they occur in any case, whether pollination has taken place or not.

Malformation of the Daisy.

A novel malformation in the flower-head of the common Daisy is the subject of a note by Dr. Masters in the last issue of the *Annals of Botany* (vol. vii., p. 381). The specimens were sent the summer before last as a "new British plant"; they were kept through the winter and on flowering this year have reproduced the peculiarities previously observed. The young flower-head was oblong, and the bracts of the involucre fewer in number and less wide-spreading than usual. The outer or ray-florets were of the usual colour, but only five in number; some were spreading, others erect or more or less twisted. By far the most striking peculiarity, however, lay in the disc, where, instead of a great number of separate corollas, was a single petaloid cup composed

of several corollas, apparently flattened out and united by their margins. The free border of the tube showed lobes and other signs of a composite nature. Within the cup were the stamens in a single row, very numerous, quite free, and surrounding a club-shaped expansion of the axis which occupied the centre of the flower-head. This dilatation was solid and undivided below, but above gave off a number of deltoid processes, "which doubtless represent bracts or paleæ." In no case, says the writer, was there any trace of styles, ovary, or ovule, except in the ray-floret, which enclosed a two-lobed style as usual. One is tempted, however, to suggest from Dr. Masters's figures and description, that the central head of "bracts or paleæ" might represent the carpels, which have, like the corolla and stamens, become aggregated and then also leafy in character.

It is saying a great deal for the rarity of this phenomenon when Dr. Masters admits that he has never before observed such a case in the family of Compositæ. The nearest approach is a very common malformation of the foxglove, where the corollas of the flowers in the upper part of the raceme are blended into one terminal cup.

Lepidostrobus.

Professor F. O. Bower has been examining in the light of recent advances in Palæophytology the structure of the axis of the cone of *Lepidostrobus Brownii*. This fine fossil specimen, preserved in the British Museum, was the subject of a paper, by Robert Brown, in vol. xx. of the Linnean Society's *Transactions*, where the author, as Professor Bower points out in his communication to the *Annals of Botany* (vol. vii., no. 27), described details of great importance in classification which have been neglected by subsequent workers. In the paper now before us, attention is called to the close correspondence of the tissues to those of *Psilotum*, both as regards arrangement and structure; while in the stellate, connected central xylem, the fossil bears a certain resemblance not only to the Psilotaceæ but also to species of *Lycopodium* and *Selaginella*. As the author has recently demonstrated points of similarity between the sporangia of the fossil genus *Lepidodendron* and those of *Tmesipteris*, this correspondence in internal anatomy between *Lepidostrobus* and the Psilotaceæ becomes especially interesting.

In a comparison of the cortex of living Lycopods and Psilotaceæ, of the *Lepidostrobus* and stems of *Lepidodendron*, the author shows that while considerable variety of detail is manifested, it is possible to match the different types of structure found in the fossils with similar characters in closely-allied living forms. He also concludes, by a consideration of another set of examples, that the well-known triarcular development in *Selaginella*, traces of which also occur in *Lepidostrobus*, is a specialised and more definite example of that

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lacunar development which appears in such various forms and positions in cortical tissues of other Lycopodinous plants.

All these considerations, the author remarks, serve to draw the Lycopodinous plants of the present and past more closely together as a natural family, while it is interesting to note that the lines of similarity "do not focus themselves specially between any two genera, but are such as to suggest complex cross-relationship between the several representatives of this very natural series."

B. H. Halsted has been investigating (*Bull. Torrey Botanical Club*, October) a disease which has, in the last two or three years, become very common among hot-house Pelargoniums in the United States. The leaves lose their healthy green colour, and become speckled or blotched with yellow, while corky ridges are found on the stem and petioles, and the whole plant may become sick, stunted, and useless. The usual appearance on the blades is that of numerous specks, which seem to be supercharged with water, giving the part a clear amber look when held up to the light. A similar appearance in Carnation leaves, previously studied, was known to be due to micro-organisms, but, on investigation, no trace of bacteria or infection by inoculation could be found in the Pelargonium, and it was concluded that the plants were suffering from a dropsical affection due to excess of water and insufficient light. Professor Atkinson has described a similar trouble which he calls Œdema of the Tomato. The remedy would seem to be a cooler, drier soil and increased light for the aërial parts wherever possible.

An explanation of the want of accord in the results of estimation, by methods hitherto used, of theobromin and caffein in specimens of tea and coffee, preparations of cacao and cola nuts, is supplied by some recent work by A. Hilger and other chemists, on the cola nut and seeds of the cacao. In both of these there exists a nitrogenous glucoside, from which caffein or theobromin respectively, besides other compounds, are produced by action of a diastatic ferment, also present in the seeds. The authors succeeded in isolating both the glucoside and the ferment. To get trustworthy results in the estimation of theobromin or caffein, the glucoside must be completely split up before these bodies are isolated. In the cacao it is broken up into theobromin, dextrose and a non-nitrogenous body, cacaoroth, while with the cola nut an analogous reaction obtains, caffein and kolaroth being formed. For details, the reader is referred to the *Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege*, vol. xxv., pt. 3.

In the *Annals of Botany* Mr. E. H. Acton gives the result of an analysis of wheat grain grown near Canterbury nearly thirty years ago, and threshed in 1892, and compares it with some of last year's

crop from the same field. As was to be expected, the old grains contained much less water than the new, the proportion being 9 and 14 per cent. respectively, while the insoluble food-material stored in the seed, such as proteids and starch, were found to have changed considerably in the direction of compounds soluble in water. Thus whereas in the new wheat scarcely one-seventh of the proteid matter was thus soluble, in the old nearly half dissolved, and while no sugar was found in the new, and only just over 1 per cent. of dextrins, the old contained 6·2 per cent. sugars and 6 per cent. dextrins; with, of course, less starch. These changes are probably the result of the action of ferments which were originally present, but have since perished, as the power of converting starch or proteid was found to be nil. This being the case, it is not surprising to find the author describing the old sample as "apparently dead," having shown no signs of germination under favourable conditions maintained for two months. In the absence of ferments to break up the insoluble albumen, starch, &c., the supply of food and energy required for germination would be unattainable.

NATURE lovers in our own country are often pained by the vandalism of so-called naturalists, who, possessed with the rage for collecting, threaten the existence of our rarer plants and animals. Now, according to the *Orchid Review* (November), the Rajah of Sarawak is closing his dominion to collectors, owing to the depredations committed by orchid hunters and the like. It is scarcely to be wondered that a man objects to have the rare and beautiful objects of the fauna and flora of his country carted off wholesale to gratify the passing whim of a moneyed class in another hemisphere.

MR. G. F. SCOTT ELLIOT is again in Africa on a botanical and generally scientific expedition. This time his destination is Uganda, which he hopes to reach *viâ* Mombasa and the Victoria Nyanza. Mr. Elliot, who is known to readers of NATURAL SCIENCE as well as to botanists in particular, has already touched the great continent at several points. A few years ago he made a trip through the Transvaal and Natal, and shortly after spent some time in Madagascar. His next visit was to the North, where he was anxious to work at the Morocco Flora, but, owing to political disturbances, was unable to get into the State, and so went on to Egypt, and collected up the Nile, as far as the Wady Halfa. Finally, about two years ago, he visited Sierra Leone as botanist to the Boundary Commission. The scientific results of these expeditions have been published in the Linnean Society's *Journal* and the *Annals* and *Journal of Botany*. Uganda is a promising field, and we wish Mr. Elliot a successful trip and a safe return with a rich harvest of results.

MR. ERNEST GEDGE, whose admirable letters to the *Times* as Special Correspondent of that paper in Uganda are, doubtless, well known to all our readers, has just returned to the coast, and is now on his way to Matabeleland. Mr. Gedge's hasty return is to be regretted, as otherwise he would, probably, have carried out his original programme and visited the unknown but important region between Elgon and Karamoyo on his way back to the coast.

MR. HERBERT WARD has an article in *The English Illustrated Magazine* for November entitled "Martyrs to a New Crusade." He gives a brief sketch, with portraits, of those of Stanley's companions who passed away, either during the last expedition, or from privations endured while attached to it. Major Barttelot, Thomas Heazle Parke, James Sligo Jameson, Robert Nelson, and Captain Stairs, are pathetically sketched by their comrade, who also pays a tribute to the fidelity of the black followers.

THOSE interested in slugs will find in the December 21 number of *The Conchologist* the completion of Professor Cockerell's Check-List of those molluscs. It is difficult to estimate the value of these publications, where one can find forms described in widely-scattered publications all collected together for the first time. The author knows as well as his critics the weak points in his list, and offers it for destruction and re-construction, until reasonable finality is reached and permits of a revised edition.

The editor, Mr. W. E. Collinge, has made sundry comments upon the notes of the author, and these, though in many cases apt, are testy, and do not, moreover, justify the insertion of Mr. Collinge's name on the title as joint-author. One remark of Mr. Collinge's at first strikes one as very sound, viz., that modern science "demands a knowledge of internal as well as external morphology, and *rightly refuses to recognise inadequate descriptions, or descriptions of shells apart from the animal, or to acknowledge genera or species founded upon purely external features.*" But, however much we may deplore the naming of a shell without study of the mollusc which produced it, we must not forget the difficulty in most cases in obtaining the animal, and it is a debatable question whether it is not better to name the shell when found, rather than wait for the soft parts to be forthcoming. In any case, we hope that Poli's idea of naming both the shell and the mollusc will not again come into favour.

THE siphuncles, or, to speak more correctly, the siphuncular tubes of the straight forms of Palæozoic Cephalopoda present many variations in structure that greatly puzzle the morphologist. In some interesting "Remarks on Specific Characters in *Orthoceras*" (*Amer. Geol.*, vol. xii., pp. 232–236), Aug. F. Foerste adds an item to our

knowledge by showing that in *Orthoceras erraticum* the siphuncle moved, during the growth of the shell, from one side to the other, while the siphuncular tube became less annular and more cylindrical. Similar movement of the siphuncle is known in coiled Ammonoidea, and may be accounted for by the coiling; but one fails to see its meaning in a straight and regularly conical shell.

Another curious instance of variation in Cephalopoda is recorded by A. Appellöf (*Bergens Museums Aarbog* for 1892, p. 14, just published). In all Octopoda one of the arms, usually the third on the right, undergoes a peculiar modification for reproductive purposes, which is known as "Hectocotylisation." In a specimen of *Eledone cirrhosa*, Dr. Appellöf has found the third arm on the left also similarly affected, without, however, any corresponding duplication of the genital opening.

The clever sketches called "Zigzags at the Zoo," published in recent numbers of the *Strand Magazine*, are above the average. We do not remember to have seen anything more funny than the drowning of the two bluebottles by the rhinoceros, "Tom." Mr. J. A. Shepperd has exactly caught the flap of the ears, and his drawings are full of suggestion and life. The comparison of the racoons to a couple of well-known music-hall "artistes," if not in the highest degree scientific, has a sense of humour doubly dear to one whose wits have been much muddled by ponderous verbosity; while no one, after laughing over the illustration, will fail to remember that "*These animals bite.*"

An elaborate memoir, entitled "A Contribution to the History of the Geology of the Borough of Leicester," by Mr. Montagu Browne, has been published by the Leicester Literary and Philosophical Society (*Trans.*, vol. iii., pp. 123–240). It contains a very full account of all that is known about the Keuper and Rhætic Beds, the Lower Lias, and the Glacial Drifts; with notes on the Water Supply. The Palæontological Tables enumerate the fossils found in the several formations in the district, including the "derived" specimens obtained from the Boulder Clay, and those found in Pleistocene valley-gravels and more recent deposits. The authorities for each record are noted by means of a somewhat complicated system of symbols.

An excellent account of the Geology of Dublin and its neighbourhood, from the pen of Professor W. J. Sollas, appears in the *Proceedings of the Geologists' Association* (vol. xiii., pp. 91–122). It is well illustrated, and was intended as a guide to the members of the Association who visited Dublin and Wicklow last July.

WE learn that Professor Huxley will contribute a final chapter to the "Memoirs of Professor Owen," which are promised early next year. This will be an estimate of Professor Owen's work, and the scientific public will await with considerable interest the words of one so peculiarly and eminently fitted to deal with this important subject.

APROPOS of the article in our last number on Natural Science at the Chicago Exhibition, we may mention that the *American Geologist* intends to give very full accounts of all the geological exhibits at the Fair. The October number contains a description of the Geological Maps and Models, and also gives a summary of the proceedings at the World's Congress on Geology, which was held in Chicago during the week August 21 to 26.

IT is announced that *The Conchologist*, a quarterly journal edited and published by Mr. W. E. Collinge, of the Mason College, Birmingham, will be known in future as *The Journal of Malacology*, the first part of vol. iii. being issued in January, 1894. For the present, it will deal almost exclusively with the slugs, and its aim will be to focus within one journal abstracts of the current literature relating to these molluscs, while affording a means of publication for original work.

OUR reviewer, when he wrote the recent notice of the *Zoological Record*, must have been gifted with second sight; at all events, no more apt illustration of his ironical remarks anent the customary mode of reviewing that work could have been afforded than the notice which made its appearance but a day or two later in the columns of a weekly contemporary (*Athenæum*, November 11).

The whole notice is literally taken up in calling attention to those misprints and other oversights from which, owing to human frailty and the direct interposition of the printer's devil, a work of this character is never free. The only suggestion offered by this would-be smart-critic is to repeat his last year's recommendation that the editor of the *Record* should procure a blue pencil and use it. Apparently this editorial requisite is equally wanted in other quarters, for we can hardly imagine a critique (?) such as our contemporary has admitted being allowed to appear in any other journal.

One is tempted to wonder how similar works, did they come up for review, would be handled. What, for instance, would be said about the valuable, and formerly well-edited, *Journal of the Royal Microscopical Society*? In the biological record there published we habitually find notices of papers on invertebrate embryology mixed up with those on vertebrate embryology, under the heading "A.—Vertebrata." Misprints, too, are by no means unknown there, and we have met with an instance where two papers on very different subjects, and pages apart, have politely exchanged references.

I.

High-Level Shelly-Sands and Gravels.

PROBABLY there is no subject of geological controversy that has provoked more animated discussion than the shelly-sands and gravels, found in various localities in the British Isles and Ireland, at levels ranging up to about 1,400 feet above sea-level. The question that excites geologists is the same that is reported to have troubled George the Third respecting the apples in the dumpling—how they got there. Having received a cordial invitation from the Editor of NATURAL SCIENCE to unburden myself on this topic, I gladly avail myself of the opportunity.

And here I am met with my first difficulty. I shall, in the course of this article, have to call these deposits High-Level *Glacial* Drift, which I suppose is begging the question, as their *diluvial* origin is stoutly and ingeniously contended for by no less an authority than Sir Henry Howorth. However, as I simply adopt the name that these sands and gravels are generally known by, I hope I may receive absolution.

Among working geologists of the present day only two explanations are offered as possible.

The older one, which passed unchallenged for over a quarter of a century, is that these shelly-sands and gravels were laid down when the relative levels of land and sea were different to what they are now, and postulates a subsidence of the land or rise of the sea, known as "the Great Submergence."

The newer hypothesis adopted by geologists who find various difficulties which they think are unexplained by the submergence theory, is that the high-level shelly drift is sea-bottom, not *in situ*, but carried up to its present position frozen in the sole of a glacier, or pushed up in front of it while advancing from Scotland over the bed of the Irish Sea.

It is difficult to give the precise details of how it is conceived this operation was carried on, as I have never yet been fortunate enough to meet with much more than a general statement, but one hypothesis is that the sands and gravels were released from their icy matrix by the sudden melting of the ice at the close of the Glacial Period, hence the rounding of the stones and the false-bedding of the deposits.

It is evident to anyone of ordinary common sense in scientific matters that such questions can only be answered—if they can be answered—by appeal to facts; unfortunately—or perhaps fortunately—facts present themselves in different colours to different minds, and what one looks upon as conclusive proof of the action of the sea, another thinks points decisively towards land-ice. Let us, then, shortly review what is known of these chameleon-like drifts, and try to present the information in a way that each reasoner may have an opportunity of forming his own conclusions—if, indeed, he feels equal to the task of taking so much trouble.

The first discovery of the high-level shelly drift in the British Isles was made by Joshua Trimmer, and recorded in the *Proceedings* of the Geological Society in 1831. It occurred on Moel Tryfaen, Carnarvonshire, at a level of between 1,300 and 1,400 feet.

There have been many discoveries in other localities since, but Moel Tryfaen remains classic ground, not alone on account of priority, but because of certain features which differentiate it from the other shelly-sands and gravels, of which I have given detailed descriptions and illustrations in a monograph lately published by the Liverpool Geological Society.[1] The next discovery was made by the veteran geologist, Mr. Joseph Prestwich, of a shelly patch near the Setter Dog, between Macclesfield and Buxton, at a level of about 1,200 feet above the sea. The Macclesfield Cemetery beds, at a level of about 600 feet—consisting mostly of shelly-sands some 70 feet thick—followed, and were described by the late Dr. Sainter and Mr. R. D. Darbishire.[2] Similar shelly-sands and gravels were discovered and described in Flintshire at Halkin Mountain, and up to 1,000 at Moel y Crio, by the late Daniel Mackintosh; and between Minera and Llangollen the same indefatigable observer, to whom Drift Geology owes so much, found an extension of the same shelly drift at levels ranging from 1,000 to 1,200 feet. Still more recently, these high-level shelly-sands and gravels have been traced further south, and occur at Gloppa, two miles from Oswestry, in great force up to the level of 1,200 feet above the sea, having a depth of 60 feet unbottomed. Mr. A. C. Nicholson, F.G.S., has given an excellent description of these beds[3] which, fortunately for geologists, he assiduously worked during the whole of the excavations made in carrying out the works of the new water-supply for Liverpool from the River Vyrnwy. Meanwhile, geologists in the Sister Isle were not idle, and the Rev. Maxwell H. Close recorded the presence of a remarkable deposit of shelly-gravels at Ballyedmonduff on the Three-rock Mountain, near Dublin, at an elevation of over 1,200 feet,[4] and of other exposures at various

[1] The Drift Beds of the Moel Tryfaen Area of the North Wales Coast; Session 1892-3.

[2] Notes on Marine Shells found in the Stratified Drift near Macclesfield, by R. D. Darbishire. Lit. & Phil. Soc. of Manchester, Session 1864-5.

[3] *Q. J. Geol. Soc.*, 1892. [4] *Geol. Mag.*, 1874.

levels of a similar nature, indicating a wide extension of these deposits penetrating into the northern parts of the Wicklow Mountains.

Other minor discoveries of shelly-sands, gravels, and clays have been made at various localities and at varying heights, but the purport of this paper is to deal with the prominently High-level Shelly Drifts, as bearing upon the two opposing theories of their origin.

Premising that I speak from personal observation of all the deposits the history of whose discovery I have briefly sketched out, I may say that, discounting local conditions, they have all certain characteristics in common. They are these—

1. The rocks and materials of which they are composed are largely foreign to the locality, but mixed to a greater or less extent with local materials.

2. Some of the rocks are far travelled and from different directions, but generally from a northerly direction.

3. The foreign materials are often above the level of their parent rock.

4. The pebbles, boulders, and small gravel are to a considerable but varying extent well rounded, a few are striated, some planed.

5. The sands have often all the aspects of marine sands containing a very large proportion of polished and rounded quartz grains.

In cases where local material preponderates, or materials which may have only travelled a few miles, as at Ballyedmonduff, highly-polished quartz grains are often disseminated through the mass, and can be obtained by washing and riddling.

6. As their name indicates, these sands and gravels contain sea-shells, mostly in a fragmentary condition, but often well preserved, especially the gasteropods. In fact, shells in any gravelly beach present the same aspect. The fragments are frequently worn at the edges, and microscopic grains of shell-fragments can be obtained by washing, and distinguished by the aid of the microscope.

7. The local rocks and gravelly materials are frequently angular, though sometimes well rounded—they are, as a rule, more angular than the travelled materials.

8. The gravels and sands are often well bedded, commonly current-bedded.

9. In some cases these characteristic marine beds are underlaid or overlaid, or both, with characteristic glacial till, composed almost wholly of local rocks and materials that have come down from a higher level. Moel Tryfaen is the only case in which I have seen this phenomenon, but I have no doubt there are others could we bare the ground. It can only happen in localities where there are high mountains near, commanding the site of the beds.

10. Finally, the whole aspect of the shelly-sands and gravels is that characteristic of aqueous deposits.

There are other high-level sands and gravels which, though no

shells may have been detected in them, can be identified as of the same nature and origin.

Interbedded with these sands and gravels we find beds of clay and of dirty sand and gravel, but the typical sands and gravels are very clean and require no washing when mechanically separating their mineral constituents by means of sieves of small mesh.

It must not be thought that shelly-sands and gravels are confined to the high-level localities mentioned. On the contrary, they are to be found at all levels from the sea to the highest mentioned. They vary according to locality and the nature of the surrounding rocks, but they are essentially the same deposits, whether found at fourteen or fourteen hundred feet above the sea. For instance, there is a striking similarity between the shelly-sands and gravels of Howth, near Dublin, which rise from sea-level to about 350 feet above it, and the high-level deposits already described at Gloppa, near Oswestry, 1,200 feet above the sea. The foreign rocks they contain differ; at Gloppa they are mostly from the Lake District, Scotland, and Wales, possibly some of the flints may be from Antrim. At Howth the rocks are from the northern parts of Ireland and from the west. They have not, however, been examined to anything like the same extent as in England.

The low-level glacial deposits are differentiated from the high-level by the greater prevalence of Boulder Clays in the former, and these clays are as a rule distinguished by the still greater prevalence of foreign rocks and their more frequent and intense glaciation. These clays also contain shell fragments, and on washing yield just the same mineral grains and rounded and polished quartz grains. The Boulder Clays cover a very large area of the North-West of England. They are the purest at a distance from mountain masses; near them, local materials preponderate.

I have now sketched out the salient features and differences of the high-level and low-level drifts. It would be easy to multiply details, but my object and meaning might then get deeply buried under a mass of what might be called sedimentary information. For anyone who wishes to peruse the subject, there exist papers sufficient to supply continuous reading of the most solid kind for two or three years to come. It would be interesting to know in what state of mind a reader would emerge from such a task, probably his last state would be worse than his first.

By the earlier and perhaps simpler-minded geologists who paid attention to the subject, such as Trimmer, Ramsay, and Lyell, the position of these shells in the sands and gravels not being explicable by hypothesis of cartage or kitchen-middens, carriage by birds or pilgrims, nor even by waves of translation, were accounted for by submergence of the land by subsidence. They were looked upon, in fact, as marine beds *in situ*, their heterogeneous character being due to floating ice, which brought contributions from many localities.

Latter-day geologists coming from the study of glacial deposits in other countries have felt difficulties in accepting this explanation. They have found that an ice-sheet comparable to that of Greenland of the present day best explains the phenomena in the countries in which they have studied the glacial drift, and they seek to apply the same explanations to the Drift of the British Isles.[4] The low-level drifts are more easily dealt with on this hypothesis than the high-level, but as they are essentially of the same nature, it seems necessary for consistency' sake to apply the same explanation to both. There are also certain phenomena which are thought to be inconsistent with the submergence theory, one of them being the position of boulders at a level higher than the parent rocks from which they were derived.

Again, it is said that the high-level drift is very partial and sporadic in distribution, that there is no marine drift in the interior mountain valleys, that there are no raised beaches, that the shells are always broken as if a heavy body had moved over them, that they occur mixed in species in a way that never happens in nature; that the two valves of bivalves are never found united—that they never occur in any bed as if the molluscs had lived on the spot, and finally, the species characteristic of warmer climates are mixed with those of colder climes, such as never occur together in the sea at the present day. Furthermore, the advocates of submergence are asked to produce stones from the Drift with the remains of barnacles or other organisms upon them.

It is also roundly asserted that rocks have never been found north of their origin, and by implication never will be. It is also suggested that, if the sea has flowed over the land to such a depth as the submergers require, deep-sea beds or beds of some sort containing sea-shells *in situ*, like what we find in the so-called Clyde Beds, should be common, whereas they do not exist.

The shading off of the glacial deposits southward is also looked upon as consistent with a land-ice, and inconsistent with a sea-borne origin.[5] These are what we may classify as objections founded upon observation. I think I have catalogued the greater number, but if any are omitted I have not the least doubt that I shall be reminded of them. In the meantime, we will consider another class of objections that are physical and theoretical. The subsidence of this solid land without evidence of volcanic action seems to some minds so improbable as to demand the clearest and fullest proofs before it can be admitted. The worst is, that when one condition is satisfied and paid

[5] What are called striated pavements, in which the striæ in the boulders are approximately parallel, are pointed to as evidence of the passage of an ice-sheet. These are, however, only found, so far as I know, at low-levels in the Boulder Clay. It is considered that floating ice could not have affected the boulders in this way. I do not propose to examine now the further question of bed-rock striation which is a still wider one than the origin of the Marine Drift.

for in mental coin, the price is raised and more demanded. Granted the subsidence, another difficulty looms ahead, it must have been differential, and it is pretty positively asserted that no evidence of any kind exists of subsidence south of the British Channel. Also, if there had been differential subsidence and elevation, faults of glacial date might be looked for in the solid crust, whereas there are none.

These objections as a whole are considered by some geologists serious enough to warrant the introduction of an agent to account for them, of which, unfortunately, we have very little knowledge. This agent is land-ice, and to Greenland we are asked to project our mental vision that we may understand the Glacial Period. I am now neither denying nor affirming the former existence, or probable former existence, of an icy covering to our isles such as is predicated. If, instead of the theoretical possibilities of such a tremendous agent as is postulated being dwelt upon, we were treated to actual facts drawn from the investigation of the Greenland ice-sheet, some of us would have a more comfortable assurance of being on solid ground forming a base for the reasoning process. Unfortunately, in the absence of recorded observations, we have to fall back on theoretical considerations alone. I trust it will not be considered arbitrary, when dealing with an agent possessing such potency for work of varied kinds as is claimed for an ice-sheet, to ask for the fullest proof of its powers. Formerly the proof of the action of land-ice was looked for in the local nature of the materials of the Boulder Clay, which represented the ground-moraine, together with the preponderance of planed and striated stones. Now everything in the Drift—foreign rocks, shells, local materials, rounded pebbles and boulders, sea-sands, are all claimed as the effect of land-ice, whether found on the top of a mountain or in the bottom of a valley.

There is a system of land-ice physics worked up with astonishing ingenuity and skill by which all these puzzling phenomena are theoretically, if we admit the premises, fully explained. If delicate shells are found intact in the Drift at an inland locality such as Gloppa, forty miles from the sea, it is suggested that they have been conveyed over hill and dale simply frozen in the sole of the glacier, forgetful of the fact that the generally comminuted condition of the Drift shells had previously been claimed as a proof of the passage of land-ice across a sea bed. Other materials, such as foreign rocks, have been conveyed in the heart of the glacier, and the whole—rocks, sea-sand, shells have been washed out, rounded and deposited in stratified masses on the sudden melting of the ice which is supposed to have occurred at the close of the Glacial Period. Unfortunately, no instances are quoted of any analogous kind of work performed by existing glaciers or ice-sheets. It is of this dearth of examples that geologists accustomed to the methods of investigation pursued by Hutton and Lyell have a legitimate right to complain. We should all be glad to assent to these various propositions if adequate proof were fo[illegible]

coming, but unless some more solid food is vouchsafed us, I fear many will die unconvinced.

With so much theory and more hypotheses we might reasonably expect the theoretical physics of the ice-sheet to be quantitatively worked out. But have they been? Has anyone yet, in proof of the view that the shell-beds of Tryfaen have been pushed up from 1,200 to 1,400 feet above the level of the sea by a glacier advancing from the north over the Irish Sea bottom, calculated the thickness of the ice required at the "area of greatest precipitation" to overcome the glacier descending from Snowdonia. As concrete examples cannot be quoted of such work being performed by land-ice either in Greenland or elsewhere at the present moment, it becomes important for the support of the hypothesis that its theoretical possibility should be established. If this has been done the investigation has not come under my notice, though I have been studying glacial geology well-nigh a quarter of a century.

Unknown agencies are very unsatisfactory things to deal with scientifically. It is as easy to deny as to affirm, but as the affirmations seem to bottom upon the alleged impossibility of any other agent than an ice-sheet being able to produce all the complicated phenomena of the Drift, it will be the most reasonable procedure to examine also the validity and force of the various objections which have been urged against the older theory of submergence, and which I have in the preceding pages attempted to summarise.

We will deal with the objections to the submergence theory seriatim.

1.—*Boulders, shingle, and gravel are found above the level of the rocks from which they were derived.*

This by some is considered conclusive proof that the moving agent must have been land-ice. Here, again, we want satisfactory examples of an analogous kind drawn from existing glaciers. It seems to me much more probable that a glacier such as the Irish-Sea glacier is supposed to have been, would shear in its own substance, than that it would push sea-bottom before it uphill. The same reasoning applies to the supposititious case of the sea-bottom getting frozen into the sole of the glacier, for if this were to take place the obstruction to the movement of the ice over the sea-bottom would be increased. We may also ask where the shearing took place if not in the ice itself? It is difficult to conceive a sea-bottom frozen solid shearing under such stresses. Alternate thawing and freezing of the bottom may be suggested to meet the difficulty, but this would render the carriage of shells in the sole of the glacier without breaking more hard to conceive than ever. But may it not be justly asked whether resort to all these ingenious suppositions is not a sign of weakness?

Let us now consider what effect submergence would have. A study of any of our shores will yield ample proof that the sea works

material shorewards, and throws it up sometimes out of its own reach. In a subsiding area, material might in this way be worked up a considerable distance above its original level. I believe the sea alone is capable of doing this, but assisted by shore-ice it would be more efficacious, and would account for some of the striations on the stones.

The material in which the erratic stones is embedded is unmistakably shore sand.

The sand of Tryfaen could not have come from anywhere but the sea itself. It also has been worked uphill, for there is no local material to yield it, and the wear and polish of the grains are distinguishingly marine features.

To anyone who has paid attention to this part of the subject, its marine character is unmistakable. The rocks that have been worked uphill are, as a rule, water-worn and rounded.

2.—*The High-level Shelly Drift is partial and sporadic.*

If the high-level drift were due to submergence, say the land-ice theorists, we should find it all over the hills, whereas it is confined to a few elevated places. This seems to me to be a singular argument. According to the postulate, the whole of the Drift above sea-level has been pushed up. Lancashire and Cheshire are pretty nigh covered with Drift. So much so, that almost every boring made into the solid rock through it involves more or less change in the mapping of the Triassic beds.

This is no disgrace to the surveyors who mapped them; they could not see through the Drift—even if Drift-geologists can? If, then, the ice-sheet only leaves sporadic deposits, why is not the Drift to the height of, say, 400 feet sporadic instead of continuous? Whether placed there by an Irish-Sea glacier or by the sea itself, both high-level and low-level Drift obey the laws of gravitation, consequently the most material is found at the lower levels.

In working out this objection, the supporters of the Irish-Sea glacier unconsciously minimise the quantity of high-level Shelly Drift—there is much more in existence than they have persuaded themselves to believe. There is no occasion for me to name the localities over again, as I have stated them at the commencement of this paper. Again, it can hardly be expected that all high-level deposits laid down by the sea should contain shells, and further it is not philosophical to assume that all high-level shelly drifts have been discovered. Their discovery has generally been in the nature of an accident. I fear the advocates of the Irish-Sea glacier are continually forgetting how difficult it is to prove a negative, and, unfortunately, their arguments are too frequently of the negative kind.

But, whether laid down by an Irish-Sea glacier or by the sea, the high-level drifts have been subjected to very active denudation since, as the rainfall is much greater in these elevated localities than

on the plains, and the time that has elapsed since they were deposited, in my estimation, is certainly not less than 60,000 years. I know that some good geologists have persuaded themselves that not more than 10,000 years have elapsed since the melting of the ice, but, though respecting their opinions, I cannot agree with their premises.

3.—*The shells are always broken as if a heavy body had moved over them.*

This formula is not strictly true, especially in the light of modern discoveries. There is no doubt, however, that the generally fragmentary condition of the shells has been a source of difficulty to observers in the past. The demands of the opponents of submergence are, however, rather exacting. They ask to be shown bivalve shells with both valves united. I have personally never found them in this condition, excepting in the Clyde Beds, but I am assured by Mr. S. A. Stewart, of the Belfast Museum, that he has taken them out of the glacial beds in the neighbourhood of that city with the valves together.[6] In *Nature*[7] is the *précis* "of a most careful and important investigation into the shell-bearing clays of Clava in Nairn," in which a "shelly blue clay with stones in the lower part," 16 feet in thickness, overlain by 63 feet of Boulder Clay and sands, and reposing upon 36 feet of coarse gravel and sand and brown clay and stones, is described. "The highest part of the shelly clay is 503½ feet above sea-level, and the deposit appears continuous for a distance of 190 yards." "The shells are well preserved, neither rubbed nor striated, and the deposit is a true marine silt which, if not *in situ*, must have been transported in mass." The majority of the committee of investigation "consider the evidence sufficiently strong to prove a submergence of at least 500 feet." Here, then, appears to be the very thing the land glacialists are asking the submergers to produce; but, unfortunately, their demands are again varied, as some are not satisfied, and assume that the bed has been shifted *en masse*.

Considering that the valves of bivalves are only held together by ligaments that readily decay, it is not surprising if we do not find them in apposition in deposits which show evidence of strong current action, as most of the marine glacial deposits do, especially the high-level shelly gravels; perfect single valves are common enough and delicate univalves such as *Trophon*, *Nassa*, and the fine spires of *Turritella*.

The high-level shells are beach shells, that is, they consist of shells such as are thrown up and can be found together on many beaches. We do not, of course, get the exact *facies*, but in this connection I may say that I have found on the Crosby beach, with one exception, the whole of the shells named by Forbes from Trimmer's

[6] See Mr. Stewart's "Mollusca of the Boulder Clay of the North-East of Ireland." Proc. Belfast Naturalists' Field Club, 1879-80. "*Leda pygmæa*—Nock, Woodburne, Ballyrudder." "Almost invariably found in a perfect state."

[7] Reports of British Association papers, Section C., *Nature*, Sept. 28, 1893, p. 532.

original find on Moel Tryfaen. On the same shore, near to where I live, my sons and I have found no less than 70 species, though, probably, not more than 20 live in the immediate locality. On gravelly beaches the fragmentary shells far outnumber those that are unbroken. The glacial shelly fragments are also generally much rounded and water worn. If we add to the ordinary effect of wave action that of shore-ice, which would prevent, except under very peculiar circumstances, the preservation of any deposit undisturbed, the mystery with which these glacial shell-beds has been surrounded is largely dissipated. Of course, it would, in some respects, be more satisfactory to meet with the shells as perfect as in a museum, but in that case I make no doubt that their glacial origin would not be admitted. The explanation of the fragmentary condition of the shells also explains why barnacles are not found on the stones. The mixture of northern and southern species takes place at the present day at Cape Cod.

4.—*The Rocks of the Drift are never found north of their origin.*

This is another sweeping statement which wants more proof than that vouchsafed us. Very little attention has been paid by observers to the point, though doubtless, the great majority of the stones are from positions north of where they are found. The tracking of erratics to their origin is a very difficult and laborious process in most cases, as I know from personal experience. It is only where the masses of rock yielding them are in considerable force, and their character marked like Shap and Eskdale granite, that this can be satisfactorily done. It can be done, in other cases, by a system of tracking and exclusion. To differentiate Dalbeattie from Criffel granite when found in the Drift, as has been attempted, is well-nigh impossible. Eskdale granite is to be found in the Drift between Ravenglass and St. Bees, north of its origin. The rocks yielding erratics in the greatest abundance must have been the sites of glaciers, and these were principally in the Lake District and in Wales. How far north of their origin Welsh rocks are to be found cannot be known, because the sea occupies the space to the north. Charnwood Forest rocks are found in the Boulder Clay of Nottingham, and therefore must have travelled 15 miles northward (*Q.J.G.S.*, vol. xlii., p. 480). Other examples could be quoted of varying value. A great deal of gypsum is found in the Drift by the Estuary of the Dee, and this, there is every probability, came from further south, in Cheshire. There is, however, an absence of characteristic rocks south of Lancashire and Cheshire by which to trace the flow. In the high-level shelly drift the rocks, though from all levels, mostly come from localities where they may have been derived from high levels.

In my opinion, the distribution of the erratics is quite explicable by the prevalence of north-westerly winds, and by the tides. It is not probable, with a tidal current running through the "Severn

Straits" from the bight of Liverpool Bay, that many, if any, erratics would cross it westwardly from Wales.

Eskdale and Scotch granite are found along a base line from Macclesfield to Carnarvonshire, and for the two granites to be so distributed they must have crossed each other in their courses. The much talked-of Riebeckite—which is identified with that of Ailsa Craig, Professor Sollas informs me—has been found in the Drift south of Dublin; it also occurs west of Liverpool, as well as at intermediate localities in Wales. For an Irish-Sea glacier to perform this prodigy of distribution seems incredible. A glacier distributes its load in stream-lines, and the stones from one side do not cross to the other, and *vice versâ*.

5.—*If the land has been submerged, deep-sea beds containing shells in the position in which they have lived and died should be common, whereas there are none.*

This is practically number 3 objection in another form—that is, the fragmentary condition of the shells is considered to be a proof that a glacier has passed over and crushed them. The difficulty of meeting this objection is that we do not know what deep-sea beds under glacial conditions would be like. There is a physical element that is entirely overlooked, and that is the action of the tides. I have shown[8] that the whole body of water is moved down to the greatest depths by the tides, and not merely the surface.

Most of the glacial beds that I have seen bear the marks of this disturbance, in the form of current bedding, especially the sands. The sands are intercalated in the clays, and it is difficult to separate the one from the other. The Boulder Clays, according to my interpretation, bear the marks of current action also in the smallness and rounded nature of the shell fragments, the roundness of the grains of sand and their high polish, and the manner in which sand-beds occur therein. The proportion of sand to clay is generally very considerable, the small gravel is also often highly polished and rounded. Numerous examinations of low-level Boulder Clays by washing, sifting, and separation of the grains, impresses me very strongly with the enormous wear which every constituent particle of the Boulder Clay has undergone, something totally different to what is seen in sand washed out of a living glacier which is uniformly angular. It is, therefore, quite probable that such deep-sea beds as are asked for do not occur in tidal seas, or if they do, only in exceptional positions. It is true the dredge brings up live shells from the sea-bottom now, but it is a surface gleaning, and the shell fragments are usually much more numerous than the living examples. Molluscs dying on the bottom of a tidal sea would not lie undisturbed, they would be rolled about with each tide, so that it is quite possible that undisturbed beds may not be accumulated.

[8] "Tidal action a geological cause." *Proc. L'pool. Geol. Soc.* Session 1873-4; also *Phil. Mag.*, 1888, vol. xxv., pp. 338-343.

These considerations may not be deemed conclusive, but neither is the objection. We want more knowledge to speak positively either way. Also we have not examined the *whole* of the Drift yet, so that it contains many unknown possibilities.

6.—*A Subsidence of the land to the required depth is an improbability.*

I confess the attitude of mind which this statement discloses is one I have a difficulty in comprehending. There is no known fact in physical geology on surer foundations than that the land in all known continents and islands has undergone considerable fluctuation of level. Some land glacialists admit it as regards elevation and even invoke former continental elevation as *the cause* of the Glacial Period. They are unwilling to admit subsidence, but the clearest evidence up to 5,000 feet is to be obtained in North America and to 1,000 feet in Greenland,[9] and examples could be quoted from well nigh the whole world. I am not speaking now of aught but post-Tertiary subsidence. Our own coasts in the presence of buried river channels far below low-water bear the marks of subsidence. If we are prepared to dispute their evidence it will be necessary to recommence the study of geology on a new basis.[10] There is no evidence of any kind to indicate that this sort of elevation and subsidence takes place by or is accompanied by faulting, but plenty to show that it is by the bending of the earth's crust and differential vertical movement shading off to nothing.

Conclusions.

Having now to the best of my ability fairly stated the case against submergence as an explanation of the marine glacial beds, it will be well to consider the general nature of the objections. A very little examination will show that the criticisms are largely based upon negative evidence, which may become invalid at any moment by further discovery.

The allegation of the extremely partial nature of the high-level shelly gravels is, I have shown, not borne out by facts, and every additional discovery of shelly drift weakens the argument. That no perfect shells, or shells with the two valves in apposition, were to be found in the Boulder Clay is an assertion that is already disproved, and more examples in the same direction may be forthcoming.

That the erratics, including in this term all foreign rocks whether occurring as pebbles or boulders, are not found north of their origin, is another objection which has not been proved. The absence of deep-sea beds such as the land glacialists demand, is a further statement of the same sort that it is impossible to prove. There remain, then,

[9] De Rance, *Glacialists' Magazine*, August, 1893, p. 6.

[10] Since this was written, Dr. Geo. Dawson (*Proc. Geol. Soc.*, Nov. 8) has described the finding of Mammoth remains in the Pribilov Islands and Alaska, indicating a former land-connection of the North American Continent with Asia, and therefore a subsidence either during or since the Glacial Period.

only two arguments to answer, namely, the occurrence of boulders at a higher level than the parent rocks, and the supposed improbability of a subsidence of the land to the extent required by the submergence theory. The former I have shown is explicable by ordinary sea action assisted by shore-ice, and the latter is no improbability but a difficulty born of unfamiliarity with ordinary geological phenomena.

It is now time to turn our attention to the difficulties presented by the alternative hypothesis—that of the Irish-Sea glacier—and here, indeed, we are at a loss where to begin, they are so many and various. A huge machinery of ice-sheets is called in to explain the carrying up of boulders above their origin, though no examples of the power of ice-sheets to do this are given. The crushing of the shells is referred to the same agent, though many examples are known of glaciers passing over soft beds without disturbing them. At the same time, delicate shells found on elevated lands far inland have to be accounted for, and it is suggested that they have been safely conveyed frozen in the ice-sole of the Irish-Sea glacier which carried them over hill and dale to be washed out and deposited on the final melting of the ice. The rounding of the gravel and boulders and their deposit on the hill-tops, it is said, took place in a somewhat similar manner; but, as usual, no existing examples of such action are referred to, nor is it explained why the deposits on hill-tops should be more water-worn than those on the plain. To account for the distribution of the erratics, either several glacial and interglacial periods are required, of which there is no record, or a most extraordinary set of currents of ice and undercurrents are postulated; everything, in fact, except vortex movement. Yet, with all this complicated machinery, none have been able, in theory, to satisfactorily get the *stones on one side of the Irish-Sea glacier carried to the other side*. Finally, the physics of the ice-sheet are not subjected to a quantitative test.

When we call up before our mental vision the simple and well-known facts of nature which suffice to explain the marine drifts on the theory of submergence, it seems unnecessary to resort to the ingenious and artificial system of physics elaborated to explain the phenomena by land-ice.

When we have more knowledge of the glaciers of the Arctic Regions, and facts, in place of ingenious suppositions, to base our reasoning upon, we may possibly have to revise all our glacial conceptions. In the meantime, the submergence theory of the origin of the high-level shelly gravels and sands seems to me by far the simpler of the two theories, and the most consistent with the facts and phenomena which the labours of a succession of enthusiastic geologists have made us acquainted with.

T. Mellard Reade.

II.

Some Facts of Telegony.

TELEGONY is a term recently proposed by Professor Weismann to denote the influence which a first sire is believed by many breeders to have on the offspring of the same mother by a second sire. In his second article on "The All-sufficiency of Natural Selection," which recently appeared in the *Contemporary Review* (1), and in his lately-published book "The Germ-Plasm," Professor Weismann states his belief that the evidence for this phenomenon is not sufficiently convincing to establish it as a fact. This, I venture to think, may possibly arise from an insufficient examination of the evidence. Telegony is, no doubt, as Mr. Romanes concludes, a rare occurrence; and hence it is quite possible for breeders of large experience never to have met with a case, as has happened, according to Professor Weismann, with Settegast, Nathusius, and Kühn, in Germany. Mr. Tegetmeier also, our greatest authority on poultry, recently stated in the *Field* that, with fowls, the influence of the last sire is prepotent. Nevertheless, the well-known physiological variability of animals, which is as marked as their differences of form and colour, must surely be taken into account; and I hereupon proceed to give some cases which seem difficult of explanation on any other than the Telegonic theory. And first, with regard to horses, I should not mention Lord Morton's celebrated mare, as the case has been so often quoted, were it not for the fact that Professor Weismann quotes Settegast to the effect that in the drawings by Agasse of the striped colts borne by the mare to the black Arab after the quagga-hybrid, which are preserved in the Museum of the Royal College of Surgeons, no resemblance to the quagga is perceptible beyond the stripes.

Darwin (2), who mentions that these colts had also stiff, quagga-like manes, does not, according to Professor Weismann, seem to have known of these drawings; but Dr. Alexander Harvey (3), whose very interesting pamphlet "On a Curious Effect of Cross Breeding" is referred to by Darwin, quotes therein McGillivray (*Aberdeen Journal*, March 28, 1869), who mentions the case of a mare belonging to Sir Gore Ouseley, which, after bearing a hybrid to a *Zebra*, produced to horses two striped foals; whose portraits and skins are said to be in the Museum above mentioned. Since Dr. Harvey also

mentions Lord Morton's mare, it would seem that Settegast, and not he, has confused two distinct cases here. At any rate, it would be well that these pictures should be re-examined, and their history clearly stated.

In this same pamphlet McGillivray is the authority for two other curious cases, both occurring in pure horses. In several foals, in the Royal stud at Hampton Court, the progeny of the horse Actæon, there were unquestionable marks of the horse Colonel, to whom the dams of these foals had been put the previous year. Also a colt, the property of the Earl of Suffield, the son of the horse Laurel, so resembled another horse, Camel, "that it was whispered, nay, even asserted, at Newmarket, that he must have been got by Camel." It was found, however, that all that connected them was that the colt's dam had been put to Camel the previous year.

I do not think that the case of the onager's foal at the Zoological Gardens, recorded by Mr. Tegetmeier in the *Field*, in December last, is so unmistakable an evidence of Telegony as Dr. Romanes (4) appears to think. The onager was put to an Abyssinian wild ass, produced a hybrid, and then bore to a male onager a chestnut foal with a white blaze on the forehead; but as this foal thus resembled neither parent, and in fact exhibited a horse's rather than an ass's marking, the case is surely one of Analogous Variation.

With respect to the case of Carneri's cattle, alluded to on page 385 of "The Germ-Plasm," it is noticeable that Carneri, who, since he kept a herd, had presumably some experience of cattle-breeding, evidently considered the germ-infection theory more reasonable than that of reversion to a previous cross. Professor Wallace (5), in his book on the farm live-stock of Great Britain, cautions breeders against putting a mongrel bull to good cows, as well on this account as for general reasons against keeping bad stock; and Mr. Bourne assures me that this is a matter of practical consideration with cattle breeders.

In sheep, besides the interesting instance given by Darwin of some Merino ewes, which after being put to a Merino ram with neck-lappets, bore lambs with this abnormal character to other sires, there is a very remarkable case given by Dr. Harvey, on the authority of Mr. W. M'Combie, of Tilliefour, Aberdeenshire:—

Six very superior pure-bred black-faced horned ewes, the property of Mr. Harry Shaw, in the parish of Lochiel-Cushnie, in Aberdeenshire, were put, in the autumn of 1844, some to a Leicester (white-faced and polled), and others to a Southdown (dun-faced and polled) ram, and produced cross-bred lambs. In the autumn of 1845 the same ewes were put to a very fine pure black-faced horned ram (*i.e.*, of their own breed). The lambs were *all* polled and brownish in the face, much to Mr. Shaw's astonishment. In autumn, 1846 the ewes were again put to another very superior ram of their own breed. Again the lambs were mongrels, not showing so much of the alien breeds as

those of the year before; but two were polled and one dun-faced, with very small horns, while the other three were white-faced, with small round horns only. Mr. Shaw at length parted from those fine ewes, without obtaining from them one pure-bred lamb.

In the case quoted by Darwin of a sow of Western's black-and-white breed, which, after bearing to a chestnut wild boar a litter of cross-bred pigs, produced long after his death, to a boar of her own breed, pigs showing chestnut markings—though this might be put down to reversion, it is noteworthy that Harvey, who mentions this case, adds that on a *subsequent* impregnation, still by a boar of her own breed, she yet produced pigs, some of which were *slightly* marked with chestnut.

Youatt also says (6): "The boar to whom the sow has her first litter of pigs has a considerable influence on future litters, especially if of a very pure breed. In one instance a black sow was put to a white boar, and afterwards continuously to a black boar for three litters, yet in all these three litters there were white or black-and-white pigs."

In the "Book of the Rabbit" (7), Mr. Edward McKay, I believe a well-known breeder, is quoted as saying, "The influence of a first sire sometimes extends for generations, *i.e.*, influence of previous sires over offspring by other sires out of the same dam. I have known curious cases, which I cannot help thinking are the result of the above; for instance, a silver-grey doe was put to a Himalayan buck, afterwards to a Dutch sire, and afterwards to a buck of her own class, and in every succeeding litter, for several generations, were youngsters of all the above kinds. This may seem improbable to some, but, in my mind, no doubt exists."

In the case of dogs, Herr Lang, of Stuttgart (quoted by Professor Weismann), and Dr. Romanes have only got negative results, and in response to an appeal by Mr. Tegetmeier for facts on this subject, Mr. W. Godwin (8), of Market Drayton, in the *Field*, of Oct. 14, 1893, remarks that in numberless cases he has failed to notice any influence of one sire on succeeding offspring by another. However, he gives some instances of this, for which he can vouch, both with dogs and fowls. Those relating to dogs are as follows:—

A spaniel bitch littered to a terrier; all were destroyed, and in the next litter, to a spaniel, one was more like a terrier than a spaniel, the rest being true spaniels. An Irish terrier bitch, mated with a fox terrier, and bore her next litter to a well-bred Irish terrier; most of the last were like, apparently, pure Irish, two or three were red and white, one was all white, except a brindle red mark on the head and another on the stern, and strongly like a fox-terrier, except in size. A cocker-spaniel bitch, when being taken to a curly black spaniel, escaped and paired with a pointer, but was re-captured and paired to the spaniel in a very short time. Yet the resulting litter were half-bred pointers, save one, which was undoubtedly got

by the spaniel; but this in form and habits showed traces of pointer blood.

With birds, the conditions differ widely from those present in the case of mammals, since in the former class there is no possibility of the infection of the maternal blood by a cross-bred fœtus; yet there is evidence of the telegonic phenomenon in them also. Mr. Lewis Wright (9) is so convinced of its occurrence that he says, "We would never on any account allow a male bird of any strange breed to enter, even for a day in winter, a yard of hens which we greatly valued." He gives some instances, from which I select the following:—

A Mr. Payne, in England, had two Spanish pullets running with both a Spanish and Cochin cock. After they began to lay the Cochin was removed, and *six weeks* after the eggs were saved and set, but the chickens were feather-legged, in all other points resembling the Spanish.

In America, a breeder of game finding a neighbour's feather-legged bantam cock came over his fence, penned in his fowls securely, and saved no eggs for a month after, but several chicks still had feathered legs, though with no other sign of the cross. Dr. Harvey, who favoured the theory of maternal infection by the fœtus, states, as proof that telegony, as would be expected on this view, does not occur in birds, that he has been assured that crossing with a bantam during one season does not affect the progeny of a common hen during the next.

But Mr. Godwin, quoted above, also gives some instances of the working of this principle with fowls. A Dorking hen (5-toed, with bare white legs), after running with a Dorking cock, was put to a dark Brahma (4-toed, with feathered yellow legs.) The eggs on the two following days produced pure Dorkings; the egg on the third or fourth day a Dorking with three or four feathers on one leg; the next egg a feather-legged Dorking with four toes only on one foot, both legs being white; the next egg, and all after, yellow-legged birds, with a lot of feathers, as if the influence of the sire were progressive. This case came to his knowledge some thirty years back.

In Mr. S. Fielding's yard, at Trentham, Mr. Godwin saw some young fowls of game character, yet with a decidedly mongrel look. They were the offspring of a Silver Hamburg pullet, which had been running with a game cock, and three weeks had been allowed to elapse after her removal from him before any eggs were set; yet this was the result.

It is obvious that some of the cases above given can hardly be set down to reversion, feather-legged fowls and polled sheep not being ancestral types; nor is previous crossing a likely explanation. Before concluding, I may mention a remarkable fact stated in Professor Newton's new "Dictionary of Birds" (10); according to this, Nathusius came to the conclusion that by microscopical examination, which

reveals differences between eggs of birds of various species, the egg of a bird paired with an alien male may be distinguished from one laid by the same bird with a mate of her own species. And Mr. A. G. Butler (11) affirms that a canary in his possession laid eggs coloured and marked like those of a chaffinch, when paired with a male of that species. Experiments are much needed on these points; and since we fortunately have in domestication birds nearly allied, yet perfectly distinct in species, such as the Muscovy and common ducks, Chinese and common geese, Guinea fowl and common fowl, and collared turtle-dove and pigeon, it is to be hoped that someone will be sufficiently energetic to experiment with some or all of these; hence, he may thus help on the solution of more than one debated question interesting to scientific men in general, for the same series of experiments might be made to throw light on Hybridism as well as on the subject of this paper.

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FRANK FINN.

III.

Further Notes upon Arachnid and Insect Development.

IN an article in NATURAL SCIENCE (June, 1893) I gave a summary of some recent memoirs upon the classification of arachnids Mr. Goodrich has since (NATURAL SCIENCE, September, 1893) kindly pointed out that the views of Mr. Pocock on the relationships of the arachnid orders, therein set forth, agree in the main with those put forward twelve years ago by Professor Ray Lankester (1), in his memoir advocating the arachnid affinities of the King-Crab.

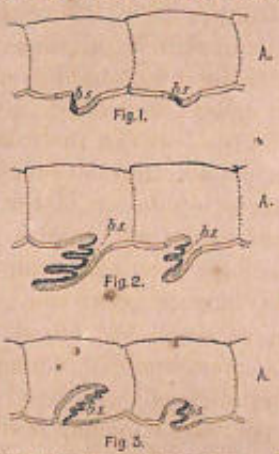
Ever since the appearance of that well-known paper by Professor Lankester, *Limulus* has been an animal of exceptional interest to zoologists. The recent publication by Mr. Kingsley (2) of a detailed description of the embryology of the King-Crab is therefore worthy of special notice. His results, on the whole, strongly support the arachnid affinities of *Limulus*; its development is shown to have but little correspondence with that of Crustaceans.

The structure of the ovary resembles that of scorpions, spiders, and mites. The ova are developed from the columnar epithelium of the ovarian tubes, and pass into a space between the epithelium and the tunica, which, at first, are in contact with each other. The segmentation of the egg proceeds by delamination, similar to that observed in various spiders, harvestmen, and mites. The mesoderm arises by proliferation from the thickened region of the blastoderm, and cœlomic cavities are formed in the somites by the splitting of the mesoderm into two sheets. The body-cavity of the adult is, however, due to secondary spaces in the mesodermal tissues. Each cœlomic cavity becomes divided into a dorsal and a ventral part; the former is believed to give rise to the generative glands; the latter is lost in most of the somites, but in the fifth it develops into the coxal gland (nephridium). This corresponds with the fate of the cœlome and the origin of nephridia in *Peripatus* and in arthropods generally; it specially recalls the origin of the coxal glands in arachnids.[2] The formation of the alimentary tract agrees with what occurs in arachnids, and contrasts with the corresponding process in crustaceans, for, while the stomodæum is long, the proctodæum is short, and late in its appearance. Mr. Kingsley states that his results are, in the main, confirmed by the researches of Mr. Kishinouye (3) upon the development of *Limulus longispina*.

[1] SCIENCE, vol. i., p. 282. [2] *Ibid.*, pp. 524, 525.

In giving an account of the development of the gill-books of *Limulus*, Mr. Kingsley refers to Mr. Laurie's views on the relationship of these organs to the lung-book of arachnids,[3] and ingeniously shows that the reversal of the faces of the appendage suggested by Mr. Laurie is not necessary. From the embryonic appendage with invagination behind it (Fig. 1), can be derived the gills of *Limulus* (Fig. 2) or the lung of an arachnid (Fig. 3), the folds in the latter case, though on the forward side of the invagination, really corresponding with the hinder surface of the primitive appendage.

No details of the nervous system are given by Mr. Kingsley, but its development and adult structure have been lately described



FIGS. 1–3.—Diagrams of (1) Primitive Appendages, which develop into (2) Gills of *Limulus*, or (3) Lungs of Arachnid. A—Anterior aspect of animal; *b.s.*—Blood-spaces, which are seen to correspond in all three figures. [After Kingsley.]

by Dr. Patten (4), who shows the correspondence of the embryonic brain of *Limulus* with that of scorpions, spiders, and insects. He describes a pair of cephalic lobes with three divisions, from which are developed the large "cerebral hemispheres" of the adult. To the ganglia which innervate the cheliceræ, the term "mid-brain" is applied; the five next pairs are called the "hind-brain," and the next pair the "accessory brain." These terms indicate the homologies which Dr. Patten sees between the brain of *Limulus* and that of the Vertebrata, claiming that in the nervous system of the King-Crab fresh support is to be found for his startling theory of the arachnid ancestry of the back-boned animals. The front wall of the cephalic lobes passes downwards into the cavities of a pair of semicircular lobes, identified by Dr. Patten with the infundibulum of the vertebrate brain. From these arise the roots of the nerve of the median eyes, which are compared to the pineal eye of vertebrates. Correspondences are also found between the olfactory organs of *Limulus* and of vertebrates, specially lampreys. Dr. Patten, indeed, states that the nervous

[3] NAT. SCI., vol. i., p. 524.

system of the adult King-Crab is remarkably distinct in appearance from that of any other arthropod, and "shows a profound structural similarity to that of the vertebrates." In order to see this, one must "tear off the deceptive arthropod mask that disguises *Limulus*." The true value of Dr. Patten's homologies can only be estimated by students of the vertebrate brain. But more evidence will be required to convince most naturalists that the many profound structural distinctions between arthropods and vertebrates do not point to a wide divergence of origin. In all arthropods the œsophagus pierces the nervous system. Dr. Patten suggests that in the primitive arthropod ancestor of vertebrates the œsophagus "broke through the narrow band of nerve-tissue in front of it." But, unfortunately, in *Limulus*, which he regards as the nearest living representative of this ancestor, the nerve-tissue in front of the œsophagus is not a narrow band, but forms the great cephalic lobes; and so, if this explanation is to be accepted, the correspondences between these lobes in *Limulus* and the vertebrates must be homoplastic, and not homologous.

Dr. Patten gives interesting details of the structure of the sense-organs of *Limulus*. Sensory spines on the joints of the legs are covered with tubular openings, each containing a chitinous tubule in connection with a single elongate nerve-cell. These have a tasting function. In the organ of smell are found clusters of large cells surrounding a multipolar ganglion-cell, connected with a chitinous tube ending near the outer surface.

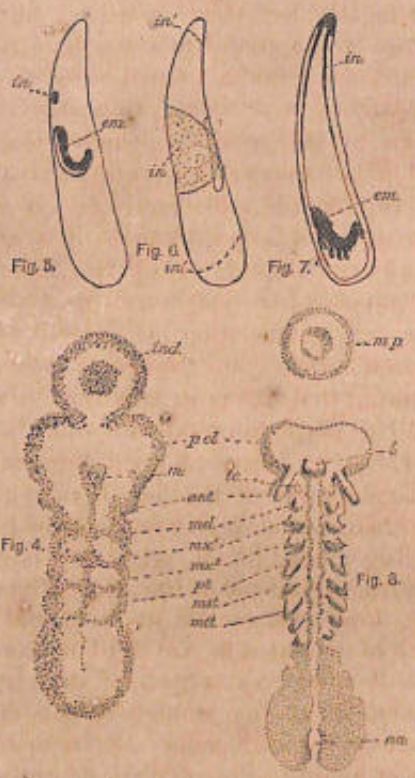
At the end of his paper, Mr. Kingsley reviews the question of the systematic position of *Limulus*. In a tabulated statement, he instances but six points in which its development and adult structure agree with those of the crustaceans, while the points of agreement with the arachnids number twenty-eight. He joins *Limulus* with the Eurypterids as a sub-class Gigantostraca forming, with the sub-class Arachnida, the class Acerata. With such strong confirmation of its arachnid affinities, most naturalists will prefer to call *Limulus* simply an arachnid. The association of the Eurypterids with the same class is justified by the researches recently summarised for us by Mr. Laurie.[4] But the Trilobites, for some time past considered as related to these forms, must probably be restored to the Crustacea since the discovery of their antennæ.[5]

Mr. Kingsley proceeds to the discussion of the classification of the Arthropoda generally. The Acerata and Crustacea form a sub-phylum Branchiata. The sub-phylum Insecta includes classes Hexapoda and Chilopoda while the Diplopoda form a separate sub-phylum. This view, that the centipedes are more nearly related to the true insects than to the millipedes, and that the Myriapoda must therefore be considered an unnatural group, has been put forward before, but has lately been elaborated also by Mr. Pocock (5) who

[4] NAT. SCI., vol. iii., p. 124. [5] NAT. SCI., vol. iii., p. 162.

lays special stress upon the position of the genital opening. The Diplopoda and Pauropoda (in which this opening occurs in the anterior region of the body) are classed by him as Progoneata, in opposition to the Chilopoda, Symphyla,[6] and Hexapoda, in which the generative duct opens at the hinder end of the animal, and which are accordingly called Opisthogoneata.

Mr. Kingsley reviews the main points of structure of the Diplopods,



FIGS. 5–7.—Embryos of *Xiphidium*: *em.* embryo. *in.* indusium. *in'.* furthest extension of indusium. FIG. 4.—Young embryo of *Xiphidium*. FIG. 8.—Embryo of *Anurida*. *ind.* indusium. *mp.* micropyle. *p.cl.* procephalic lobes. *m.* mouth. *l.* labrum. *ant.* antennæ. *te.* tritocerebral appendage. *md.* mandibles. *mx*¹, *mx*². maxillæ 1 and 2. *pt.*, *mst.*, *mtt.* thoracic segments. *an.* anus. [After Wheeler.]

Chilopods, and true Insects, and finds further support for the association of the two latter groups in opposition to the former, in the correspondence of the pairs of jaws in centipedes and insects, and the coalescence of the somites in pairs in the millipedes. The systematic position of the Pauropoda, Pycnogonida, Trilobites, Tardigrada, and Malacopoda (*Peripatus*) is considered uncertain by

[6] According to some observers, however, the genital opening in the Symphyla (Scolopendrella) is situated in the fourth body-segment. Nevertheless, these animals must be nearly related both to the Chilopoda and the Thysanura.

Mr. Kingsley. Indeed, he considers it doubtful if the latter animals should be regarded as arthropods, pointing out that their serial nephridia, muscular pharynx, generally unstriated muscle-fibre, eyes, and pre-oral antennæ point to their relationship with the Chætopod worms. However, their reduced cœlome, heart and pericardium, tracheæ, and jaws will probably induce most naturalists to retain them as primitive arthropods.[6]

While Mr. Kingsley has thus increased our knowledge of the development of *Limulus*, another American investigator, Dr. W. M. Wheeler, has (6) made a valuable contribution to the embryology of insects. His researches have been principally carried out upon a species of *Xiphidium*, an orthopterous genus belonging to the family of the long-horned grasshoppers (Phasgonuridæ).[7] The females of these insects lay their eggs in the galls produced on willows by midges (*Cecidomyia*). The eggs of a grasshopper of a nearly related genus (*Orchelimum*), a cricket (*Gryllus*), a "praying-insect" (*Stagmomantis*), and a marine collembolan (*Anurida*) were also studied.

The process of gastrulation is described, and confirms Cholodkowsky's researches on the cockroach and Graber's on *Stenobothrus* in showing that this process in the Orthoptera corresponds more closely than had been supposed with what occurs in the higher groups of insects. A structure of great interest observed by Dr. Wheeler in the development of the long-horned grasshoppers, is the indusium. This organ, which seems not to have been before noticed in the embryo of any winged insect, arises as a circular thickening of the blastoderm in front of the head (Fig. 4). While the embryo moves through the yolk, from the ventral to the dorsal aspect of the egg (Fig. 5) the indusium increases in size until it has assumed a saddle-shaped form, covering the greater part of the egg's surface (Fig. 6). A layer, corresponding to the amnion of the embryo, is formed within the serosa, over the indusium. Before hatching, the embryo in its growth comes again to the original ventral aspect of the egg (Fig. 7) absorbing the yolk, while the indusium becomes reduced to a cap of cells at the apex of the egg. Dr. Wheeler considers the indusium in these grasshoppers to represent the micropyle observed in embryos of the Poduridæ, and he gives a figure of the embryo of *Anurida* (Fig. 8) to confirm this view. He would also compare the structure with the dorsal organ of Crustacea, the primitive cumulus of spider-embryos, and the embryonic sucking-disk of certain leeches.

The antennæ of the insect embryos examined are stated by Dr. Wheeler to arise behind the mouth, and are so figured (Figs. 4, 8) thus confirming Cholodkowsky's observations upon the cockroach.[8] They are innervated from the deuto-cerebrum or first post-oral nerve-mass. The segment of the trito-cerebrum (the ganglion next behind) bears a pair of evanescent appendages in the embryo of *Anurida* (Figs. 8, *tc.*). With regard to the hindermost appendages of the

[7] Locustidæ, auct. [8] Nat. Sci., vol. i., p. 281.

body, Dr. Wheeler brings forward evidence to support the accepted view that they are utilised in connection with the reproductive opening.

Dr. Wheeler's observations upon the origin of the germ-cells confirm, on the whole, the results of Dr. Heymons with *Phyllodromia*,[9] except that doubt is thrown upon their very early differentiation as described by the latter observer. They have now been noticed in all but two of the abdominal segments of embryo insects, recalling very strongly the condition in worms. Dr. Wheeler's researches on the origin of the genital ducts confirm the view that they are modified nephridia. A peculiar mass of cells beneath the œsophagus, developed from the mesoderm of the trito-cerebral somite, is thought by Dr. Wheeler to be a vestigial nephridium, and, accepting the homology of this segment with the antennal segment of the Decapod Crustacea, to represent the green gland of a crayfish. This homology of the segments will bring the antennæ of insects into line with the crustacean antennule, and make the mandibles in Crustacea, Arachnida, and Insecta correspond. Dr. Patten, however, insists (4) that no segments have been suppressed in either insects or arachnids, and that the antennæ of the former correspond with the chelicerae of the latter; but it seems impossible that all the recent describers of vestigial appendages can be mistaken.

With these contributions to the embryology of insects, it is well to record some recent memoirs upon their development after hatching. Professor Miall (7) has discovered the larva and pupa of a genus of Crane-fly (*Dicranota*) whose metamorphosis had been previously unknown. The grubs of the "Daddy-long-legs" (*Tipula*), belonging to the same family, are only too well-known as the "leather-jackets," which often devastate the roots of corn and grass crops. The larva of *Dicranota*, however, is a flesh-feeder; it lives in gravel and mud at the bottom of streams, and preys upon the well-known red worm, *Tubifex*. The head of the larva is small, and can be retracted within the thorax. The third to seventh abdominal segments bear pairs of fleshy legs with circles of hooks at the end, recalling the claspers of moth-caterpillars; the ninth (hindmost) segment carries three pairs of appendages, of which the posterior are long, and contain tracheæ. When the head is retracted, the brain, with the subœsophageal and prothoracic ganglia, is found in the mesothoracic segment. The alimentary canal shows adaptation to the carnivorous habit, being straight, and wanting the diverticula found in the grubs of *Tipula*. As in tipulid larvæ generally, there is but one pair of spiracles, situated on the back of the hindmost segment, which is also provided with tracheal gills, so that the grub can breathe by thrusting its tail either into air or water. A well-developed tracheal system provides for the storage of oxygen. The male generative organs develop very early, and ripe spermatozoa were observed in a larva not full-grown.

The pupa lives in damp earth, and breathes through a pair of

[9] NAT. SCI., vol. i., pp. 54, 55.

"respiratory trumpets" on the prothorax. The air is admitted to these through a series of small oval apertures, fine enough to keep out water or dirt, if they are not closed (as may, perhaps, be the case) by a transparent membrane. The abdominal segments bear dorsal plates with spines, by means of which the pupa makes its way out of the ground, before the fly emerges; ventral pro-legs, corresponding to those found in the larva, assist in this function.

The structure of the stigmata of the grub of the Common Cockchafer, as described by Herr Boas (8), recall the arrangement of the respiratory trumpet of the *Dicranota* pupa. The ordinary opening of an insect stigma is found to be closed in this larva, and the insect takes in air through minute pores in the "sieve-plate" surrounding the normal aperture. In both cases we see the adaptation to an underground life.

The highly interesting pupa of a minute moth (*Micropteryx*) has been lately described by Dr. Chapman (9). This pupa lives in a very strong cocoon, several inches underground. It is remarkable among lepidopterous pupæ in possessing a well-developed labrum, and mandibles which Dr. Chapman (allowing for the relative sizes of the insects) compares to those of a stag-beetle! As is well-known, these organs are usually reduced to the merest vestiges in moths and their pupæ. In the present case, the obvious use of these powerful jaws is to effect the escape of the pupa from its buried cocoon, before the emergence of the moth. The arrangement recalls what occurs in the Caddis-flies, and is another strong argument in favour of the primitive position of the Micropterygidæ among Lepidoptera, the wing-neuration having already led to the acknowledgment of their affinities with the Trichoptera. Yet another fact which tells the same tale is that the pupa seems entirely incomplete (the limbs are not soldered to the body) and the moth escapes through a simple dorsal slit—an arrangement hitherto unknown among moths according to Dr. Chapman.

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GEO. H. CARPENTER.

IV.

Note on the Evolution of the Scales of Fishes.

MUCH has been written by those who have studied the minute structure of fish-scales, concerning their primitive nature and mode of origin. It is generally admitted that the earliest hard parts appearing in the skin of fishes, were merely isolated calcified points or papillæ, such as those in which the tubercles of sharks and skates arise; and that all the later types of scales and plates are only the result of the fusion and elaboration of these fundamental structures. Palæontology, too, specially favours this idea; and an elaborate memoir just published by Dr. Rohon (3) shows more clearly than ever how large a majority of the fragments of dermal armour of chordate animals in the Upper Silurian rocks are almost identical in structure with the comparatively simple skin tubercles of the sharks and skates.

There is, however, another aspect of the subject which seems to have been less studied, and to which it is now possible to make one more small contribution. We refer to the question of the original character and subsequent phases of evolution of the typical overlapping scales of the trunk of ordinary fishes. Professor Ryder has inferred (4), from theoretical considerations, that these scales must have been originally disposed round the body in a series of rings corresponding to the successive plates of muscle; and he attempts to show, by an argument from mechanics, that they must have been at first rhombic in shape, in consequence of the direction of the strains arising from the working of the muscles during motion. It is obvious that the only proof of the theory can be obtained from Palæontology; and we propose in this brief note to summarise a few new or little-known facts bearing upon the subject.

Perhaps the simplest form of squamation met with in any fish-like organism is that of the Lower Palæozoic *Cephalaspis* and its allies, in which (as Professor Lankester first remarked) each segment of muscle seems to be surrounded by a distinct ring of six or seven scales; this ring being overlapped by the one immediately in front and overlapping that next behind (2, 5). Towards the extremity of the tail in *Cephalaspis*, however, where the motion is much greater than in the rest of the trunk, each of these rings is subdivided into a set of comparatively small rhomboidal scales—an arrangement

decidedly suggestive of the truth of Professor Ryder's theory of the effect of mechanical strains.

Among undoubted fishes it is not easy to discover any facts particularly apposite to the question under consideration in the groups of Dipnoi and Fringe-finned "Ganoids"; but it seems possible to turn with more satisfaction to the higher "Ganoids" of the sub-class Teleostomi. Here it is quite evident, in a general way, that as the fishes become higher in organisation—become specialised—the scales are more deeply overlapping, thinner, and almost or regularly cycloidal. We can now even quote instances in which the anterior part of the trunk is covered by typical quadrangular ganoid scales, united by a peg-and-socket articulation; while the base of the tail of the same fish is enveloped by deeply-overlapping thin rounded scales, which would be typically "cycloid" were it not for the presence of a few remnants of superficial enamel.

Such an instance is a deep-bodied fish related to the European Liassic genus *Dapedius*, obtained from the Hawkesbury Formation of New South Wales and shortly to be described under the name of *Aetheolepis*. Typical examples of its scales are shown in the accompanying figures. Those of the anterior series, as shown by the

Scales of *Aetheolepis* from Hawkesbury Formation, New South Wales.

drawing to the left, are thick and rhombic with well-developed peg-and-socket articulation; while on the caudal region all the scales are very thin and much overlapping, and they gradually degenerate backwards in the manner here indicated. The specimens are not sufficiently well-preserved to admit of the making of microscopic sections; but it is probable that the hindermost scales are destitute of bony tissue, and the traces of enamel are merely a few isolated tubercles. *Aetheolepis*, in fact, so far as its scales are concerned, belongs in its anterior half to the "order Ganoidei" of Agassiz, while posteriorly it almost exactly enters the "order Cycloidei" of the same author.

There is a tendency to the same kind of scale-arrangement also in a fish from the English Lias, apparently of the genus *Endactis*; and from both instances it may probably be inferred that the cycloidal scales have resulted from the modification of the thick quadrangular

scales by the mechanical conditions in the comparatively mobile caudal region where they are found.

It is well known that even in ordinary ganoid fishes with rhombic scales, their peg-and-socket union is almost always wanting on the tail, as if to ensure greater freedom of motion. So far as the present writer is aware, however, no case has hitherto been noticed in which firmly-united quadrangular scales are fixed at their upper and lower borders by more than the peg and socket. It is, therefore, of much interest to record that the scales of the Pycnodont genus *Mesturus* (from the Bavarian Lithographic Stone and the English Oxford Clay) are joined together above and below by a deeply dentated, interlocking suture, exactly as observed in certain scales of crocodiles. *Mesturus*, indeed, must have had an almost inflexible scale-armour.

If further proof were required that rhombic scales in the Palæozoic fishes are more primitive than deeply overlapping cycloidal scales, we need only refer to the case of the Palæoniscidæ. In this group the fishes with cycloidal scales always retain the thick rhombic ones on the upper lobe of the tail, however much the aspect of the trunk-squamation itself may have changed (1). It is therefore curious to find that among the Devonian fringe-finned ganoids, those with the most primitive fins (Holoptychiidæ) have the scales round and deeply overlapping, while many of those with more specialised fins (Osteolepidæ) have the squamation rhombic. At the same time, it must be remembered that the rhombic-scaled *Megalichthys* of the Coal-measures gradually passes into the round-scaled *Rhizodopsis* of the same age, the inner rib of each scale then degenerating into a small tubercle; and some of the round scales of the Devonian fishes show this inner tubercle, which is likely to have arisen in the same manner.

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A. SMITH WOODWARD.

V.

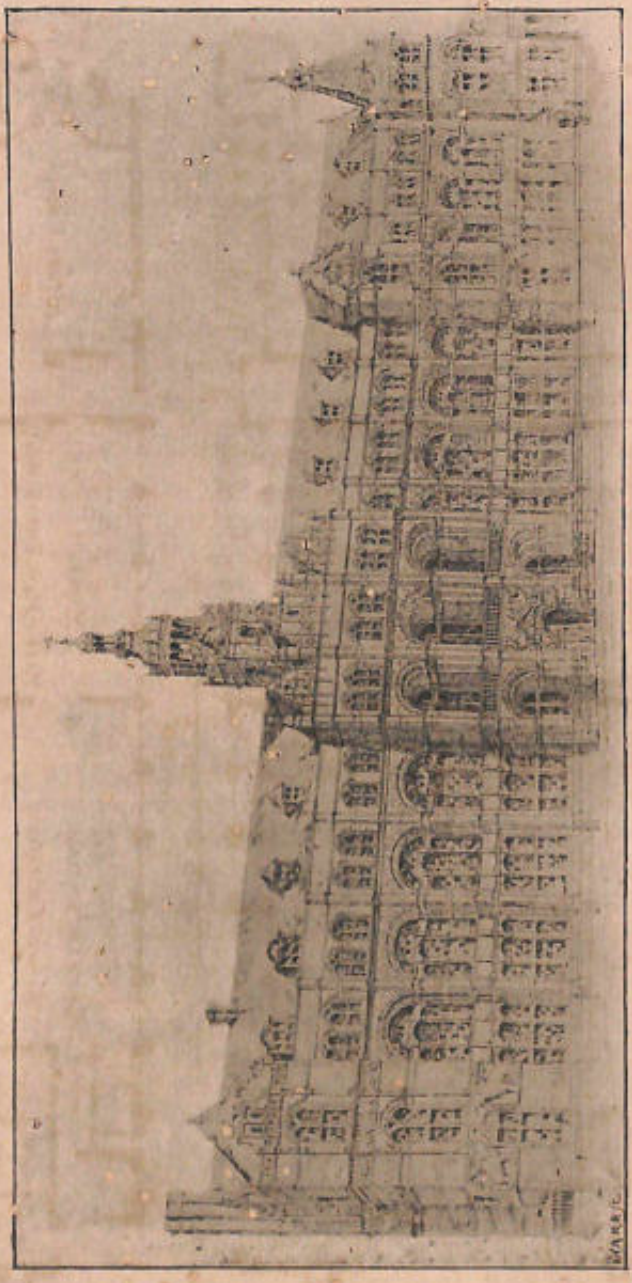
The Sedgwick Museum, Cambridge.

WHEN the question of a memorial to Professor Sedgwick came to be considered, it was generally agreed that nothing could be more appropriate than a Museum to contain the collections which had been so largely made during the fifty-five years of his Professorship, and which even then (1873) were too extensive for the building which they still occupy. For this purpose a sum of money, which now amounts to £23,000, was quickly subscribed.

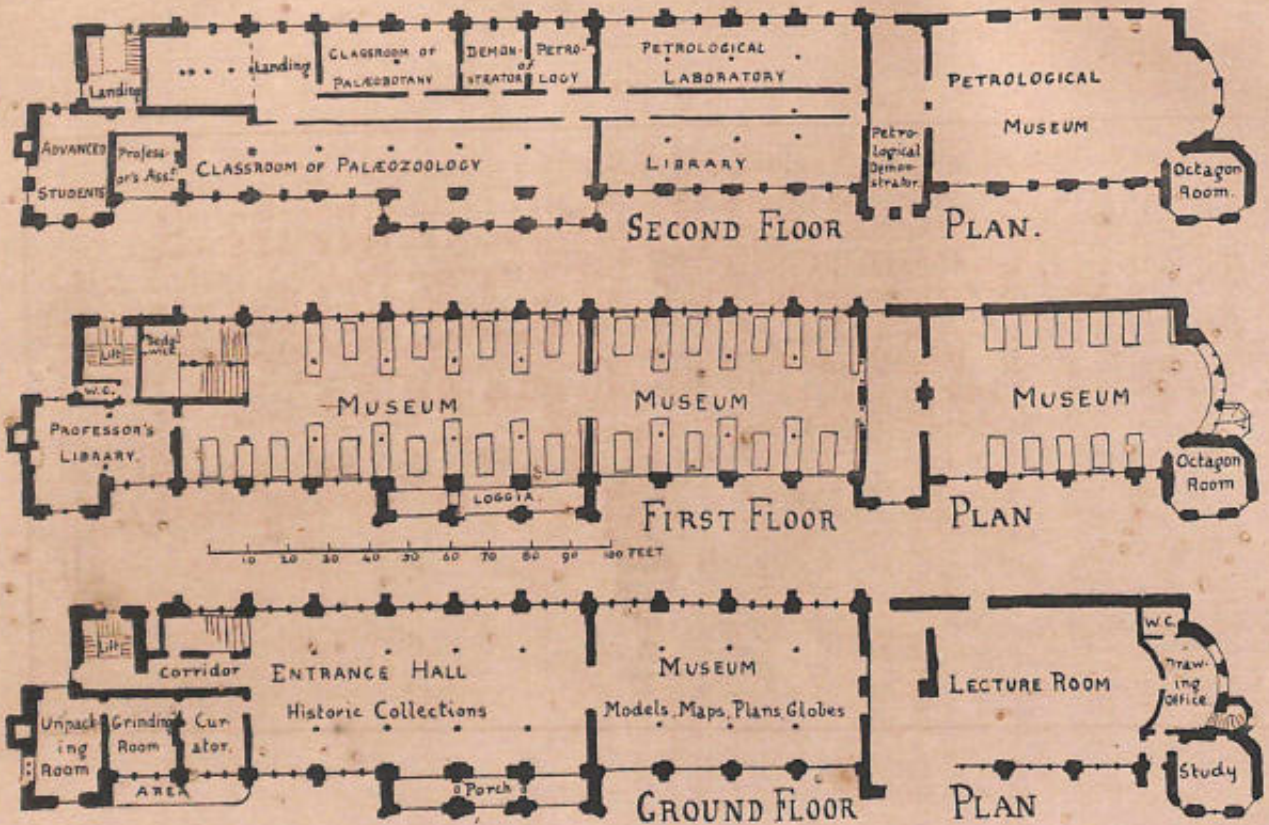
Several syndicates have been appointed by the University from time to time to carry out this object. But no real advance was made until 1891, when the site was fixed upon as that part of the Old Botanic Gardens which faces Downing and Pembroke Streets, and extends from Corn Exchange Street to near the Chemical Laboratory. This position will be most convenient both for teachers and students, since all the other departments of Science have their museums and laboratories close at hand. Another Syndicate was appointed on February 4, 1892, to prepare plans and obtain estimates for the building. The services of the eminent architect, Mr. T. G. Jackson, A.R.A., were obtained; and his plans, together with the report of the Syndicate, were presented to the University on November 22, 1892, and after some slight modifications were accepted by the Senate on May 4, 1893.

The perspective elevation and floor-plans are shown in the accompanying illustrations. The materials to be used in the construction are red brick and Clipsham stone (Lincolnshire Oolite). The length of the building will be 304 feet, and the breadth 44 feet (outside measurement); the main wall will be set back 10 feet from the street, and the projecting portions 4 feet. There will be three floors in addition to cellars and attics.

On the ground floor will be the entrance-hall, containing the original Woodwardian and other historic collections; to the right will be a room for models, maps, sections, etc., and beyond this a lecture-room; to the left will be the Curator's room and other offices. At the north-west corner will be the principal staircase. The first floor will be devoted entirely to the Museum of Stratigraphical Palæontology, with the exception of a room at the west end for the Professor's library. This museum will be divided by upright cases,



The Proposed Sedgwick Museum, Cambridge.



Plans of Proposed Sedgwick Museum, Cambridge.

placed between the windows, into compartments, in each of which a table case will be placed. The third floor will be occupied by laboratories for Palæontology and Petrology, by private rooms for lecturers and demonstrators, and by the Museum of Petrology.

The two lower floors will be lighted by long windows reaching up to the second floor-level; the top floor will be lighted by smaller and more numerous windows.

The laboratories are placed on the second floor in order to secure the best light. This has made it necessary to place two rows of pillars in the lower floors for support; in the main museum they will pass through the middle of the upright cases, and will consequently be quite inconspicuous. At the head of the first flight of stairs there will be a statue of Professor Sedgwick. The eastern part of the site assigned to the Museum is at present occupied by the medical school, and as this cannot yet be accommodated elsewhere, it is proposed to build first a part of the Museum on the ground which is free.

HENRY WOODS.

VI.

Recent Explorations of the Maltese and Sicilian Caverns.

SOME interesting progress has recently been made in investigating the bone-caves of Malta and Sicily. It has long been a matter of common belief that these islands are the remnants of one of the old land barriers connecting Europe with Northern Africa during at least part of the Pliocene and Pleistocene periods; and a detailed study of the animal remains met with in the fissures and caverns is thus one of the foremost importance. Not only is it possible to recognise the mingling of northern and southern animals, and the apparent effect of isolation upon them before their complete extinction as the feeding-area became more and more reduced by subsidence; but it also seems likely that some idea of the nature of recent physical changes in the region in question can be obtained from a comparison of the sequence of deposits in the various localities examined. The work in Malta has been carried on by Mr. John H. Cooke, with the aid of a grant from the Royal Society of London (7); the new researches in Sicily are those of Dr. Hans Pohlig, based upon a large collection of bones in the Palermo Museum from the Cavern of Pontale, at Carini (10).

The bone-caves of Malta were discovered so long ago as the middle of the seventeenth century (1), and they have long been well-known through the explorations and researches of the late Rear-Admiral Spratt (11), Professor Leith Adams (2–5), Dr. Hugh Falconer (8), and Dr. George Busk (6). They have yielded some dwarf elephants, described under the names of *Elephas melitensis*, *E. falconeri*, and *E. mnaidriensis*, and a dwarf hippopotamus (*H. pentlandi*). They have also furnished evidence of a so-called gigantic dormouse (*Myoxus melitensis*), some large land-tortoises (3, 4), and various birds (9). Mr. Cooke's researches, therefore, are for the most part only an independent verification of results already obtained; but, at the same time, he has succeeded in making one or two striking additions to our knowledge of the extinct fauna in question. The principal specimens obtained have been placed in the British Museum, and a detailed report on these fossils by Mr. A. S. Woodward is appended to Mr. Cooke's account of his results.

Attention has been chiefly confined to one cavern in the Har

Dalam Gorge, near Marsa Scirocco Bay, where Spratt and Adams accomplished so much. It is situated 500 yards from the shore, and its mouth is now 40 ft. above the bed of the small stream which, in rainy weather, flows through the gorge. There is evidence everywhere of former torrential rains, where all is now comparatively parched; and Mr. Cooke considers that no other phenomenon could have filled the caves and fissures as he finds them. In most parts the Har Dalam Cave was filled to a height of within 2 ft. of the roof, and one of the principal sections of the contents showed the following succession of layers:—(i.) Unstratified surface *débris*, 6 in.; (ii.) Red clayey loam, 3 ft., with *Hippopotamus*, *Cervus*, and pottery; (iii.) Unfossiliferous black earth, 4 in.; (iv.) Dark red plastic clay, 1 ft. 6 in., with *Hippopotamus*; (v.) Reddish clay, 1 ft., with *Hippopotamus* and *Cervus*; (vi.) Unfossiliferous yellow plastic clay, 2 ft. In one place a human bone was found at about the base of layer iii.; and layer v. also yielded the first evidence of extinct carnivorous animals discovered in Malta—a portion of mandible with teeth of a bear, and one tooth of a canine quadruped as large as a wolf.

The discovery of these carnivora in the Maltese caverns is of great interest, because gnawed bones have already been noticed; but it is probable that still others remain to be found, notably the lion and hyæna. The jaw of the bear, moreover, cannot be specifically determined—cannot be definitely assigned either to any extinct animal, to the brown bear, the grisly, or to those of Northern Africa; can only be distinguished with certainty from our cave bear. As for the canine, a single tooth is insufficient to prove whether or not it belongs to a domestic dog.

Still more interesting is the discovery of a great number of bones of a small deer, mostly identified with the diminutive race of the common stag met with in Northern Africa and known as the Barbary Deer. The Fallow Deer may also have been present, but there are no characteristic fragments. One superficial layer in the Har Dalam cave consisted almost entirely of these remains in stalagmite, belonging to animals in all stages of growth, perhaps even from the unborn fœtus onwards. The adults vary much in size, but the largest complete antler measures only about 2 feet in length.

It is worthy of notice, in reference to Dr. Pohlig's recent memoir on the Cave of Carini, that the same form of dwarf deer is also now recognised in Sicily. Dr. Pohlig, however, gives a new sub-specific name to the animal, terming it *Cervus (elaphus) sicilia*; and his nomenclature for the associated species is not altogether such as will commend itself to many zoologists.

The chief advance made by Dr. Pohlig, indeed, consists in his adding to the known Pleistocene fauna of the Sicilian area this small deer and the dwarf elephant commonly known as *Elephas melitensis*. Of the latter, even finer specimens have been discovered at Carini than those obtained from Malta; and Dr. Pohlig is now able to

describe the whole skull for the first time. As the result, he is more than ever convinced of the correctness of his opinion, expressed some years ago, that all the dwarf Maltese elephants described by Falconer, Busk, and Adams are merely a stunted race of the typically European *Elephas antiquus*. He even goes further, and concludes that the latter elephant wandered in Pleistocene times as far south as India, being represented in the Narbada Valley by the so-called *E. namadicus*.

Besides the bones of animals in the new Sicilian cave, which are now described as including remains of such familiar European species as *Bos primigenius* and *Bison priscus*, there are also some traces of man, in the form of rude pottery and stone implements. There is, however, no very clear evidence as yet to indicate man's relationship to the extinct fauna; and we refrain from quoting Dr. Pohlig's table of the succession of episodes supposed to be proved by the series of deposits he has examined. All these new facts will some day be of great service when the time for broad generalisations as to the recent changes of land and sea in the Mediterranean area is at hand; at present it seems futile to base speculations on isolated phenomena.

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SOME NEW BOOKS.

AUSTRALASIA. Vol. i., Australia and New Zealand. Stanford's Compendium of Geography and Travel (new issue). By Alfred Russel Wallace, LL.D., D.C.L. 8vo. Pp. xvi., 505, maps, and illustrations. London: Stanford, 1893. Price 15 shillings.

"In the present volume an attempt has been made to give a compact description of our great Australian Colonies, which should be useful to intending visitors or emigrants; and which will also be interesting to the general reader who may wish to become acquainted with the natural features and social condition of the Britain of the Southern Hemisphere." With the above words the author prefaces his volume, noting also that he has treated of the history and geology, the physical characteristics, customs, languages, and probable origin of the aboriginal inhabitants. Further information is given as to the history of Australian exploration and the special characteristics and productions of the several colonies as well as their more important industries. Prefixed to the general work is a list of works used in preparing the volume, which, though arranged without any regard for alphabetical order or date, will prove useful to the reader if he wishes to pursue his subject further, for the book is essentially one of reference, although readable enough in itself.

Dr. Wallace opens with the general subject, dealing with the definition and nomenclature of Australasia, showing how the area covered by the word has gradually been restricted by geographers until, at present, it is held to comprise only Australia and New Zealand, together with the large islands as far north as New Guinea and the New Hebrides. He, however, holds it to include the islands of the Malay Archipelago as well, and prefers to retain the word Australasia, rather than Oceania, because the former indicates the true relation of the group to the great land-mass of which it forms a southern prolongation.

The geographical and physical features of Australasia are unique, and are thus sketched by Dr. Wallace. In the west, the Malay archipelago, comprising the largest islands in the world (Australia excepted), is unsurpassed for the luxuriance of its vegetation, as well as for the variety and beauty of its forms of animal life. Further to the east are the islands of the Pacific, remarkable for their numbers and their beauty; while to the south is Australia, as unique in its physical features as it is in its singular forms of vegetable and animal life. Still further south lies New Zealand, almost the antipodes of Britain, but possessing a milder climate, and a more varied physiography. Comprised thus between the northern tropic and the 40th degree of south latitude, this land-mass possesses as tropical a climate as Africa, while owing to its being so completely oceanic, and to its wide exterior, it presents diversities of

physical features and of organic life hardly to be found in any of the other divisions of the globe, Asia, perhaps, excepted.

The most striking contrasts of geological structure are exhibited by the coral islands of the Pacific, the active volcanoes of the Malay Archipelago, and the ancient rocks of New Zealand and Tasmania. The most opposite aspects of vegetation are presented by the luxuriant forests of Borneo and New Guinea and the waterless plains of Central Australia. In the Sunda Islands we have an abundance of all the higher and larger forms of mammalia; while farther to the east, in Australia and the Pacific Islands, the absence of all the higher mammals is so marked as to distinguish these countries from every other part of the world. When the land-surface is so completely broken up into islands we cannot expect to find any of the more prominent geographical features which characterise large continents. There are no great lakes, rivers, or mountain ranges. The only land-area capable of supporting a great river is exceptionally arid, yet the Murray of Eastern Australia will rank with the largest European rivers, its basin having an area about equal to that of the Dnieper. Mountains are numerous, and are much higher in the islands than in Australia itself. In such remote localities as Sumatra, Borneo, the Sandwich Islands, and New Zealand, there are mountains which just fall short of 14,000 feet. In New Guinea they probably exceed this altitude, if, as reported, the central range situated close to the equator is snow-covered; while in Australia itself the most elevated point is little more than half as high.

In studying the natural history of Australasia an accurate knowledge of the depths of the waters intervening between the several islands is of considerable importance, and Dr. Wallace provides some details which show that the depth of the seas is greatest near the larger land-masses in this region as in most others. For instance, close to the New Hebrides the soundings record 16,900 feet; between Sydney and New Zealand, 15,600 feet; and a little to the south-east of New Guinea, 14,700 feet. There is a comparatively shallow sea round the coasts of Australia itself, which gradually deepens, till at some 300 to 500 miles on the east, south, and west, the depth of 15,000 feet is attained. The sea connecting Australia with New Guinea and the Moluccas is rather shallow, with intervening basins of great depth. In the Banda Sea the line records 12,000 feet, in the Celebes and Sooloo Seas over 15,000 feet, and in the China Sea, west of Luzon, is a depth of 12,600 feet. Further westward the sea shallows abruptly, so that Borneo, Java, and Sumatra are connected with each other, and with the Malay and Siamese peninsulas, by a submarine bank rarely exceeding 200 or 300 feet in depth.

There are some five or six races of mankind in the area: Malays, Papuans, Australians, Polynesians, Tasmanians (now extinct), and the Negritos. With regard to some of these Polynesians, Dr. Wallace remarks that those inhabiting Samoa and the Marquesas are in no respects inferior to the average European, either in their complexion, physical beauty, or nobility of expression; and he laments that these higher tribes are all disappearing under the fatal contact of our much-vaunted civilisation. To this we venture to add our own regrets on the inevitable extermination of the fine race of Maories in New Zealand.

In the Zoology and Botany, Australia is characterised by possessing a number of peculiar forms, as well as by the absence of many which are common in almost every other part of the globe. The

mammalia are almost exclusively marsupial, the only other representatives of the group now living being the opossums of America. Of the birds, there are no familiar types, as vultures, pheasants, and woodpeckers, but such peculiar forms as honeysuckers, paradise birds, lyre birds, and cassowaries. The snakes and lizards, though numerous, are also singular.

In the plants the same peculiarities are to be seen. The Malayan flora is a special development of that which prevails from the Himalaya to the Malay Peninsula and South China. Further east, this flora intermingles with that of Australia and Polynesia. The Australian flora is highly peculiar, and very rich in species; while that of New Zealand is poor but very isolated. Special sketches of both faunas and floras are given in connection with each colony.

The geological relations of this portion of the world are undoubtedly with Asia. Dr. Wallace thinks that the exceedingly shallow seas connecting the islands of the northern area show that not only Java and Borneo, but even the Philippines formed a south-eastern extension of the Asiatic Continent in a comparatively recent period. The vegetable and animal life shows this still more clearly. But between this mass and that of New Guinea and Australia, we pass over deep seas and find ourselves among a set of animals for the most part totally unlike those of the Asiatic Continent, or any other part of the globe. The resemblances of these southern life-forms to the fauna of Europe during the Secondary period are very striking, and lead Dr. Wallace to say that it is generally believed that the countries they now inhabit have been almost completely isolated from other land-masses since the Oolitic period. The evidence derived from a study of New Guinea, the Moluccas, and the islands as far as Lombok in the north, and Tasmania in the south, goes to prove that Australia was formerly more extensive than it is at present. That this is clear as regards its eastern seaboard is proved by the Great Barrier Reef, whose coral walls still indicate the former limits of the coast-line in this direction. On the same coast, but further removed from the mainland, are some scattered islands, conspicuous among which is New Caledonia, and this is slowly sinking.

Australia must, therefore, says Dr. Wallace, be regarded as an ancient continent of the Secondary or early Tertiary period now gradually diminishing, and this phenomenon of subsidence is displayed in New Caledonia and in some other islands of the South Pacific Ocean. Dr. Wallace is careful to point out that it is now recognised that coral islands do not prove subsidence wherever they occur, but may, and frequently do, indicate areas of elevation at a recent period.

Passing now to the special chapters of the book, we find the present volume confined to two great divisions, (1) Australia, including Tasmania, and (2) the New Zealand group. Malaysia, Melanesia, Polynesia, and Micronesia will form the subject of Dr. Wallace's second volume.

Australia itself, as a whole, is first dealt with, subsequent chapters treating in turn of New South Wales, Victoria, South Australia, West Australia, Queensland, and Tasmania.

With regard to Australia as a whole, Dr. Wallace calls attention to the simple conformation of the land-mass, which rises generally from the coasts into elevated uplands in the interior. But the assumption that Australia forms a vast table-land, with elevated borders sloping towards the interior, must be taken with considerable qualification. The Australian highlands form no connected whole,

but are everywhere intersected by depressions, and a subsidence of 2,000 feet would convert the whole continent into a group of islands; though this, of course, is true of most other continents. A further peculiarity of these uplands is their distribution mainly along the coast, and skirting the interior where no extensive mountain-ranges have hitherto been discovered. The forbidding and desolate plains of the interior of Central and Western Australia are described, and attention is called to their peculiar vegetation (the "Mallee scrub" and the "Mulga scrub") and the extraordinary deficiency of water.

The river-system of the eastern half of the continent is compared with those of the Old and New worlds, and notes are given as to the excessive irregularities of droughts and floods. In the central and western portions streams appear in the wet seasons, but after a short time disappear in the sands; hence, while one explorer has found a total absence of water and herbaceous vegetation, another, arriving in the same region after one of the rare rainy periods, has been delighted with the running streams, luxuriant herbage, and abundance of animal life.

The climate is much less variable than might be supposed. It may be described as hot and dry, and on the whole healthy. The mean temperature at Melbourne is 58° Fahr.; at Sydney it is about 63°; at Adelaide it is slightly higher, while at Perth it is about the same. The extremes of heat are reached when the hot winds blow from the interior, the highest point yet recorded being the bursting of Captain Sturt's thermometer at 127° in the shade. The same traveller found a mean temperature of 100° Fahr. for three months on one occasion. The rainfall is heaviest at Sydney (50 inches), diminishing as one goes inland, while in the south at Melbourne it is 25 inches, and in Western Australia about 30 inches.

The chapters on the Botany, Zoology, and Geology, as one might expect from Dr. Wallace, are full and interesting. The geological map presents a mass of detail, and is generally effective, but one would like to have seen a more modern and precise word than "Trap" used, and the "Crystalline or Metamorphic" rocks not put down as Primary or Palæozoic. We fancy also that it will be news for some geologists to find Oligocene classed as Lower Tertiary, and Volcanic rocks under the head "Igneous or Plutonic."

Each division of Australia is treated in similar detailed fashion, but want of space forbids us from giving a general sketch of each.

New Zealand occupies the last 70 pages of the book, and receives a careful description, similar to that given to Australia. Dr. Wallace, however, is not quite correct in saying that the secluded and romantic waters of lake Taupo are furrowed only by the canoes of the natives, for there is an English settlement there, and we believe the ardent sportsman may now be conveyed by the more expeditious, if less picturesque, steam launch. We also rather doubt the magnificent spectacle of six geysers playing at once, as shown in the somewhat wooden cut of a view on the river Waikato (p. 418). A little contempt, too, seems to be bestowed on the great extinct birds in the sentence—No birds or reptiles have been found except such as are allied to forms still living on the island. But Dr. Wallace refers more kindly to these important relics of the past on p. 446, and our readers have been kept well up-to-date as to recent discoveries and observations on moas in the pages of this Journal.

Taken as a whole, the book is full of interest, is generally well

illustrated, both by woodcuts and maps, and will prove an exceedingly valuable book of reference. Some few slips are found here and there, and some omissions of information to be found in the most recent publications; but Dr. Wallace may well be excused, for in the compilation of books such as these much time is occupied, and no one regrets more than the authors the impossibility of putting in more facts.

The Zambesi Basin and Nyassaland. By Dan J. Rankin. 8vo. Pp. vii., 277, with 3 maps and 10 full-page illustrations. London: William Blackwood and Sons, 1893.

We must confess to considerable disappointment with this book, for we had expected much and have got but little. The author is known to be a man of considerable literary ability and a first-rate Arabic scholar. He has spent much time in East Africa, and knows the district and its natives well, while his work in connection with the opening of the mouth of the Chindi to trade has had an important influence on the commercial prospects of the whole of the Zambesi basin. Nevertheless, there is very little of permanent value in the book. The flora of the district is barely mentioned; the only information about the fauna consists of a few accounts of hippopotamus hunting, and the inevitable, but amusing, crocodile tragedies, which play in Africa the part of the "grizzly" in the stories of our imaginative Western cousins. This is the more unfortunate as the author seems to have met with some very queer beasts, such as the intrusive scorpions, which were so numerous in a steamer's saloon that they had to be dusted off; then there are the worms, which seem to thrive in the iron plates of the same steamer, and a brittle hippopotamus, both sides of whose head were smashed away by a blow on the jaw from only a 12-bore bullet. The book includes more than its title would lead one to expect, as one chapter is devoted to Mombasa, which is certainly far beyond the limits of either the Zambesi Basin and Nyassaland; the remarks on Mombasa, moreover, are disappointing, and as there is no fresh information in the chapter, we must conclude that the author's researches into the history of the early settlements of the Arabs have not been rewarded by new discoveries. This is much to be regretted, considering the anthropological importance of the subject. The author also gives a sketch of the scheme and objects of the International Flotilla Company, which hopes to establish communication from the mouth of the Zambesi to Cairo by the Nile, the great lakes, and the Shiré.

The book is extremely well printed and got up, many of the stories are amusing, and we cannot resist quoting one which illustrates the cuteness of mission companies when they do take to business. The author and two other Europeans once, at the extreme peril of their lives, saved a mission station by running the gauntlet down the Shiré, capturing and repairing the mission steamer which had been seized by the natives, and then working it back to Katunga. Apparently the only thanks they received for this plucky performance was *a bill for their passage*.

Elementary Palæontology. By Henry Woods, B.A., F.G.S. Crown 8vo. Pp. viii., 222. Cambridge Natural Science Manuals. Biological Science. General Editor, A. E. Shipley, M.A. Cambridge: C. J. Clay & Sons, 1893. Price 6s.

Our chief quarrel with this little book is with its title. If, instead of naming it "Elementary Palæontology," the author had called it a

Rule of Thumb Guide to Invertebrate Zoology for the use of Examination Candidates in Geology, we should have had nothing to say against it. All that those gentlemen want is to be able to spot their fossils with a minimum of trouble and a maximum [?] of certainty; they reck naught of morphological niceties so long as they can "pip the examiner." To this goal Mr. Woods will prove an excellent guide, and that his goal is no nobler one is probably deplored as much by himself as it is by us. As to our own ideal, the curious—if such there be—will find it in NATURAL SCIENCE, vol. ii., p. 307.

We would willingly leave the book here, but as it is the first of a series issued under the auspices of no less a body than Cambridge University, it is only due to the Editor and his co-labourers that we should treat it with some seriousness. We hope, in the first place, that the other writers of elementary manuals will not use quite so many long words, at all events, without explaining them. Here the terms "isotropic silica" (p. 3), "optically biaxial" (p. 4), "siphonostomatous" (p. 10), "phylogeny" (p. 11), would make the ordinary undergraduate throw the book out of the window before he had finished the introduction. To judge from his definition of the Foraminifera, Mr. Woods has never heard of the student who was brought to a stand by a similar one. When his teacher said, "I suppose you don't know the meaning of *arenaceous*," he proudly replied, "Oh! yes. I have heard of arenaceous *foods*."

There is, as we have implied, far too much Zoology for a book that calls itself a "Palæontology." Under no circumstances can a palæontologist as such, much less a geologist, want to know anything about the nervous system of the Bryozoa or the renal organs of the Mollusca. The compilers of Zoological text-books have a right to prosecute Mr. Woods for poaching on their preserves.

The absence of finished pictures of specimens is a commendable feature, for such only help the lazy reader. Other writers in the series will do well to follow Mr. Woods' example, but it is to be hoped they will steer clear of his occasional habit of using a plural nominative with a single verb, *e.g.*, "The Porifera includes the Sponges." It would also have been an improvement had the name of some typical species, not necessarily the type species, been added to the descriptions of the genera. In the present state of Brachiopod nomenclature, for instance, it will take the student a few hours to determine what the author means by *Strophomena* and *Leptæna* respectively.

It is a pleasure to find the Pteropods at last removed from the Cephalopods to the Gastropods; but the account of the Cephalopods is no great advance on that of previous text-books. It is all very well for a teacher to be conservative; but there does come a time when one must either "mend or end." "The species of *Ammonites* . . . . differ so much from one another that they are now regarded as representing many distinct genera. . . . . But in an elementary work like the present it will be convenient to retain the old genus *Ammonites*." Long may the good old genus *Ammonites* remain—*in our text-books*! Seriously, what would Mr. Woods or anyone else think, if a writer were to substitute the word *Enerinus* for *Ammonites* in the above quotation? Anyhow, it is worse than absurd, after disposing of the various Families, not merely Genera, of Ammonitinæ in this manner, to devote a whole page to descriptions of the uncoiled Cretaceous forms, which are not genera at all.

Ammonites is described as "possessing two pairs of gills, two

pairs of auricles, and two pairs of kidneys," while other details of its anatomy are given. Of course, if this is the case, the author is perfectly justified in retaining it in an Order Tetrabranchiata, although in opposition to the views of recent writers. It is, however, to be hoped that the specimen from which these details have been determined will soon be adequately figured and exhibited before some learned society by preference, the Royal.

On the whole, the definitions and descriptions are fairly accurate and moderately clear, while the bibliography at the end should prove useful to English students; but is it not stretching a point to describe the calyx of *Apiocrinus* as "pear-shaped," when its shape really resembles that of a piece used in the game of draughts? Still, if there are slips of this kind, and we must admit that we have counted not a few, it is only because the author has attempted an impossible task. It is far more difficult to compile an accurate and intelligible elementary manual than it is to write an elaborate original monograph. Certainly, no one who has not done the latter should ever be encouraged to attempt the former.

F. A. B.

TEXT-BOOK OF GEOLOGY. By Sir Archibald Geikie, D.Sc., LL.D., F.R.S., etc. Third edition. Pp. 1,147. London: Macmillan & Co., 1893. Price 28s.

EIGHT years have elapsed since the second edition of this book was published, and the work accomplished during the interval has led to an increase of 155 pages in the volume. Nevertheless, its actual bulk is not perceptibly increased—the paper is excellent, and the illustrations, of which there are thirty-two more than in the last edition, are admirably printed. The author maintains the moderate uniformitarian doctrines which he has hitherto professed, remarking "that the few centuries, wherein man has been observing nature, form much too brief an interval by which to measure the intensity of geological action in all past time."

Throughout, the work has been revised where needful, and additional references are given to the latest sources of detailed information. On all matters relating to rocks and rock-structure, to the work of modern agents, and the appearances of the rocks in the field, the definitions and descriptions are most lucid. In the matter of palæontological succession and of the value of organic remains in correlating strata at a distance, the views of the author are not in harmony with those of advanced zonalists. If "strict contemporaneity cannot be asserted of any strata merely on the ground of similarity or identity of fossils," yet practical contemporaneity may in many cases be justly maintained, especially when a similar sequence of "zones" is found to characterise areas widely separated. Zonal subdivision and attempts at very minute correlation may, however, be carried to an extreme degree; and further research may justify the cautious attitude of the author, who, in comparing the rocks of distant countries, remarks, "all that we can safely affirm regarding them is, that those containing the same or a representative assemblage of marine organic remains belong to the same epoch in the history of biological progress in each area."

In the portions of the book relating to Stratigraphical Geology, large additions are made to the sections dealing with the pre-Cambrian and older Palæozoic rocks, and other sections are duly brought up to date by the insertion of new facts and further references. The book, however, is so well known, that we need say nothing more, except to

congratulate the author on the completion of his task—one which, considering its comprehensive character, must indeed have proved an immense labour; but which, for this very reason, is a signal service to Science.

TABLES FOR THE DETERMINATION OF THE ROCK-FORMING MINERALS. Compiled by Professor F. Lœwinson-Lessing, translated from the Russian by J. W. Gregory, B.Sc., F.G.S., with a chapter on the Petrological Microscope by Professor Grenville A. J. Cole, M.R.I.A., F.G.S. London: Macmillan & Co., 1893. Price 4s. 6d. net.

MANY investigators who have not had the chance of proper training in a petrographical laboratory, and have had to gain their acquaintance with the methods by books, must have felt their lot a very hard one. Even when they have become fairly well practised in the branch of the microscopic mineralogy of rocks, the determinations in many cases are long and tedious, as classified diagnostic tables were still wanting. So far we had only the systematic description of minerals as they occurred in rocks, but they were classified according to their chemistry, and not by their microscopic characters. The above work admirably fills that gap, and now, after a few lessons in microscopical tactics, a student with a fair knowledge of mineralogy can start as a rock describer.

The original work appeared in Russian, a language to all intents and purposes beyond the knowledge of European and American scientists, and Dr. J. W. Gregory has made all petrographers his debtors for translating Professor Lœwinson-Lessing's book. Dr. Gregory had intended to add a chapter on the petrological microscope, but one of his multifarious duties called him to African exploration, so that his partially-finished chapter was placed in the able hands of Professor Grenville A. J. Cole, who has acted as the contributor of it to this joint work.

In the introduction we have a general outline of the methods to be employed. In the next chapter the main requisites of a petrographical microscope and their uses are described. In part II. of the book we have the tables. Minerals are divided for investigation into opaque, semi-opaque, and transparent, and under each the optical, chemical, and other tests are given. The transparent are divided into colourless and coloured. These groups are subdivided again, according to whether the mineral shows crystal outlines, crystalline grains, or grains in crystalline rocks. Table III. gives a classification of other morphological characters, such as tabular crystals, needles, broad tablets, flakes, filaments, or aggregates, while the presence of colour is again used to group the minerals. In table IV. the determination of the crystalline system under the microscope is treated of. This is followed in table V. by a classification of the rock-forming minerals according to their crystalline system. The book terminates by a table of minerals according to their optical signs.

Only one or two questionable statements attracted our attention, one being, "It is convenient to leave leucite in the prismatic system, which it resembles in form; it appears, however, to be really a monoclinic mineral which has acquired prismatic form by compound mimetic twinning." Most of us hoped that the interminable literature and ingenious contrivances of transcendental crystallographers to explain the optical anomalies of leucite were once for all finished, and that simple temperature strain in an isometric mineral had been accepted as the cause.

In fine, we can only say of the book to all students of petrography, "get it!"

H. J. Johnston-Lavis.

The Journal of Marine Zoology and Microscopy. Vol. i., no. 1. November 1893 [Quarterly]. 8vo. Jersey and London. Price 2s. per annum, post free, or with 14 Microscopical Studies in Marine Zoology, 21s.

It is unnecessary to call attention to the beautiful microscopical preparations sent out by the Jersey Biological Station, they speak for themselves; but it will be interesting to many to learn of a new publication, the chief feature of which will be the illustration and explanation of a yearly series of Messrs. Sinel and Hornell's slides. With this first number are sent out three preparations: *Lucernaria*; *Tomopteris*, the pelagic Annelid; and *Salpa mucronata-democratica*. Two plates accompany the descriptions, and though somewhat rough in execution, are amply sufficient for the purpose intended. Mr. Hornell also describes an albino lobster, 14 inches long, taken in St. Aubin's Bay. A list of other albino organisms is given, and a note follows on the colouration of sponges. Some observations on the Octopus are also added, notably information concerning the strength of this mollusc, its food, and the cleansing process of skin-casting from the suckers. The journal consists of 24 pages, the first 14 being devoted to the special notes just enumerated.

The Out-Door World; or, the Young Collector's Handbook. By W. Furneaux, F.R.G.S. 8vo. Pp. 411, 18 plates, and 549 figures. London: Longmans & Co., 1893. Price 7s. 6d.

Popular books on natural history are so seldom noteworthy for accuracy, that we are glad to draw attention to one that is distinctly above the average. Mr. Furneaux's second title explains the plan adopted, and also accounts for the disproportionate amount of space devoted to certain groups. The volume is divided into three sections: animal life, the vegetable world, and the mineral world; but three hundred pages are devoted to the first section, which is also much the most satisfactory. Butterflies, moths, and birds' eggs are especially well illustrated, the coloured plates being particularly good for so cheap a volume. Certain of the other groups are not so well done, many of the mollusca, for instance, being scarcely recognisable unless one already knows the species. In a second edition, the botanical portion should be revised. The plan has evidently been altered while the proofs were being corrected, for a chapter on grasses is misplaced, and is illustrated by plates out of the numerical order. We would suggest also that in future editions it would be well to draw attention to the importance of noting the exact date, locality, and conditions under which each specimen was obtained. The recording of these facts is an excellent training for the boy, besides adding greatly to the value of his collection.

We learn that the first part of Mr. C. Davies Sherborn's "Index to the Genera and Species of the Foraminifera" will appear about the end of this year. This index, which is brought down to December, 1889, and which is being published by the Smithsonian Institution, occupied the author over four years, and went to press early in 1890. The second and concluding portion may be expected in 1895.

OBITUARY.

HERMANN AUGUST HAGEN.

BORN MAY 30, 1817. DIED NOVEMBER 11, 1893.

SO long as the study of Insects is continued, so long will the memory of Professor H. A. Hagen be venerated. The author of "Bibliotheca Entomologica; Die Litterature über das ganze Gebiet der Entomologie bis zum Jahre 1862," which appeared in 1862–1863, has done so much to lighten the labours of his fellows, that he will always be remembered with gratitude. Hagen was born in Königsberg in 1817, and practised there, as a Doctor of Medicine, until 1867, when he left Germany for America and became assistant to Louis Agassiz. In 1870 he was appointed to the chair of Entomology at Harvard, a professorship he held till his death. Hagen, though specially interested in the Literature of Entomology, found time to write several hundred papers on Insects themselves, among which may be mentioned a Catalogue of *Termitina* in the British Museum, 1858.

AUGUST KARL EDUARD BALDAMUS.

BORN APRIL 18, 1812. DIED OCTOBER 30, 1893.

THIS well-known ornithologist was born at Giersleben, in Saxony. He studied theology at Berlin, and in 1859 was elected Professor at the Köthen Gymnasium. Here he became acquainted with the brothers Naumann, and profited from their ornithological researches. He was pastor at Dielzig in 1849, and passed, in 1858, to the same office at Osternienburg. After his retirement, in 1870, he lived at Coburg. Baldamus collaborated with Blasius in bringing out Naumann's "Naturgeschichte der Vögel Deutschlands," and wrote "Illustriertes Handbuch der Federviehzucht" (ed. 2, 1881), and "Vogelmärchen," 1882. To his initiative was due the foundation of the German Ornithological Society; while from 1849–1858 he published the journal called *Naumannia*.

GEORGE BENNETT.

BORN, 1803. DIED, OCTOBER 1, 1893.

BORN at Plymouth ninety years ago, George Bennett was educated for the medical profession. Visiting Sydney in 1830, he decided to practise there, but did not finally settle until six years later. A

lifelong friend of Richard Owen, Bennett became an enthusiastic naturalist, and provided his friend with rich and valuable material. The first to find Nautilus in the living state, he forwarded his prize to Owen, who wrote upon it his celebrated "Memoir on the Pearly Nautilus," 1832. Bennett wrote numerous papers on Natural History, the most important perhaps being his observation on *Ornithorhynchus* (*Proc. Zool. Soc.*, 1859); he also published "Wanderings in New South Wales," 1834; and "Gatherings of a Naturalist," 1860. On the foundation of the Australian Museum, Bennett was chosen as first Secretary, and the growth and importance of that Institution is largely due to the energy and industry of this eminent naturalist.

NEWS has been received of the death of DR. DIONYS STUR, the eminent geologist and palæobotanist, who passed away at Vienna on October 9. For more than forty years he was connected with the Imperial Geological Survey of Austria, and in 1885 became Director. He retired from the service in April of last year. In 1890 Dr. Stur received the Cothenius Medal from the Leopold Caroline Academy, and was elected a Foreign Member of the Geological Society of London.

WE also have to record the death, on November 13, of MR. WILLIAM DINNING, the well-known Honorary Secretary of the Natural History Society of Northumberland and Durham. He had attained the age of 57, and had been in failing health for some time. Mr. Dinning's chief scientific occupation, pursued in whatever leisure he could obtain in the midst of absorbing business-engagements, was the collection and patient preparation of the vertebrate fossils of the local Carboniferous and Permian formations. He accomplished much in association with the late Messrs. T. P. Barkas and Thomas Atthey, and several of his beautiful drawings illustrate the papers of Messrs. Hancock, Atthey, and Embleton in the *Annals of Natural History* and the Northumberland *Transactions*.

IN our October number (p. 308) we gave a short notice of the late Edward Charlesworth. We were unable at the time to ascertain the dates of his birth and death. We learn (from the *Geological Magazine*) that he was born September 5, 1813, and died July 28, 1893.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. Karl von Dalla Torre has been appointed Extraordinary Professor of Botany at the University of Innsbruck.

Dr. A. Grob has been appointed assistant in the Plant Physiological Institute of the Polytechnic at Zurich.

Dr. Achille Terracciano has retired from his position as Conservator of the Royal Botanical Institute at Rome, and Dr. Oswald Kruch has taken his place.

Professor P. Soraner resigned, on October 1st, the superintendence of the plant physiological research station at Proskan, and Dr. Rudolf Aderhold, formerly first assistant in the similar institute at Geisenheim, has been appointed in his stead, while Dr. F. Krüger takes Dr. Aderhold's place.

The Professorship of Geology in the Bohemian University of Prague has been filled by the election of Dr. Woldrich, of Vienna.

Professor Charles Stewart, of the Royal College of Surgeons, has been elected Fullerian Professor of Physiology at the Royal Institution of Great Britain.

The Owen Memorial Committee has entrusted the execution of the statue of the late Sir Richard Owen to Mr. T. Brock, R.A.

Dr. T. Pleske has succeeded the late Dr. Strauch as Director of the Natural History Museum of the Imperial Academy of Sciences, St. Petersburg.

It is proposed to establish a small Biological Station at Millport, for the study of the Marine Zoology of the Firth of Clyde and West of Scotland generally. A temporary station has already existed for some years at the same place, and during 1891–92 the Government Grants' Committee of the Royal Society of London provided £100 for the investigation of the Clyde Sea area. Much valuable work can be done in the district, and contributions to the building-fund of the station are earnestly solicited by the Committee, who have just issued a circular on the subject. The treasurer of the fund is Robert Gourlay, Esq., Bank of Scotland.

On November 8, the Mayor of Carlisle opened a new Institute of Science, Art, and Literature, built by the town corporation at a cost of £20,000. The Museum contains a large collection of local rocks and fossils, antiquities, and birds, besides other natural history specimens.

The Year-Book of the Bergen Museum for 1892 has just come to hand. In it Dr. J. Brunchorst gives, in German, an account of the biological station in Bergen.

and of the machinery by which the aquaria are supplied with air and addition to the official reports of the curators, the volume contains pa[pers] Appellöf (on "Cephalopoda of the North Sea"), O. Nordgaard (on "[Fi]by A. Echinodermata and Hydroids of Beitstad Fjord"), J. A. Grieg (on "Ophiuroid[s] Greenland"), and J. Brunchorst ("Some Illnesses of Norwegian Timber"), as well as two archæological papers.

Better days are dawning for the South African Museum at Cape Town, which is at present "cribbed, cabined and confined" in one wing of the handsome Public Library building. A new Museum is to be erected in the Government Gardens at a cost of £20,000, and the present rooms handed over to the Library. Let us hope that the working expenses of the Museum will be provided for in a like liberal manner.

In Hong Kong there is a Museum rather of the Curiosity Shop order; but an energetic literary and scientific local society, known as The Odd Volumes, are taking the matter up. They have had a deputation to the Governor, Sir William Robinson, and a lecture from an official of the British Museum. The Governor is willing, but the Press are not. The old objection is in the way—want of money. We can but wish The Odd Volumes the success they deserve.

We learn from the *American Naturalist* that some public-spirited citizens of Chicago have formed a corporation for the purpose of creating and sustaining a Museum, which shall furnish to the public of the city an educational exhibition. It is proposed that the Museum shall be located near to Jackson Park and the University, and for the present the California Building of the World's Fair is to be utilised. Professor F. W. Putnam, the distinguished archæologist, has been appointed managing director. The idea is that Professor Putnam will organise the Museum into departments, placing over each a competent head, who will make the institution a medium for original research, as well as for exhibition, as is the case with all the best museums of the world. It will thus become useful, not only to the general public, but to the University and to the Academy of Sciences. This body of scientific experts, connected with the Museum and University, should stir up the Chicago Academy of Sciences, which has laid dormant so many years.

A Royal Medal was awarded to Professor Harry Marshall Ward at the Annual Meeting of the Royal Society of London on November 30, in recognition of his researches on the life-history of fungi and schizomycetes. The Copley Medal is this year awarded to the distinguished physicist, Sir George Gabriel Stokes, Bart. The only change in the list of officers of the Society consists in the succession of Sir Joseph Lister to the Foreign Secretaryship, vacated by Sir Archibald Geikie. The new members of Council representing Natural Science are Professor A. H. Green, Sir John Kirk, Sir John Lubbock, and Professor Burdon Sanderson.

The Zoological Society of London have printed, in their last issue of *Proceedings*, a valuable and important statement of the exact date of issue of their octavo publications. The dates of their quarto publications have long been listed and printed on the covers of the *Transactions*. The information, which shows the number of the issue, the pages contained in each issue, and the date of delivery from the printers, has been supplied to the Society by Messrs. Taylor and Francis, the Society's printers, and will be of assistance in settling vexed questions of priority. We have often wondered why Societies are so shy of printing the date of issue of their publications side by side with the signatures. The example has been set in certain separate publications for many years, as Bronn's "Nomenclator Paleontologicus," and Godman and Salvin's "Biologia Centrali Americana," where the